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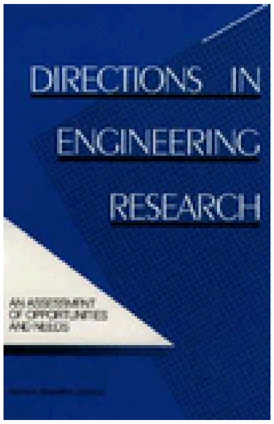

DIRECTIONS IN ENGINEERING RESEARCH

AN ASSESSMENT OF OPPORTUNITIES AND NEEDS

Directions in Engineering Research: An Assessment of Opportunities and Needs

Engineering Research Board, National Research Council

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THE NATIONAL ACADEMIES
Advisers to the Nation on Science, Engineering, and Medicine

Directions in Engineering Research

An Assessment of Opportunities and Needs

Report of the
Engineering Research Board
Commission on Engineering and Technical Systems
National Research Council Washington, D.C.

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NOTICE: The project that is the subject of this report was approved by the Governing Board of the National Research Council, whose members are drawn from the councils of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. The members of the committee responsible for the report were chosen for their special competences and with regard for appropriate balance.

This report has been reviewed by a group other than the authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

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Any group comprising individuals from diverse engineering fields, however knowledgeable they may be in their own areas, would be hard-pressed to ascertain the most important needs facing the whole of engineering without seeking the advice and views of many others throughout the engineering community. Accordingly, the Engineering Research Board gratefully received input of various kinds from a range of sources.

We wish to acknowledge, first, the important contributions made by those who spoke to the board during its early meetings, and whose counsel helped to establish the framework for our study. They are the Hon. George A. Keyworth, then Director of the Office of Science and Technology Policy and Science and Technology Advisor to the President; Congressman George E. Brown, Jr., a member of the House Committee on Science and Technology; from the National Science Foundation: Nam P. Suh, Assistant Director for Engineering, and Carl W. Hall, Deputy Assistant Director for Engineering; from the Department of Energy: James S. Kane, Deputy Director of the Office of Energy Research, and Oscar P. Manley, of the Division of Engineering, Mathematical, and Geosciences; John W. Lyons, Acting Deputy Director of the National Bureau of Standards; from the National Aeronautics and Space Administration: Raymond S. Colladay, Deputy Associate Administrator for Aeronautics and Space Technology, and Paul R. Brockman, Chief of Intergovernmental Affairs (retired); from the Department of Defense, Office of the Undersecretary for Research and Engineering: Raymond F. Siewert, Director of Military Systems Technology, and Leo Young, Director of Research and Laboratory Management; and James Spates, Assistant Director for Research, Department of the Army.

Our confidence in our own deliberations and findings was greatly augmented by the replies we received to a survey sent out to well over 100 professional societies, universities, government and national laboratories, and Presidential Young Investigators. The assistance of the many individuals who participated in preparing those responses is greatly appreciated. We thank in particular the members of the Institute of Electrical and Electronics Engineers who helped to prepare several detailed and highly informative reports relevant to the work of various panels.

We must take this opportunity to express our gratitude to the many individuals who sat on the seven panels formed by the board, and without whose long and painstaking efforts our own work could not have been performed. Finally, we offer our appreciation to the staff and consultants of the Engineering Research Board for their fine and professional support: to Executive Director Ralph D. Cooper, who accomplished the enormous task of organizing and managing the affairs of the panels and the board; to Courtland S. Lewis, for his tireless effort in writing and assembling the board's reports; to Stanley W. Doroff, for his able assistance in collecting, analyzing, and tabulating data for the Board's use; to Robert J. Burger, for his experienced advice on a variety of key issues; to Viviane Scott, for doing a tremendous job as Administrative Assistant and Budget Analyst; and to both Michael Resnick and Janet Crooks, for their dedicated support as Administrative Secretary over the course of the study.

ALLEN E. PUCKETT
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PANEL ON BIOENGINEERING SYSTEMS RESEARCH

The panel acknowledges with gratitude the many individuals who contributed to its deliberations and to the preparation of this report. We appreciate the thoughtful comments relevant to the work of this panel provided by many professional societies, universities, government and national laboratories, and Presidential Young Investigators in response to a request by the Engineering Research Board for supplementary information to assist in carrying out the study. (These institutions and the affiliations of individual respondents are identified in the Appendix.)

The Panel on Bioengineering Systems Research greatly appreciates the time and effort that many people gave to making its work more productive. In particular, we wish to thank those who gave invited presentations on aspects of bioengineering research. From the National Institutes of Health: Allan Berson, Devices and Technology Branch, Division of Heart and Vascular Diseases; Robert Dedrick, Chief, Chemical Engineering Section, Biomedical Engineering and Instrumentation Branch, Division of Research Services; Rosalee Dunn, Senior Staff Scientist, Devices and Technology Branch; Murray Eden, Chief, Biomedical Engineering and Instrumentation Branch, Division of Research Services; W. J. Holliman, Chief, Research Documentation Section, Statistics and Analysis Branch, Division of Research Branch; and John T. Watson, Chief, Devices and Technology Branch, National Heart, Lung, and Blood Institute. From the Veterans Administration: Frank Coombs, Manager of the Rehabilitation Program. From the National Science Foundation: William Freedman, Program Director, Bioengineering and Research for the Handicapped; Carl Hall, Deputy Assistant Director for Engineering; and Marshall M. Lih, Director of the Division of Chemical and Process Engineering in the Engineering Directorate. From the National Bureau of Standards: Jaromir J. Ulbrecht, Chemical Process Metrology Division. And from the National Research Council: David Policansky, of the Committee on Biotechnology Applied to Naval Needs, Board on Basic Biology, Commission of Life Sciences. The panel thanks everyone who participated in our meetings and contributed to this important task of the Engineering Research Board.

Finally, we also thank the consultants/writers: Theodore Esders of the Eastman Kodak Company, and Jeffrey Cohn, Stanley

Doroff, and Courtland Lewis for their excellent support and assistance in organizing the panel's activities and preparing its report. We are also grateful to the staff of the Engineering Research Board: Ralph D. Cooper, Executive Director; Viviane Scott, Administrative Assistant and Budget Analyst; Janet Crooks, Administrative Secretary; and Michael Resnick, Administrative Secretary, for their expert assistance during the course of this study.

JOHN A. QUINN,
CHAIRMAN
LEO J. THOMAS, JR.,
VICE-CHAIRMAN

PANEL ON CONSTRUCTION AND STRUCTURAL DESIGN SYSTEMS RESEARCH

The panel acknowledges with gratitude the many individuals who contributed to its deliberations and to the preparation of this report. We appreciate the thoughtful comments relevant to the work of this panel provided by many professional societies, universities, government and national laboratories, and Presidential Young Investigators in response to a request by the Engineering Research Board for supplementary information to assist in carrying out the study. (These institutions and the affiliations of individuals are identified in the Appendix.)

The Panel on Construction and Structural Design Systems Research received assistance from too many people to thank individually. We appreciate the time and effort each person gave to making the panel's work more productive. However, the panel thanks, in particular, John P. Eberhard, Executive Director of the Building Research Board of the National Research Council, for a very instructive presentation on the engineering research related activities of his board. In addition, the panel is very grateful for assistance rendered in assessing various aspects of the federal construction infrastructure by Richard N. Wright, Director of the Center for Building Technology at the National Bureau of Standards; Noel J. Raufaste, Executive Secretary, International Technology Council, Building Research Board; and Henry A. Borger, Executive Secretary, Federal Construction Council, Building Research Board. The panel also thanks all those who participated in our meetings and contributed to this important task of the Engineering Research Board.

Finally, we also thank consultants Stanley Doroff and Court-land Lewis for their excellent support and assistance in organizing the panel's activities and preparing its report. We are also grateful to the staff of the Engineering Research Board: Ralph D. Cooper, Executive Director; Viviane Scott, Administrative Assistant and Budget Analyst; Janet Crooks, Administrative Secretary; and Michael Resnick, Administrative Secretary, for their expert assistance during the course of this study.

RICHARD H. GALLAGHER,

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VICE-CHAIRMAN

PANEL ON ENERGY, MINERAL, AND ENVIRONMENTAL SYSTEMS RESEARCH

The panel greatly appreciates the thoughtful contributions made by the individuals and organizations who provided background material for this report. In particular, we thank Dennis Miller, Executive Director of the Energy Engineering Board of the National Research Council, who provided the panel with information on the board's activities and on needed research in the energy area; Stephen Parker, Executive Director of the Water Science and Technology Board of the National Research Council, who discussed the activities of this Board in evaluating water resource problems; and Myron Uman, Executive Director of the Environmental Studies Board of the National Research Council, who pointed out particular areas in which environmental problems could benefit from additional research.

The panel is especially grateful to the many professional organizations, university schools and departments of engineering, and faculty recipients of Presidential Young Investigator Awards who provided detailed discussions of research needs in energy, water, and mineral resources development and on the control of environmental pollution. Their comments and recommendations on research policy questions as well as needed areas of research in their particular fields of expertise were most useful to the panel in arriving at its own conclusions and recommendations. (Their responses are summarized in the Appendix.) It was obvious from the materials received that many individuals put a great deal of time and thought into their responses. For this we are most thankful.

Finally, we wish to express our appreciation to consultant/writer Courtland Lewis for his outstanding support and assistance in assembling and organizing material for this report, and to the staff members of the Engineering Research Board—especially Ralph D. Cooper, Executive Director; Viviane Scott, Administrative Assistant and Budget Analyst; Janet J. Crooks, Administrative Secretary; and Michael Resnick, Administrative Secretary—for the expert assistance they provided during this study.

PERRY L. MCCARTY,

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PANEL ON INFORMATION, COMMUNICATIONS, COMPUTATION, AND CONTROL SYSTEMS RESEARCH

The panel gratefully acknowledges the many individuals who contributed to its deliberations and to the preparation of this report. We appreciate the thoughtful comments provided by many professional societies, universities, government and national laboratories, and Presidential Young Investigators to the survey conducted by the Engineering Research Board that were relevant to the work of this panel. (See the Appendix.) In particular, we thank the members of the Institute of Electrical and Electronics Engineers' Panel on Research Priorities in Information and Computing Technology, chaired by Samuel Fuller, for their extremely insightful, incisive, and useful input. We are also grateful to Richard Marsten, of the National Research Council's Board on Telecommunications and Computer Applications, for his presentation to the panel at its first meeting; and to Ivar Stakgold, of the University of Delaware, for his attendance and wise counsel at our meetings.

We wish to express our appreciation to the staff of the Engineering Research Board, especially Executive Director Ralph D. Cooper, for their excellent professional support. Finally, we thank Courtland Lewis for his considerable help in the writing and assembly of the report.

THOMAS E. EVERHART,

CHAIRMAN

JOHN A. ARMSTRONG,

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PANEL ON MANUFACTURING SYSTEMS RESEARCH

The panel acknowledges with gratitude the helpful written information it received during the course of its work, which influenced its discussions and the preparation of this report. These thoughtful comments were provided by many professional societies, universities, government and national laboratories, and Presidential Young Investigators in response to a request by the Engineering Research Board for supplementary information to assist in carrying out the study. (The institutions and organizations that responded or with whom individual respondents were affiliated are identified in the Appendix.)

In addition, the Panel on Manufacturing Systems Research received assistance from many other individuals and we appreciate very much the time and effort each gave to making the panel's work more productive. We thank in particular the following individuals, who either gave invited presentations on aspects of manufacturing systems research to the panel or provided expert counsel as participants in its meetings: George H. Kuper, Executive Director, Manufacturing Studies Board, National Research Council; Robert Elwood, Project Director of the Navy's Project for the Rapid Acquisition of Manufactured Parts, Naval Supply Systems Command, U.S. Department of the Navy; Charles E. Feltner, Manager, Manufacturing Systems and Operations Engineering Department, and Ronald G. Hurley, Director of Manufacturing Systems Department Analysis Department, Ford Motor Company; Jacob T. Schwartz, Professor of Mathematics, Courant Institute of Mathematical Sciences, New York University; Paul Brockman, Assistant Director of Civil Affairs, National Aeronautics and Space Administration; Dennis H. Swyt, Deputy Director, Center for Manufacturing Engineering, National Bureau of Standards; and Nam P. Suh, Assistant Director for Engineering, Carl W. Hall, Deputy Assistant Director for Engineering, and William Spurgeon, Program Director of the Manufacturing Systems Program, all of the National Science Foundation.

We gratefully acknowledge the expert professional assistance provided by Ralph D. Cooper, Viviane Scott, Michael Resnick, and Janet J. Crooks; and we thank consultants Courtland S. Lewis and Stanley Doroff for the assistance they provided during the course of this study.

DAVID A. HODGES,
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PANEL ON MATERIALS SYSTEMS RESEARCH

This report was strengthened by the helpful comments and contributions of many individuals and organizations representing the materials engineering research community. The panel thanks in particular those individuals who gave invited presentations on aspects of materials research: Louis C. Ianniello, of the Department of Energy; Robert Reynik, of the National Science Foundation; Benjamin Wilcox, of the Defense Advanced Research Projects Agency; and Klaus Zwilsky, of the National Materials Advisory Board. We are also grateful for the participation and expert counsel of those who joined us in panel meetings, including Bernard Budiansky of Harvard University, Lyle Schwartz of the National Bureau of Standards, and both Tapan Mukerjee and Robert Reynik of the National Science Foundation. In addition, the assistance of Darrell Reneker, of the Office of Science and Technology Policy, Ward Winer, of the Georgia Institute of Technology, and Jay Benziger, of Princeton University, is much appreciated.

The input provided by the many individuals and organizations—among them engineering schools, professional societies, federal and national laboratories, and Presidential Young Investigators—responding to the Engineering Research Board's survey of the research community was an invaluable aid in the preparation of the report. The institutions and the affiliations of the respondents are identified in the Appendix. In particular, we are most grateful to the members of an Institute of Electrical and Electronics Engineers task force, chaired by Professor Gregory E. Stillman, which prepared "An IEEE Opinion on Research Needs in Electronic Materials" for the panel. That document was extremely useful in the panel's deliberations.

Finally, we wish to express our appreciation to consultant/writer Courtland Lewis for his outstanding support and assistance in assembling and organizing material for this report, and to the staff members of the Engineering Research Board, especially Ralph D. Cooper, Executive Director; Viviane Scott, Administrative Assistant and Budget Analyst; Janet J. Crooks, Administrative Secretary; and Michael Resnick, Administrative Secretary, for their expert assistance during this study.

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PANEL ON TRANSPORTATION SYSTEMS RESEARCH

This report was strengthened by the helpful comments and contributions of many individuals and organizations representing the transportation engineering research community. The panel thanks in particular those individuals who gave invited presentations on aspects of transportation research: Howard J. Dugoff, Science and Technology Advisor for the U.S. Department of Transportation; Raymond Siewert and Jack Bachkosky of the Office of the Undersecretary of Defense for Research and Engineering; Allen J. Dowd of the Joint Military Traffic Management Command; Robert C. Waters of the George Washington University; William E. Roper, Director of Engineering Research Programs for the Army Corps of Engineers; Jack W. Boller, Executive Director of the National Research Council's Marine Board; and A. J. Evans of the National Research Council's Aeronautics and Space Engineering Board. We are also grateful for the participation and expert counsel of those who joined us in panel meetings, including Gifford Albright of the National Science Foundation, Severino L. Koh of the U.S. Department of Energy, Paul Brockman of the National Aeronautics and Space Administration, James Spates of the U.S. Department of the Army, and Robert Elwood of the U.S. Department of the Navy.

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Preface

ENGINEERING RESEARCH BOARD

Over the past 40 years the United States has emerged as the preeminent nation in scientific research. We also enjoy one of the most entrepreneurial environments in the world. Despite these advantages, however, our national competitiveness has eroded in a number of major industries. Although many factors contribute to that eroding competitiveness, it is now widely recognized that a major factor is the lack of adequate attention to engineering research. This is a key link in the chain that includes science research, development, design, production, and service.

Today scientific and technical knowledge, along with investment capital, flow freely across national boundaries. With only 6 percent of the world's population, the United States cannot assume that its leadership in any field is secure. U.S. citizens are legitimately concerned with how the country can best maintain its international competitiveness, improve the quality of life for its people, and provide reasonably for their defense. All three of these goals can be met only by reinvesting a portion of the nation's resources to improve the prospects for the future. Because resources are finite, a thoughtful, balanced analysis of the current situation and a blueprint for action are essential. To that end, the Engineering Research Board of the National Research Council was formed with these goals:

- to study the condition of engineering research in the United States and to identify areas requiring priority attention;
- to evaluate the role of engineering research in contributing to national objectives; and
- to make recommendations for changes in public policy that would advance these objectives.

This report summarizes the results of that study. Contributions from hundreds of industrial, academic, and government experts were analyzed in the course of preparing the report. Interdisciplinary panels made up of leading engineering researchers from academia and industry examined seven carefully chosen areas of engineering systems. Thus, this report represents a major effort by the engineering research community to project its future and clarify its role in meeting major national goals. It is our hope that those concerned with industrial competitiveness, defense, and the public welfare will find it useful as they map the future of the nation.

ALLEN E. PUCKETT,
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PANEL ON BIOENGINEERING SYSTEMS RESEARCH

The Engineering Research Board was created by the National Research Council in response to a request from the National Science Foundation (NSF) for a study of engineering research. In particular, the NSF sought advice on important and/or emerging areas of engineering research.

Early in its deliberations, the Engineering Research Board identified seven cross-cutting areas of research to serve as focal points for the study. Separate panels were appointed, each chaired by a board member, to study those research areas. As one of the seven, the Panel on Bioengineering Research was specifically asked by the Engineering Research Board to:

- describe the scope and character of bioengineering research;
- identify important or emerging areas of bioengineering research;
- study policy issues affecting the federal government's support of bioengineering research;
- assess the adequacy of government and industry funding for academic research programs;
- evaluate the adequacy of mechanisms for attracting young people to bioengineering and related areas; and
- recommend actions that will improve the overall health of bioengineering research in the United States.

The eight members of the panel were equally divided between those with backgrounds in biomedical engineering and those who are biochemical engineers (see the introduction for definitions of these fields). This division represents the panel's view that the two components of modern bioengineering both have important and distinct research needs and are of vital importance to the nation.

The panel was well aware that both components of bioengineering are subjects of public controversy. The social, ethical, and philosophical questions surrounding such topics as genetic engineering, organ transplantation, artificial organs, human reproductive technology, and life-support technologies are profound and sometimes heatedly debated. Nevertheless, the panel chose not to address these issues. Our subject is engineering research per se. Lacking any expertise on these legal, sociological, and even theological matters, we felt we could add nothing of substance to the debate.

In conducting the study, the panel used several data sources. In addition to insights provided by panel members, contributions were sought by the Engineering Research Board from 15 professional engineering societies; 20 federal and national laboratories; 80 Presidential Young Investigators; deans of engineering, medical, and dental schools; and selected key officials of the National Institutes of Health and the NSF. Those institutions and the affiliations of individuals that responded to the survey with comments and information pertinent to the needs of this panel are listed in the Appendix. Recent authoritative studies of the bioengineering field were also examined.

This report is thus a highly focused effort to characterize bioengineering research and the environment in which it is carried out. Along with the other members of the panel, I hope that the report will help to focus the attention of policymakers, federal research agency officials, academic administrators, and the research community on matters of critical importance to this dynamic field.

JOHN A. QUINN,
CHAIRMAN

Contents

Directions in Engineering Research: An Assessment of Opportunities and Needs

EXECUTIVE SUMMARY

Introduction and Background

Engineering research, the application of science in the creation of products and services, is an essential area of technical activity that is seriously undersupported in the United States. This research is essential because all creative technological development in an intensely competitive world rests on it; yet it is undersupported because its central role in the development of productive goods and services is not clearly understood and recognized. This report is an attempt to close the gap in understanding the nature of engineering research and to draw attention to the need for increased support in several key fields.

The Nature of Engineering Research

Engineering can no longer be described only in the context of its traditional disciplines: civil, mechanical, chemical, electrical, and so forth. Although these disciplines still form the core of curricula in engineering education, the frontiers of engineering today concern systems—the interactions among these core disciplines, economics, social values, and the burgeoning of technical and scientific knowledge that is reordering world trade and the strategic balance among nations.

In contrast to science research, which primarily seeks new knowledge about the natural world, engineering research concentrates on the man-made world to expand the knowledge base and to identify and prove the physical principles on which advances in design and production can be based. This requires strong interactions between engineering research and science research, and the boundaries between them are often difficult to discern. Indeed, both require exactly the same types of intellectual activity—basic research aimed at improving our understanding of the underlying phenomena, and applied research aimed at developing the practical implications of the new understanding. In engineering, basic research provides the underlying competence on which applications research is based. For example, the evolution of the modern computer from electron tubes to transistors and then to integrated circuits is the result of engineering research that converted newly understood physical principles into practical working systems. Taken together, engineering and science research are crucial in a world in which competition through technology has assumed a commanding role in the interactions among nations.

Engineering and engineering projects have been an integral part of the human experience since the beginning of civilization. Until quite recently, however, advances in engineering practice were gained by slow and laborious trial-and-error procedures. Then, at about the turn of the last century, modern methods of engineering research firmly based on scientific principles were brought to bear on a wide variety of problems. Engineering knowledge and the technological developments based on it have grown rapidly and continuously ever since. Structures of every kind—residential and commercial buildings, bridges, dams, and tunnels—have become larger, stronger, safer, and easier to build through research into their design and construction. As a result of engineering research in materials, mechanics, electronics, and manufacturing processes, machines efficiently and reliably carry out functions once performed by humans and animals. Modern transportation systems—automobiles, trucks, trains, ships, and aircraft—are outstanding examples of the contributions of engineering research to such technological advances. Conversion technologies to utilize energy sources in their evolution from wood to coal to oil and to nuclear power are based on knowledge provided by engineering research. Research in electrical and electronics engineering have made our telephone, radio, and television systems possible, and have led to today's worldwide communication networks linked by satellite. Modern information and data processing systems are closely related developments.

Thus, engineering research is simultaneously a generator, stimulator, assimilator, integrator, translator, and promoter of new scientific and technical knowledge, all with the primary objective of making the production of goods and the provision of services easier and more efficient and their use and maintenance less costly. The broad scope of interests and activities encompassed by engineering research is illustrated by the following research areas of current opportunity identified in this report:*

- complex system software;
- advanced engineered materials;
- manufacturing systems integration;
- bioreactors;
- construction robotics;
- vehicle/guideway system integration;
- alternative fuel sources;
- low-grade mineral recovery;
- biomedical engineering;
- hazardous material control;
- the mechanics of slowly deteriorating systems;
- computer-aided design of structures;
- manufacturing modeling and simulation;
- and electronic device and packaging technology.

Funding Outlook

Adequate funding, both in terms of amounts and stability, is central to the success of engineering research. Approximately $3.8 billion, about 25 percent of the total federal research budget, was allocated for the support of engineering research in 1985. This rather modest percentage has remained essentially constant for

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* The Engineering Research Board attaches especially high priority to the first three research areas on the list. All 14 areas are briefly discussed in a later section of the executive summary, "Key Research Opportunities and Needs."

almost 20 years, a period during which our nation has experienced a steady decline in productivity and competitiveness. An overwhelmingly large portion (about 95 percent) of the total federal engineering research budget is devoted to applied engineering research, leaving a mere 5 percent to support basic engineering research. Basic engineering research is largely carried out by academic institutions, but with the financial support of the federal government. In recent years, the states and private industry have become increasingly active partners with the federal government and have significantly increased their support for academic engineering research, but federal funding still supports fully 70 percent of the basic science and engineering research now conducted in the United States.

Engineering research depends on a continuity of effort in order to be productive. Thus, fluctuations in funding support that can occur when federal agencies must respond to short-term crises, and the interruptions in continuity that result, can create serious problems for both basic and applied research efforts, whether they are carried out in universities, industry, or federal and national laboratories.

To the extent that the large, multidisciplinary engineering research centers, now being supported by the National Science Foundation (NSF), indicate a trend toward stable funding, they are a timely and welcome development. Two caveats, however, must be recognized. First, the funding made available to the new research centers raises questions about the adequacy of funding support for interdisciplinary research at colleges and universities that do not have such centers. Second, research administrators must strike a balance between research by individuals and the collaborative research of the new engineering research centers. The latter caution introduces the issue of adequate funding for small-scale research projects involving a single investigator and perhaps one or two graduate students. This individual research can be highly effective because it is the ideal scale on which to first explore areas of high-risk engineering research.

On the other hand, history suggests that individual researchers in academia have often been more highly and more frequently rewarded than their colleagues who engaged in collaborative research efforts such as those envisioned in the engineering research center concept. Thus, an important issue for university administrators is developing and maintaining balanced support and promotion incentives among those investigators involved in small-scale, disciplinary, individual research and those participating in large-scale, multidisciplinary, team research.

Human Resources

The second fundamental component of engineering research is people. Much evidence suggests that a long-range problem is developing at the baccalaureate level. The U.S. cohort of persons in the 18-to 20-year-old age group is shrinking. Because no decline in the demand for scientists and engineers in the work force—including those who will be engaged in engineering research—is projected, serious shortages could occur by the end of the century or shortly thereafter. At the graduate level the number of doctoral degrees in engineering granted by American universities seems to be increasing, but the estimated engineering Ph.D. output of 3,400 for 1985 is still substantially less than it was in the late 1960s. Moreover, in Japan, widely acknowledged as one of our strongest international competitors, the ratio of engineering Ph.D. output to total Ph.D. output is almost twice as high as in the United States, although the absolute numbers are significantly lower. In addition, many Japanese earn their engineering Ph.D.s in the United States, providing evidence both of Japan's national commitment to engineering research and of the high quality of engineering education in the United States. The continuation of that quality, however, is uncertain. In many fields the U.S. industrial demand and attractions for baccalaureate engineers are depleting the ranks of our graduate students and threatening the production of well-trained teachers and researchers needed for the future.

Institutional Considerations

The outlook for basic engineering research, especially in academia, is clouded by several factors. First, there is a severe lack of adequate facilities and equipment for both instructional and research purposes. The average age of laboratory equipment in engineering schools is about 25 years, and only 18 percent of it is up to state-of-the-art standards. Fully one-fourth of the equipment is totally obsolete. This problem has been temporarily alleviated in some schools for a few areas of research by sharing facilities and by recent gifts from industry. In addition, a variety of academic restrictions and industrial practices have discouraged the conduct of industry-supported research on campus, so that much needed academic/industrial interaction has been limited on issues like curriculum development, equipment loans, and personnel exchanges. Beneficial modifications of these past policies and practices are already under way, spurred on by the emerging emphasis on large, multidisciplinary research efforts that often require active industrial participation.

Recommendations

The health and vigor of engineering research in the United States is directly affected by the complex interactions among the many factors discussed previously. Thus, in addition to its primary thrust of identifying the engineering research areas of current opportunity, the Engineering Research Board has also made a number of recommendations to strengthen the nation's engineering research enterprise that take these factors into account. Brief presentations of 11 major recommendations of the board follow. The first seven recommendations require government action for their implementation. The next two are addressed to university administrators, and these are followed by one directed to industry and one to the engineering research community at large. These recommendations are discussed more fully later in this chapter.

Recommendation 1: Recognition. Congress and the federal agencies concerned with technology development must recognize the importance of engineering research to the economic health of the nation. In so doing, national patterns of support for research and development should be carefully examined to identify points at which increased federal funding for engineering research would most effectively benefit the overall national research and development (R&D) effort. In particular, serious consideration should be given to an earlier recommendation made by the National Academy of Engineering that the budget of the NSF's Engineering Directorate should be increased from its annual level of $150 million in 1985 to about $400 million by 1990.

Recommendation 2: Stability. The short-term crises encountered by many federal mission agencies frequently involve engineering problems. The engineering research budgets of such agencies are, therefore, especially vulnerable to the demands of the quick response initiatives undertaken to resolve them. Congress and the mission agencies should protect engineering research budgets from such demands. A reasonable and stable floor for the funding of core activities should be part of the agency's research budgets, and project managers should have the flexibility to tailor their resources to provide such a floor.

Recommendation 3: Equipment and Facilities. State and federal legislatures must take steps to encourage gifts of laboratory equipment to engineering schools, for example, by the passage of appropriate tax legislation or the establishment of matching fund programs. Congress should consider an earlier proposal made by the National Academy of Engineering to add a minimum of $30 million per year for the next 5 years to the budget of the NSF's Engineering Directorate for the procurement of research equipment and instrumentation. Government contracting and granting agents should permit depreciation charges as normal operating expenses and allow them to accrue toward renovation and replacement costs of equipment and facilities.

Recommendation 4: Coordination. The Office of Science and Technology Policy should take the lead in strengthening governmental coordinating activities in engineering research, which are needed to assist in setting integrated, national engineering research priorities and in monitoring the progress of engineering research programs.

Recommendation 5: High-Risk, High-Return Research. Managers of agency R&D programs must provide adequate support for high-risk, long-range engineering research with high payoff potentials as a complement to their larger interest in research projects with more immediate and direct applications. Special budget categories might be considered for such work.

Recommendation 6: Single Investigator Projects. The NSF should continue to devote a major share of its engineering research program to small-scale, single investigator projects, in balance with the current interest and activity in multidisciplinary research involving large research centers.

Recommendation 7: Stimulation of Industry Research. Congress and the policymakers of the Executive Branch of the federal government should expand legislative measures and administrative procedures to stimulate much needed increases in engineering research in industry—both research conducted in-house by industry and that conducted in academia with industrial support.

Recommendation 8: New Talent. University administrators with the assistance of government and industrial leaders must devise programs to attract and retain talented young Ph.D.s in academic engineering research and, where appropriate, to enable established senior faculty to develop new expertise in areas more relevant to current needs. The Presidential Young Investigator program and present academic sabbatical leave policies are steps in the right direction, but much more must be done, especially along the lines of providing research initiation funds and selectively reduced teaching loads for highly qualified researchers.

Recommendation 9: Multidisciplinary Research. University administrators must continue to accommodate and encourage multidisciplinary engineering research. Specifically, university policies must support, encourage, and reward successful engineering researchers involved in the use of shared facilities and active collaboration with colleagues in academia as well as in industrial and government laboratories.

Recommendation 10: Industry Support. Industry management at all levels should give greater attention to engineering research and provide more support for it—both in-house and in academia. In-house support should particularly include programs of continuing professional development and education for the engineering research staff, and the encouragement of greater interactions between these researchers and the rest of the engineering research community. Industry support for academic research could include, for example, joining with federal and state agencies in providing matching grants for engineering curriculum development and research initiation, donating laboratory equipment, and exchanging research personnel.

Recommendation 11: Transfer of Research Results. Engineering researchers and practicing engineers must begin to work consciously and vigorously toward a mutual, sympathetic understanding of each other's needs and goals so that the transfer of research results into practical engineering design tools and procedures can be accomplished effectively and efficiently. Enthusiastic collaborative interaction between researchers and practitioners, especially at the interface between engineering research and industrial design, is an important element in the transfer process and must be increased.

Key Research Opportunities and Needs

The Engineering Research Board identified areas of engineering research that, in its judgment, hold the greatest potential for contributing to the nation's economy, security, and social wellbeing. To assist it in this endeavor, the board established panels in seven fields of multidisciplinary engineering research:

1. bioengineering systems;
2. construction and structural design systems;
3. energy, mineral, and environmental systems;
4. information, communications, computation, and control systems;
5. manufacturing systems;
6. materials systems; and
7. transportation systems.

Each panel identified those fields of engineering research that appeared to offer the greatest return on the research investment. The board ultimately selected 14 fields, and brief discussions of them follow. No significance is attached to the order in which they are discussed, except to note that the board assigns especially high priority to the first three areas.

Complex System Software. The cost of producing and applying software is holding back U.S. manufacturers as well as key defense initiatives. The opportunities for advances in this area are enormous. Yet first, additional research is needed on the efficient development of large software systems. Research on compatibility, reuse, and standardization of key software modules is also important. Related research needs include (1) software reliability, testing, and verification; (2) distributed computer systems; (3) productivity aids; and (4) real-time processing of large volumes of data.

Advanced Engineered Materials. Advanced engineered materials, a designation that implies new methods of processing to obtain prespecified materials properties for specific applications, hold great promise for the creation of new products with new standards of performance in virtually every commercial field and military system. There is almost unlimited potential for this new concept of materials design, but research is needed to capitalize on the opportunities that it affords. For example, better understanding of the forces between microparticles can lead to the creation of ceramics with hitherto unattainable strength/temperature characteristics. Knowledge of the factors controlling biocompatibility is needed to produce the biomaterials needed to construct new prosthetic devices and to improve existing ones. Greater knowledge of how materials bind, deform, and rupture is clearly a key factor in satisfying the continuing demand for materials with improved service reliability.

Manufacturing Systems Integration. The integration into a manufacturing system of its human and machine-based components will lead to great improvements in manufacturing efficiency and productivity. Achieving this goal, however, will require major advances in systematic, generic approaches to the design of computer-integrated manufacturing systems. Research must provide the basis for the development of new hardware and software elements that are modular, compatible with other systems, adaptable to new requirements, and user-friendly. More basic research should address expert system approaches for the design of complex manufacturing systems.

Bioreactors. The annual world market for biotechnology products is expected to be about $100 billion by the year 2000, if anticipated new bioprocessing technology is developed and successfully scaled up to meet industrial requirements. This expectation is reflected in the current flurry of related activity in Europe, Japan, and the United States. New techniques are needed for the large-scale culture of plant and animal cells and engineered organisms. Fundamental knowledge of the effects of physical and environmental factors on the biosynthetic pathways within cells is essential to the development of such techniques. In addition, parallel research is needed to develop methods for using various enzymes or cells as catalysts for biosynthesis.

Construction Robotics. At about $200 billion per year, the construction industry is one of the largest segments of the national economy. Yet it is labor-intensive and has a low productivity rate. Humans still perform many lifting and installation operations, and consequently the size of many construction components is currently governed by human physical capacity. To extend present industrial robots and automatic material handling equipment to construction applications will require research on incorporating such new functions as mobility, flexibility, and high payload-to-weight ratios. Further research will be needed to develop the new construction design concepts, materials, and methods that will have to be devised to exploit these robotic capabilities in the construction workplace.

Vehicle/Guideway System Integration. The national transportation system should consist of a network in which all forms of transportation and their interconnections function with the greatest possible efficiency. This efficiency is greatly affected by external factors associated with the guideway on which the vehicle travels, such as weather and visibility conditions, traffic patterns, accidents, repair and construction activities, and so forth. Safety and economy can be significantly increased by improving the integration between the vehicle and its guideway, taking advantage of the smaller size and reduced cost of current computer and electronic communications equipment. Such improvements might involve, for example, communications, radar braking, navigation aids, guided steering, remote vehicle sensing, and other innovations. Research is needed on techniques for sensing, processing, and displaying data on the condition of both the vehicle and the guideway. Research is also needed for the development of engineered safeguards and operator training procedures.

Alternative Fuel Sources. Although energy supply is not currently a critical issue, it will most probably reemerge as a major problem within the next few decades. Technology development on a variety of energy sources will minimize the nation's future dependence on imported oil and pave the way for the eventual smooth transition to the use of new sources. Research is needed to provide the engineering knowledge on which to base advances not only in the traditional energy areas, including nuclear power, but also in the newer, less well-developed technologies such as coal liquifaction/gasification, beneficiation, and utilization; oil shale extraction and processing; solar energy conversion; and the conversion of low-grade or low quality fuels.

Low-Grade Mineral Recovery. U.S. national security and wellbeing demand that plentiful domestic sources of a broad spectrum of important minerals be maintained. However, many of the highest quality domestic deposits have been greatly depleted, and those being exploited today are generally low grade and both difficult and expensive to process. New and more economical techniques and technologies are required for exploration, mining, and processing. Included among the most important research needs are those involving sensors and instrumentation, computer-assisted design and systems analysis of mining and extraction processes, resource mapping and management tools, and the use of colloidal and biological processes in the concentration of minerals and the treatment of effluents. Research is also needed to provide fundamental knowledge on the behavior of minerals during fracture, dissolution, and transport.

Biomedical Engineering. Exciting developments are under way in biomedical engineering—the application of engineering principles to the study of the human body in the context of health and fitness—that hold enormous possibilities for the future. These include diagnosis without exploratory surgery, surgery-free treatment of arterial blockage, relief of deafness and possibly paralysis with neural prostheses, and the continued development and improvement of artificial organs. However, to further advance the development of devices and procedures for delivering better health care at lower cost, research is needed in the following key areas: biormechanics, to determine the response of the body to physical stress; biosensors, to convert biological responses into electronic signals; and biomaterials, to replace, repair, and augment body components and functions. Additional research is also required to continue the development of advanced metabolic imaging techniques, of which nuclear magnetic resonance and positron emission tomography are two currently important examples.

Hazardous Material Control. The health of the environment has an increasing impact on the health and quality of life of its human inhabitants. Of the many alarming environmental problems, most of which can be traced directly to growing industrialization and increasing population, the most pressing involves the treatment and management of hazardous materials—especially toxic chemicals. Research is needed on: the movement, fate, and effects of chemicals in the environment to develop control and remediation strategies and to assess the ability of the environment itself to deal safely with the contaminants; conversion techniques, such as combustion and microbial transformation, to eliminate hazardous materials from the environment rather than storing them in it; and sensors and measurement methods to permit efficient process control and accurate assessment of environmental contamination or the progress in preventing it.

Mechanics of Slowly Deteriorating Systems. The internal and invisible slow deterioration of a wide range of engineered systems, including, for example, older railroads, bridges, aircraft, ships, and pipelines, poses a serious threat to the safety of their users. Yet our ability to understand how deterioration occurs in transportation as well as other systems, and how far it has progressed, is extremely limited. To improve our understanding of the mechanics of slow deterioration, research is urgently needed to develop new methods for nondestructively assessing conditions in the interiors of deteriorating structures or parts. Many of these nondestructive evaluation methods will also have direct applications as inspection techniques in a variety of production systems.

Computer-Aided Design of Structures. Although still in its infancy, computer-aided design is becoming—and will remain—a central part of the structural design process. It will increase the integrity and versatility of structural designs and, in the end, greatly reduce the time and cost of their construction. To realize this potential, however, research must first be pursued on: nonlinear, three-dimensional analysis including the modeling of complex geometrical effects; proportioning of structural elements, that is, the translation of structural behavior data into the physical dimensions of actual structural members; interactive computer graphics for structural design applications; and project-wide integration for carrying computer-aided design through the fabrication stage, which is the structural equivalent of manufacturing.

Manufacturing Modeling and Simulation. Currently available mathematical models of materials, physical objects, and manufacturing processes are far from adequate for the requirements of computer-integrated manufacturing. Research is needed on the automatic derivation, definition, and verification of structured computer data bases for product modeling, with which product production and testing can be completely planned, controlled, and implemented. Continuing research is also needed on computer models for manufacturing processes that can predict the effects of process variables, including dynamic variations, on final results. Additionally, research is also needed on computational and algorithmic problems in robotic applications involving, for example, image processing, collision avoidance, and response to fault conditions.

Electronic Device and Packaging Technology. Continued progress in several categories of devices is vital to the future health of the U.S. computer industry and, by implication, to our national security. With regard to integrated circuits for computers, continued research is needed on chips using bipolar silicon logic circuits to obtain high switching speeds and on chips using very large-scale integration to obtain high circuit densities for both memory and logic functions. Of especial importance are the advances needed in fabrication methods for submicrometer-sized structures and in the development of three-dimensional devices and circuits. With regard to the packaging technology that provides the power and signal interconnections for the multitude of chips in large-scale general purpose and scientific computers, improvements are needed in signal delay, power dissipation, and parasitic coupling. In addition, research is also needed on magnetic and optical storage devices to achieve increased storage densities.

Conclusion

It is essential that the nation, through its governmental, industrial, and academic leaders, give greater attention and support to engineering research. In order to do this, funding must be increased and made more stable, and academic institutions must be strengthened to ensure an adequate supply of trained researchers. Finally, national emphasis on the program of research outlined here will pay large dividends in greater national security, productivity, and international competitiveness.

INTRODUCTION AND BACKGROUND

What is Engineering Research?

Other than those who pursue it or who utilize its results directly, few people have a clear understanding of the meaning of the term "engineering research." To some it sounds like a contradiction in terms: *scientists* perform research; engineers devise applications—or at least this is the common perception. However, although there is a large community of research scientists, there is an even larger community of research engineers working in universities, industry, and government. Their investigations cover a broad spectrum from the most basic (fundamental) to the most highly applied. The engineering research they conduct plays an increasingly key role in the development of technology for commercial or defense purposes, or to improve the quality of life for all.

The objective of science research is to discover new knowledge about the world of nature. Engineering researchers also discover new knowledge about the natural world, but primarily they seek new knowledge about the *man-made* world—both the world of today and the projected world of tomorrow. They utilize advances in science, mathematics, and engineering to expand the useful knowledge base and to discover the engineering principles by which significant improvements in the processes of engineering design and production can be obtained. The end result is that engineering systems, products, and services can be produced more efficiently, more economically, and with higher quality. Figure 1 illustrates the interrelation among these activities.

It is virtually impossible to formulate a single precise definition that encompasses the full range of activities and objectives represented by engineering research. The boundaries with science research on the one side and development on the other are often hard to discern. Important advances in science often open up many new lines of engineering research designed first to explore and then to establish the connection between those advances and engineering applications. In many instances, however, the fundamental engineering principles needed to form an appropriate knowledge base for engineering applications are derived not from scientific principles or discoveries, but from research into the functional characteristics of engineering systems. Indeed, product/process development efforts sometimes yield this generic type of engineering knowledge (note the feedback loops in Figure 1). Thus, the knowledge base in an area such as manufacturing will ultimately consist of engineering principles drawn from many different engineering disciplines and activities, with little direct reference to the laws of nature.

The driving force for research can be either the need for advances in engineering to meet society's demands or the desire to capitalize on new and promising technological opportunities to benefit humankind. In universities, the education and training of doctoral students and the need for an up-to-date curriculum are also important motivators; in industry the market is the main driver. In both cases, the emphasis on explicit societal demands is

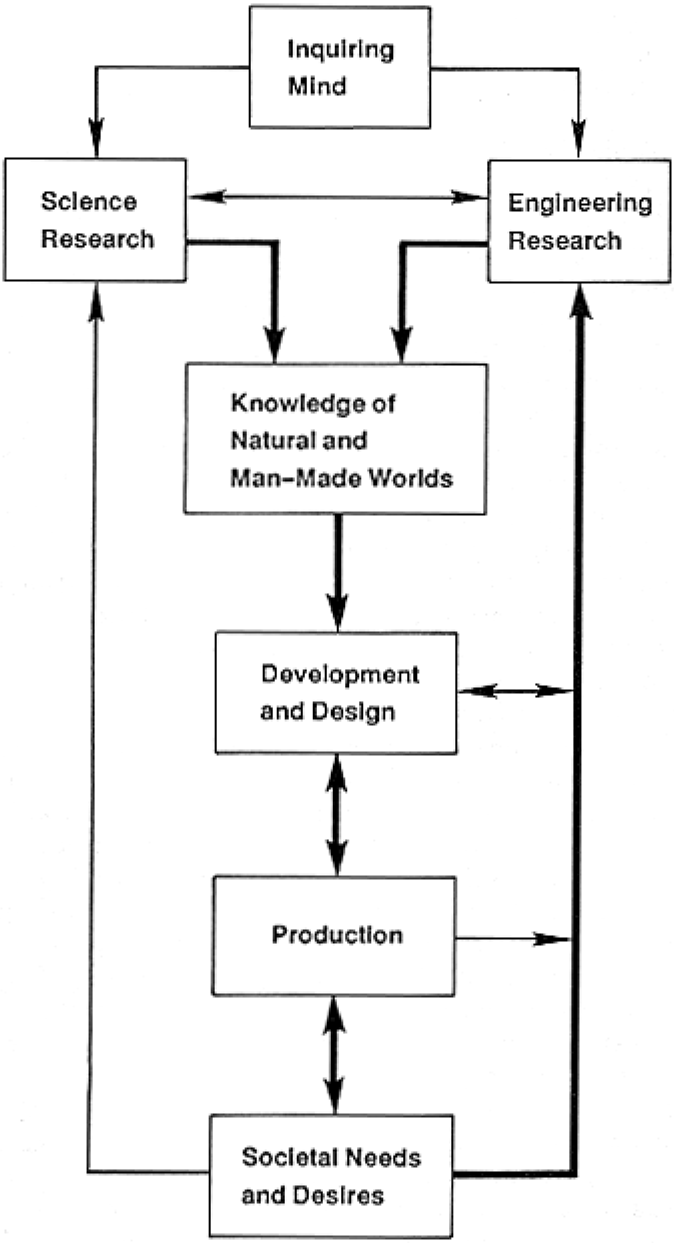

Figure 1
Engineering researchers seek new knowledge about the world of nature and the man-made world. Their ultimate objective is to improve the processes of engineering design and production, so that products and services needed and desired by society can be produced more efficiently, more economically, and with improved quality.

one of the main differences between engineering research and science research. Alternately, whereas science may support decisions, engineering research supports actions.

What is the Value of Engineering Research to Society?

Past Accomplishments

We in the industrialized world live in a technological society undreamed of even by the visionaries of a century ago. The technological miracles we take almost for granted offer us an extraordinary ease of living, security, enjoyment, and prosperity. This achievement has not been easy, however. It represents a continuous effort on the part of engineering researchers to advance our understanding of how physical and mathematical laws can be used to benefit humankind.

That effort began in ancient times; it entered its modern phase toward the end of the last century, when Thomas Edison experimented with ways of generating, channeling, and using electricity. It can be said that modern engineering research was born in his Menlo Park Laboratory. The ability to use electricity sparked the growth of modern *communications*. From the telephone to radio and TV, engineering researchers have been instrumental in making the breakthroughs that brought progress. There is no clearer symbol of the power of technology than today's instantaneous worldwide communication networks linked by satellite.

As the *energy sources* that fuel the technological society have evolved from wood to coal, and then to fossil fuels and nuclear power, engineering researchers have provided the knowledge on which the energy conversion technologies were based.

Effective, efficient *transportation* systems have also been developed on the basis of engineering research into elements of the vehicles themselves (automobiles, trucks, trains, ships, and aircraft), including the principles underlying their design, composition, propulsion, production, and control systems.

The *information and data processing* systems on which our economy increasingly relies are, collectively, a prime example of the power of science and technology to transform our lives. Computer hardware and software alike have required fundamental advances in our ability to manipulate the man-made world down to subatomic levels, advances to which engineering research has contributed heavily.

The *machines* that perform virtually every function once performed by humans and animals—and do so much more efficiently and reliably—are a result of engineering research in materials, mechanics, electronics, and manufacturing processes.

Structures of every kind—from commercial and residential buildings to bridges, dams, and tunnels—have become larger, stronger, safer, and easier to build through research into their design and construction.

Because the expansion of the man-made world often has major impacts on the natural world, engineering research has also made it possible for *environmental systems* to lessen the harmful effects of technology on human and other life forms. Through these systems we can strive to live in harmony with our environment.

Future Possibilities

None of these lines of development in technology has reached its limit, nor is any likely to. Engineering research must continue its broad support of technology if it is to meet future demands. Such research is now on the verge of yielding enormous new benefits in a number of new areas. New biological products and synthetic materials will soon appear. New manufacturing methods based on engineering research will make the production of high-quality goods more efficient, more reliable, and less expensive, thus improving the competitiveness of U.S. industries. In addition, new breakthroughs, as yet undefined, may be expected in many other engineering disciplines.

The future can bring large and rapid improvements in the quality and diversity of everyday life. For example:

- With advances in optical communications and computer technology (e.g., speech understanding, natural language programming, and larger memory), the communication of voice, data, and video signals over an integrated national network could become routine.
- "Information utilities" could provide low-cost access, from home or office, to extensive information on virtually any subject.
- Data collection and recordkeeping could become so systematized and coordinated among institutions and consumers that

most ordering, billing, and banking transactions would be done instantaneously via electronics.

- Thinking machines based on artificial intelligence might control many functions and operations in the economy, including the design of more advanced computers and software.
- Urban automobile transportation, parking, and terminal connections to rail, air, and marine modes could all be carried out smoothly, efficiently, safely, and conveniently under network control regardless of weather conditions and peak demands.
- Much construction could be automated, as well as mining and exploration operations on land and in the oceans.
- The physical environment of the United States and even of the Earth itself could be modeled in some detail, so that (for example) weather and the environmental impacts of certain large-scale events could be predicted with greater confidence. Data collection on environmental factors could become so thorough that the status of the environment could be monitored fairly accurately.
- Large-scale production of engineered microorganisms and their chemical by-products could offer safe and inexpensive ways of controlling insects, neutralizing toxic wastes, and producing a variety of foods, drugs, and other products.
- Medical technology could alleviate most common ailments and thus prolong the average life span while reducing the infirmities of old age. Deafness, blindness, muteness, and paralysis are all examples of disabilities whose effects could be mitigated by biomedical devices.
- "Molecular reactors" based on chemical catalytic processes might be able to selectively produce novel and complex chemical products such as drugs, fibers, and fuels on a large scale.
- The availability of a wide range of inexpensive polymeric and ceramic composite materials would mean that equipment of all kinds could be much more durable, contain fewer parts, and require less lubrication and fuel. (Large structures in space are likely to be fabricated mainly from these materials.)

Why is Engineering Research Essential?

With the continuing explosion of scientific and technical knowledge that has characterized the years since World War II, engineering research has become an ever more essential link in technology development. Each field of engineering relies on an expanding knowledge base for its continued growth. In addition, advances in basic knowledge in every field of science and engineering now must be "translated" through research before they are accessible to application. Products in many fields—electronics, biotechnology, and the aerospace industry, for example—are so complex and expensive to develop that supporting research is indispensable.

Both the pace and complexity of discovery today are changing the nature of technology development. The flows of knowledge between science and engineering, and between academia and industry, are increasingly two way. Technological advances such as the computer are providing the basis for new research methods and are opening up new fields of scientific inquiry. Finally, advances made in industry are, in many cases, beginning to drive academic research.

These changes are a natural function of the speed and sophistication of technology development. It is a process to which some of our strongest industrial competitors are adapting more easily than we. In that increasingly two-way process, the role of engineering research becomes more important. As Dr. Roland Schmitt (1986) of General Electric puts it, "[Engineering research]—the region where the leading edge of research meets the cutting edge of application—is becoming more than ever before the key battleground of international competition."

Given the speed with which our advances in basic research are turned to commercial advantage by our international rivals, it is critical to the nation's economic future that we begin to capitalize faster and more effectively on our own breakthroughs in scientific and technological knowledge. There is a bottleneck in the technology development process. Engineering research can help break that bottleneck. Other factors also affect industrial competitiveness. Yet engineering research provides unusually strong leverage—especially when that research is efficiently coupled to applications. Engineering research may hold the greatest potential for improving the nation's competitiveness and security and achieving a higher quality of life for its citizens.

What is the Current Status of Engineering Research?

The majority of fundamental engineering research today is-carried on in university laboratories. However, industrial lab oratories, with their up-to-date equipment and highly qualified personnel, account for an increasingly large proportion of the engineering research—fundamental as well as applied—conducted in the United States.* Federal agencies provide direct funding support for most of the university work, as well as for engineering research conducted at various federal laboratories. The agencies providing that support, and their relative levels of support, are depicted in Figure 2. The main areas of emphasis for each agency are listed in Table 1.

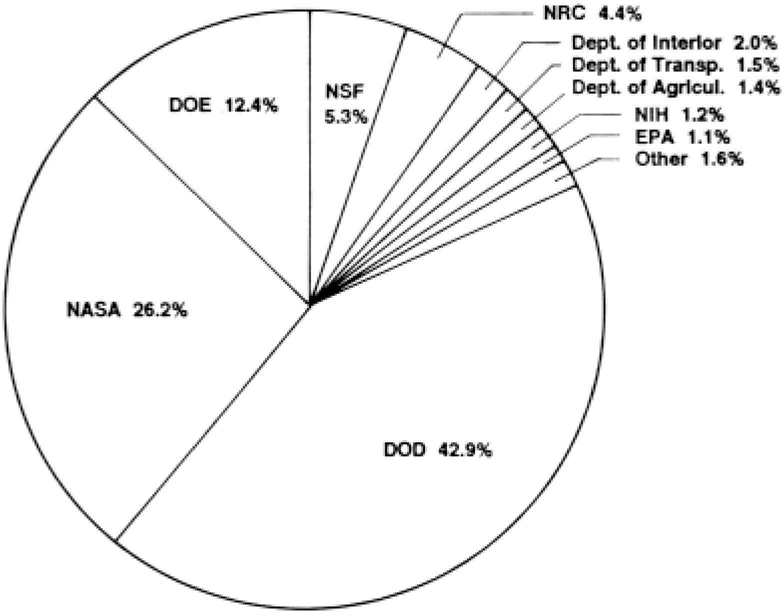

Figure 2
Federal agency support for engineering research (FY85; total: $3.85 billion, estimated).
(SOURCE: National Science Foundation, 1984c.)

Despite this range of support, the engineering research link suffers from neglect. The nation has a preeminent science base, well supported by government; in addition, both industry and government provide good support for engineering development.

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* Industry funding for all basic and applied research has risen substantially in recent years, both in dollar amounts and as a percentage of the national total (National Science Board, 1985). However, no data differentiate science and engineering research in industry, so that this statement is based only on inference. For further discussion, see the section on "Funding Issues."

Table 1 Engineering Research Emphases by Federal Agencies

Agency	Main Areas of Involvement
DOD	Microelectronics, computer architectures, information and communication systems, systems engineering, manufacturing technology, aerohydrodynamics, nuclear energy, high-energy systems
NASA	Aerodynamics, aerothermodynamics, life support, human factors, materials, dynamics, propulsion
DOE	Energy systems, power generation, alternative energy sources, nuclear fusion/fission
NSF	Engineering fields: chemical, biochemical, thermal, mechanical, structures, materials, electrical, communications, systems, design, manufacturing, computer
NRC[a]	Nuclear systems engineering, power generation
Interior	Mining and metallurgy; environmental, water, and soil resources engineering
Transportation	Systems engineering, safety/human factors, materials, communications
Agriculture	Agricultural, biotechnology, water, and soil resources
NIH	Biomedical, biochemical
EPA	Chemical, biochemical, environmental

[a] Nuclear Regulatory Commission.

Yet engineering research is generally accorded low priority by both sectors. Partly this is because engineering research is relatively long term, compared to development, whereas its natural support base is one in which time horizons are fairly short. Partly, too, it is because there have been few voices speaking on its behalf. This report is the first opportunity the engineering research community has had to present a broad-based, comprehensive overview of its needs and directions. It is addressed primarily to government leaders, to industrial R&D managers, and to academic engineering researchers and administrators—the sponsors, shapers, and doers of our nation's engineering research effort.

To strengthen the engineering research link in the technology development process we will need, as a nation, to better understand the role of engineering research. We need to identify emerging opportunities in research and be willing to focus the necessary resources on them. Finally, we need to harness the outputs of that research more effectively. This report identifies the opportunities and explores ways of addressing the needs of engineering research.

KEY ENGINEERING RESEARCH OPPORTUNITIES

A primary task of the Engineering Research Board was to identify especially important and/or emerging areas of engineering research. Early in its deliberations, the board decided to approach this daunting task by forming separate panels charged with examining selected areas of research. These panels were divided not along disciplinary or technological lines, but on the basis of engineering systems. The consensus was that such a classification would most accurately reflect the present and future "topography" of research needs in engineering—particularly those most crucial to the nation's competitiveness.* The resulting coverage of engineering research, although very broad, would not try to be comprehensive.

Thus, seven panels were defined and formed as follows:

1. Bioengineering Systems Research;
2. Construction and Structural Design Systems Research;
3. Energy, Mineral, and Environmental Systems Research;
4. Information, Communications, Computation, and Control Systems Research;
5. Manufacturing Systems Research;

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* An important category of engineering systems are those related to defense and national security objectives. The board decided not to include defense needs as a separate category of research because they inevitably cross-cut many of the other areas—such as, materials, information/communications/computation, transportation, manufacturing, etc. However, the relevance of various research areas to national security is noted in the discussions.

6. Materials Systems Research; and
7. Transportation Systems Research.

The reports of these panels provided most of the substantive input on which the Engineering Research Board's report is based (see the individual panel reports also in this volume.)

The "Systems" Context

Previous reports on engineering research have examined research needs in either a single discipline of engineering practice (e.g., civil or mechanical engineering) or a single field (e.g., materials, manufacturing, or biotechnology). This is the first such report to adopt an organizing principle based on the "systems" nature of engineering activity. Because this concept may be unfamiliar to some, it merits discussion.

The concept of engineering systems means different things to different people, depending on an individual's background and experience, and on the context in which the term is applied. It has a different meaning in the context of a manufactured product, such as an automobile or computer, than it does in the context of a network such as a transportation or communication system. Scale is one important factor: A composite material can be considered a system on a microscale, whereas an aircraft frame constructed of various metals and composites is a material system on a macroscale.

The important characteristic shared by all systems—regardless of their scale—is the integration of various components to optimize certain desired features. Most industrial firms design and manufacture systems as their end product; indeed, most manufacturing processes are in themselves systems. In the context of a product, the concept of engineering systems is very close to what is implied by "systems engineering"—in which various components and subsystems are designed to interact in such a way that performance, weight, reliability, appearance, cost, and other important parameters can be optimized appropriately. In the context of a process, the concept closely resembles operations research or management research directed at optimizing an overall endeavor or process.

In this report, the use of the term "systems" includes both meanings. Products are regarded as integrated systems within a larger process that is itself a system. Research in the engineering systems environment must therefore take into account not only the elegance of analysis but also the usefulness of potential products, the likely efficiency in the use of resources of all kinds, and the potential human, economic, and societal impacts. Feedback at all levels is an essential element of any true system. (It might be noted that the technology development process, as depicted in Figure 1, is itself a system, with multiple feedback loops.)

Such research is by its nature cross-disciplinary. It is in this latter characteristic, in particular, that engineering systems research represents a change in the traditional culture of engineering research that developed after World War II. However, the success of any system requires that both its component parts and its overall organization be functionally strong. Thus, support for the systems concept of engineering research and education in no way diminishes the need for strong traditional engineering disciplines, although it may refocus some of the emphases within those disciplines. To give but one example, modern industrial engineering has an important role to play in systems design and organization.

Criteria For Selecting Research Needs

The research needs described in the following section as being particularly worthy of federal support were extracted directly from the seven panel reports. Thus, the same criteria applied in those separate studies are applicable to the topics identified here, namely:

- a strong potential to improve the industrial competitiveness, quality of life, and/or national security of the United States; and
- the technological opportunity afforded by recent advances in science or engineering.

Each of the panel reports had identified a number of engineering research areas within the purview of that panel that appeared to clearly meet these criteria. In all, more than 80 topics were identified. From each panel's list, the panel's chairman selected the highest priority topics and presented them to the board for discussion. Fourteen topics were selected.

The board then considered the possibility of further prioritizing these areas. Clearly, each of the panels already represented an enormous scope of research, with each possibly equivalent to a field like chemistry or physics. Indeed, certain panels encompass the entire engineering research role of more than one funding agency. The research needs that they had identified already represented a considerable effort in selection, and each is clearly important. For the board to have narrowed all of these high-priority research needs in all seven areas down to a handful of topics thus required an extreme effort in prioritization (comparable, for example, to assessing research needs in all of science). The board concluded that to rank-order the resulting 14 specific areas of research within the seven panels would be an arbitrary exercise that might damage the overall profile of engineering research that the nation must maintain.

Nevertheless, a consensus did develop that three research areas in particular must be considered to be of extremely high priority. Those three areas are (1) increasing the power of complex software systems and the productivity of their development, (2) manufacturing systems integration, and (3) advanced engineered materials. Descriptions of these three areas follow.

Selected Research Areas

Bioengineering Systems

It is estimated that the market for bioengineered products, including both the products of biochemical engineering in the new field of biotechnology and the products of biomedical engineering, will be as large as $100 billion by the year 2000. Bioengineered products have enormous potential for improving human health and relieving suffering. Other nations are rapidly gaining momentum in these important areas.

Bioreactors. Current fermentation technology is inadequate to meet the bioprocessing needs of industry. New techniques are needed for large-scale culture of plant and animal cells and engineered organisms. Fundamental knowledge of how physical and environmental factors influence biosynthetic pathways within cells is essential to the development of such techniques, and will require an unusual degree of interdisciplinary scientific/engineering research. Parallel research is required to develop methods for using various enzymes or cells as catalysts for biosynthesis. The challenge here is to translate the existing knowledge base for chemical reactors into biosystems, in which the operating problems and requirements are very different.

Advances in Biomedical Engineering. Americans are living longer and demanding more and better health care. Injuries are always a concern (for example, each year some 80,000 Americans sustain permanently disabling but nonfatal injuries to the brain and spinal column). At the same time, the cost of health care is increasing rapidly. Biomedical engineering is the application of engineering principles to the study of the human body to provide the knowledge needed to develop devices and procedures that can deliver better health care at lower cost. Some of its key elements are biomechanics (determining how the body responds to physical stresses), biosensors (which convert biological signals into electronic signals), and biomaterials (used to replace, repair, or augment body parts or functions). Development of advanced metabolic imaging technologies for research and diagnosis is an important area (two currently important examples are nuclear magnetic resonance and positron emission tomography). Benefits from biomedical engineering will include such things as: diagnosis without exploratory surgery; treatment of arterial blockage without surgery; relief of deafness and, possibly, paralysis with the use of neural prostheses; and development of effective artificial organs.

Construction and Structural Design Systems

Construction has suffered from a chronic lack of research attention. Yet it is a $200 billion industry in the United States alone, and one in which foreign firms are making great competitive inroads in traditional U.S. markets. New materials and new technologies could potentially improve the effectiveness with which structures are designed and built.

Construction Robotics. Because all constructed facilities are custom-designed and custom-built (largely on-site), the industry is labor-intensive and has a chronically low productivity rate. Because operations such as lifting and installation are still done mainly by humans, the size of most components is governed by human physical capacity. Construction robotics could greatly augment current capabilities by: extending the workplace into new environments; extending vision capabilities into hidden areas; performing high-quality, repeatable work operations; reducing safety and health hazards; providing lifting and positioning capability for very large payloads; and affording mobility, dimensional control, and versatility over a range of project sites and sizes. Research is needed to proceed from current industrial robotics to construction robotics and to incorporate a variety of unique functions (e.g., mobility, flexibility, and a high payload-to-weight ratio). New construction design concepts, materials, and methods will also need to be devised to exploit the capabilities provided by robotics.

Computer-Aided Design (CAD). CAD is becoming a key tool of structural design; as yet, however, it is still in its infancy. Before it can yield the many benefits it promises, several areas of research must be pursued. These areas include nonlinear analysis of structures; improving the coordination of analysis and experiment in the design process; use of supercomputers to improve behavior analysis of very large, three-dimensional structural systems (e.g., large buildings); advances in interactive computer graphics; and extending CAD from the design concept on through to the fabrication of the actual structure by means of a project-wide integration of what have been separate, computerized operations.

Energy, Mineral, And Environmental Systems

Mineral resources and energy are the basic input, and environmental impacts the output, of our technological society. They are fundamental to all other technological activities. Yet the first two are threatened by diminishing domestic supplies and dependency on foreign sources, whereas the third is beginning to affect not only our own nation's long-term well-being, but also our relations with neighboring countries. Engineering research can increase our options and give us greater control over these vital areas of concern.

Control of Hazardous Materials. Protecting the environment becomes increasingly important as industrialization, the population, and our national standards of living all continue to rise. Of many emerging environmental problems, the most pressing is that of hazardous materials—specially toxic chemicals. Better technology is essential to solving this difficult problem, as it can lead to alternative products and industrial processes that reduce the production of hazardous chemical wastes, as well as to safer and less expensive methods and processes for handling and disposing of these wastes. Conversion techniques have great promise for alleviating these environmental hazards. One such approach is combustion; a much better fundamental understanding of the overall physics and chemistry of combustion technology is needed in order to apply it more effectively. In another approach, engineered or naturally occurring microorganisms can transform many harmful organic materials into inorganic products that are harmless, natural constituents of the environment. However, more research is needed on the movement, fate, and effects of chemicals in the environment in order to better understand and exploit these microbial transformation processes. Research on sensors and measurement methods is an important related area, as it would improve the detection and monitoring of hazardous materials and permit efficient process control. Clearly, a broad systems approach integrating the various factors noted previously will be needed to produce useful solutions to these environmental problems.

Alternative Fuel Sources. A continuous, readily available supply of energy is essential to the well-being of the nation's citizens, its industry, and its defense. Long-term investment in research is necessary to provide a diversity of energy sources that will minimize the nation's vulnerability to a loss of supply for any reason. In addition to traditional sources (including nuclear power), especially important newer technologies requiring continuous attention include coal (liquefaction, gasification, and treatment to improve its properties, as well as advanced concepts for its direct utilization), extraction and processing of oil shale, solar energy (especially photovoltaic devices), new and improved techniques for extracting petroleum, and techniques for converting low-grade or low-quality fuels economically into electricity or other energy forms.

Recovery of Low-Grade Minerals. Minerals are the raw materials of technological activity. However, existing high-quality U.S. mineral deposits have been greatly depleted. Those deposits now being exploited are lower in quality and more difficult and expensive to process than were those used in the past. Because existing exploration, mining, and processing techniques are not fully adequate for exploiting low-quality reserves, new techniques are needed to improve efficiency. These techniques include improved sensors and instrumentation for exploration, mining, and processing; CAD and systems analysis of the mining and extraction process; and the use of colloidal and biological processes (e.g., leaching using engineered microorganisms) to concentrate minerals and treat effluents. In order to reduce minerals effectively to the small size range suitable for the latter processes, a fundamental data base must be developed on the behavior of minerals during fracture, dissolution, and transport.

Information, Communications, Computation, and Control Systems

These areas, taken together, embody the revolution that is proceeding in electronics and computers. They are central to modern manufacturing and defense, and are increasingly important in many aspects of daily life. Staying at the leading edge in these areas will be basic to the future economic health and security of any nation.

Productivity in Development of Complex System Software. Our ability to design, code, test, and modify large software systems has improved somewhat in recent years, but is still inadequate. U.S. manufacturing industries and defensive systems are being held back by the cost and difficulty of producing software. Fundamental research is needed on methodologies for the efficient development of large software systems (e.g., for communications systems and distributed computing). Also important is research on the compatibility, reuse, and standardization of key software modules. Related needs are for research on: software reliability, testing, and verification; distributed data bases; and real-time processing of data generated in large volumes (e.g., by spacecraft).

Electronic Device and Packaging Technology. Several categories of devices underlie progress in computation. One such category is integrated circuits. Very large-scale integration has received much attention for application to both memory and logic circuits. However, the continued improvement of bipolar silicon logic is equally important—especially for large-scale, general purpose and scientific computing—and is not receiving sufficient attention in university research. Another basic category is interconnection structures. These structures provide the thousands or even millions of signal and power interconnections used within a chip. Improvements in reliability, signal delay, power efficiency, and parasitic coupling all require better technology than is now available. A third category is information storage media and devices. A number of new approaches in both magnetic and optical storage offer the promise of enormous increases in storage density.

Manufacturing Systems

The use of new technologies based on the computer presents an opportunity to vastly increase the efficiency of the manufacturing process, and thereby to regain our nation's competitive edge in manufacturing. Taken to their logical conclusion, robotics and other elements of factory automation can be integrated to permit flexible, "lights-out" manufacturing around the clock.

Systems Integration. Integrating all of the human and machine-based elements of manufacturing will yield great improvements in efficiency. However, our basic understanding of this area falls far short of meeting the needs for systematic, generic approaches to designing computer-integrated manufacturing (CIM) systems. Major programs of cross-disciplinary research are needed to provide better engineering methods for systems integration. Research should be aimed at creating new software and hardware that is modular, compatible with other systems, adaptable to new requirements, and that can be easily understood by its users. A long-term goal is to develop the techniques of artificial intelligence to provide computer-based capabilities such as inference and intuition that can be applied to the design and "diagnosis" of manufacturing systems.

Modeling and Simulation. There are still important areas in which available models of materials, physical objects, and manufacturing processes are inadequate for the needs of CIM. Research is needed to define structured computer data bases for product modeling. Such data bases should provide all information neces sary for planning, controlling, and implementing the production and testing of a product. More research is also needed on computer models for manufacturing processes; many current models are unable to predict what effect process variables will have on final results. Computer simulation techniques are still evolving rapidly. High-quality computer graphics provided at relatively inexpensive workstations is one very desirable goal.

Materials Systems

Advanced engineered materials of various kinds hold great promise for the creation of new products and new standards of performance in virtually every commercial field, as well as in military systems. The word "advanced" here implies new methods for the synthesis and processing of materials; "engineered" refers to materials that have been *created* to meet property specifications desired for some end use (rather than finding end uses for materials with a set of properties fixed by nature, as was formerly done). This new approach to materials design applies to the entire range of materials—from critical components of micro-electronic devices to materials used in large structures, vehicles, and highways. There is enormous potential for such materials, but research is needed to capitalize on the opportunities they afford. For example, the current "ceramic fever" is fed by the expectation that new processing techniques will yield ceramics meeting hitherto unattainable strength and temperature requirements, if we can better understand the forces between particles only slightly larger than some molecules. Advances in computer architecture and processing speeds, and in lightwave technology, require renewed emphasis on a full range of electronic materials. For biomaterials, we need a better understanding of what determines biocompatibility if we are to realize the full potential of many prosthetic devices. For commerce and defense the demand continues for structural materials combining higher strength and lower unit weight. We can meet this demand through proper engineering; but first we must gain a better understanding of how materials bind, deform, and rupture, through the use of modern analytical instrumentation.

Transportation Systems

Although transportation systems and services account for some 20–25 percent of the U.S. gross national product, very little research is conducted on most modes of transportation. Yet the efficient and productive design, manufacture, operation, and control of transportation systems has a major impact on daily life, on commerce, and on our ability to respond quickly to a national emergency. Even a small increase in research effort, well placed, could yield large benefits in our ability to move people and goods in a fast and efficient manner. Without such research, the next 25 years could well bring a rapid deterioration of this vital nationwide system.

Vehicle and Guidance System Integration. Increases in computing power and data storage (with smaller component size) and advances in electronic communications (all at lower cost) have opened up new opportunities to better integrate vehicles with their guideways. In the case of cars and trucks, this might involve communications, radar braking, traction control on slippery pavement, navigation aids, guided steering, new traffic control systems, and other innovations. Similar advances are possible in other transportation modes and among modes. Research must include techniques for sensing and processing data about the condition of vehicles and guideways, algorithms for efficient logistics, and driver acceptance and training requirements. The goal is to create a network (or system) of transportation modes and modal interfaces that is optimized for maximum productivity, defense effectiveness, reliability, and safety, and for minimum congestion, cost, environmental impact, and consumption of resources.

Mechanics of Slowly Deteriorating Systems. The public is increasingly concerned with the reliability of products, machines, and structures. In both the commercial and defense arenas, we need better ways of predicting when and how such systems are likely to fail. Reliable means are needed for assessing the safety of older railroad and highway bridges as well as that of aircraft, ships, and pipelines. Yet our ability to understand how the slow process of deterioration occurs in transportation systems, to determine how far it has progressed, and to analyze existing systems (as well as to design new ones) is limited. Extensive fundamental research is needed to better understand the mechanics of slowly deteriorating systems and to develop a basis for their nondestructive evaluation. This research should include the identification of "almost-failed" systems and the mechanics of that phase of deterioration. An outstanding need is for new methods of assessing conditions in the interior of a deteriorated structure or part. Timely repair or replacement of existing systems or components—neither too early nor too late—is essential to ensure public safety at an affordable cost.

Cross-Cutting Research Needs

It is evident from the foregoing descriptions of priority research needs in each panel area—and even more so from the full reports of the panels—that a number of technologies and research thrusts cut across much of contemporary engineering research. The board believes that it is worthwhile to highlight these common threads, because they indicate areas of research that may be "gating" advances across a broad front of technology and in which the overall impact of focused research may thus be greater than the sum of the separate efforts in different fields.

Computers

The computer has revolutionized engineering and holds the promise of allowing enormous advances in the future. Complex computations can now be performed rapidly and cheaply. Great quantities of information can be stored, organized, analyzed, and displayed efficiently and effectively for decision making. To take full advantage of this marvelous tool, new engineering concepts must be developed and many new fundamental engineering principles established.

Before this revolution can continue, however, large gaps must be filled in our basic knowledge of both the physical phenomena and the most appropriate methods of computation. This need is obvious in applications such as computational mechanics for both solids and fluids, an area of research that underlies, for example, CAD of all structures, machines, and vehicles; welding and other fabrication techniques; and deformation processing of materials. It is no exaggeration to say that entirely new knowledge bases must be created to utilize the computer in emerging areas of engineering such as integrated manufacturing systems.

Powerful as today's computers and supercomputers are, they are still inadequate for some problems of engineering practice as well as for some problems that engineering researchers face. Therefore, continuing fundamental engineering research on the computer itself and its associated component devices and materials also is essential. There is an urgent need to develop new principles of computer science and engineering, and to refine and modify older principles that now have limited validity.

Modeling and Simulation

Improvements in the power and availability of computers have led to enormous advances in modeling and simulation of products and processes in every field. Whether it be a new material, a building, an aircraft, a transportation network, a battlefield, or the movement of environmental contaminants, the ability to construct an accurate computer model can offer enormous dividends in cost, time, and understanding.

However, improvements in computation have outdistanced improvements in our modeling capability. Current models for solid objects, for example, do not provide unambiguous geometric (three-dimensional) information, and they provide almost no information on nongeometric characteristics and functions. Models of processes (e.g., manufacturing) are equally incomplete.

Computer simulation techniques are being driven forward by advances in hardware, simulation software, and process models; but there is still far to go in this area as well. The use of simulation in engineering curricula is a particularly valuable application.

Systems Integration

The drive toward integration of large systems, including both product and process, may be the most fundamental change in the approach to manufacturing since Frederick W. Taylor's ideas on "scientific management" took hold in the 1920s. Again, the computer has made this trend possible, because it permits the rationalization of such a complex endeavor as modern manufacturing.

The basic concept is that, in the manufacturing context, efficiencies in design, materials, parts inventory, cost and process control, use of manpower, and nearly every other element can be achieved through optimizing choices and timing, and through sharing data. The concept also applies to nonmanufacturing systems such as a transportation system. There, vehicle design would be coordinated with the communication-control system associated with the guideways (e.g., road or flight path), which would in turn be coordinated with economic and social factors such as peak-use times or the relative costs of different modes of commercial transport.

Because the scope of potential application is so large and system conditions are generally so variable, it has proven difficult to formulate generic principles governing systems integration. However, such a knowledge base is theoretically achievable, and much progress has been made for specific applications. The advent of supercomputers and, possibly, expert systems may permit the necessary algorithms to be developed; much more theoretical research is needed to establish the general rules by which these problems should be approached.

Processes and Processing

Because Americans have a talent for discovery, the nation has tended to emphasize development of the science base in many fields. Our achievements in materials science, computer science, and microbiology, for example, are preeminent. However, we have not paid corresponding attention to the processes associated with the use or commercialization of related products. As a result, other nations reap many of the benefits of our basic scientific discoveries.

For example, Japan, West Germany, and France all have government-funded programs directed at speeding up the commercialization of biotechnology—a field in which processing is currently the main bottleneck. The United States does not have such a program. Similarly, U.S. government programs fund materials science much more heavily than they do materials processing, even though processing is currently the key to a new world of engineered materials that may soon assume enormous economic importance. Japan is said to be producing large numbers of materials-processing engineers—the United States is not (National Research Council, 1984). Microelectronics is another area in which process design and control is becoming the crucial point for competitiveness.

Engineering research can lead to substantial breakthroughs in the efficiency, economy, and versatility of processing techniques. This is one of the most important ways to break the bottleneck in the technology development process and take full advantage of the nation's strong science base.

ISSUES THAT DETERMINE THE HEALTH OF ENGINEERING RESEARCH

In attempting to gain an overview of engineering research, the Engineering Research Board began by identifying seven subsets of engineering systems research, as described in the introduction. A cursory glance at the names of those seven panel areas might suggest that they have little common ground. One is hard-pressed, for example, to find many obvious common threads between bioengineering and transportation. Indeed, each of the panels has identified issues specific to its own nature and circumstances.

Nevertheless, there is a remarkable degree of commonality in the problems, pressures, and needs that are central to the health of each of the areas represented by the panels. Some of these may vary over time in their impact on any one panel, but the factors affect all areas to some degree.

We begin this assessment of the health of engineering research with a brief, specific summary of the health of each of the selected areas. We will then attempt an overview of the entire field from the standpoints of:

- funding issues;
- the adequacy of research facilities; and
- the adequacy of personnel resources.

It should be noted that there is a degree of inherent overlap between these issues and those discussed in the section on "Policy Issues," because policy questions—whether federal, academic, or industrial—have a substantial impact on the health of engineering research.

Summaries: The Health of the Selected Fields

Bioengineering

Bioengineering systems encompass both biochemical engineering (the engineering aspects of biotechnology) and biomedical engineering (which is concerned with medical technology and devices). Although this field has received great attention in recent years and has potentially enormous economic significance in both of its components, it is presently underfunded, and the research support is scattered among various federal agencies.

The primary support agency for bioengineering research is the National Institutes of Health (NIH), which devotes less than 2 percent of its overall budget to the entire field. NIH's support for bioengineering research is tiny compared with that for biological or medical sciences; yet engineering research is now the bottleneck for further development of these technologies by the United States. Support by the NSF is still small (less than $12 million). Although the recent move to coordinate support for biotechnology within the foundation is a hopeful sign (as is the new Biotechnology Process Engineering Center), other nations (i.e., Japan, West Germany, and Great Britain) all have stronger and better coordinated government sponsorship of research in this field.

Over the past decade, the proportion of bioengineering students in U.S. graduate schools relative to all engineering graduate students has remained roughly steady at about 2 percent. There is a shortage of biochemical engineers and faculty in particular. To carry out needed research in these economically competitive disciplines, more trained bioengineers will be needed.

Construction and Structural Design

Federal support for research in this overall field has traditionally been limited. Although modest amounts of research are being pursued in structural design, few universities have research programs in construction, and in general the field has had a hard time establishing and maintaining a respectable niche in academia. The primary reason for this situation is the considerable gap that exists between the theoretical orientation of universities and the practical values of the construction industry. In addition, the public perception of the construction field as being highly pragmatic militates against public-sector support.

Another impediment is the fact that civil engineering structures are usually one-of-a-kind items, with no time or money allocated in the contract for research. Thus, practitioners rely on established practices and on-site experimentation. The small size of most firms in the industry, and its current economic difficulties, mean that industry supports little research.

Overall, the supply of research-oriented graduates in civil engineering is adequate to meet present demands. However, although the construction field could benefit greatly from the application of advanced design and manufacturing technologies such as CAD and robotics, virtually no research specialists in these high-tech areas are interested in construction applications. If research funding were to increase significantly, the increase should be gradual in order to avoid creating a shortage (or an eventual oversupply) of qualified Ph.D.s.

Energy, Minerals, and the Environment

A present national complacency with regard to energy supply, mineral resources, and environmental protection has resulted in reduced research funding in these areas. Department of Energy (DOE) funding of energy R&D has decreased sharply. The Environmental Protection Agency's (EPA) research budget fell steeply between 1979 and 1983, and is now below that of 1977 in actual dollars. The EPA's program directed at long-term research in universities has been cut by more than 60 percent since 1981. These declines are occurring despite an increase in the proportion of industrial operating costs represented by environmental protection and growing public concern with the issue.

As research funding has declined, both undergraduate and graduate engineering enrollments in the specialities that make up this area have dropped sharply—especially in environmental and nuclear engineering. Interest in materials and metallurgy has held steady, but there is less emphasis on extraction and processing (the key engineering areas) than there is on research in materials science. Decreasing and less accessible domestic energy and mineral reserves, along with increasing hazardous materials contamination, pose future problems to national productivity and human welfare suggesting that the health of this area should not have been allowed to decline so far or so fast as it has.

Information, Communications, Computation, and Control

Although faculty shortages continue to be a problem in most areas of engineering, tremendous student interest in this field and a big demand for researchers in industry have led to an especially severe faculty shortage in this field. To limit the problem, many schools have set enrollment ceilings in electrical engineering and computer science departments. The quality of students is high, but many leave at the B.S. level. As a result, new Ph.D.s are in short supply—especially in "hot" areas such as artificial intelligence, robotics, and computers, where the demand is greatest and the faculty shortage is worst. Another major problem is the lack of up-to-date equipment for teaching and research.

Funding for research is adequate in most areas, although fairly uncoordinated across agencies and heavily oriented toward defense needs (so that restrictions on research publication are a recurrent problem). The field is in good health, broadly speaking. However, it is experiencing all of the difficulties as well as the advantages and excitement that accompany rapid progress.

Manufacturing

Given its implications for competitiveness, federal support for research on manufacturing systems has been very low (e.g., some $82 million was spent on programmable automation research in 1984, with the majority being defense-related). The NSF and National Bureau of Standards (NBS) programs in this field are good, but small. Emphasis on manufacturing systems research is growing, however, as evidenced by NSF's establishment of two Engineering Research Centers focusing on aspects of manufacturing.

The field lacks a strong professional tradition, a fact that hampers the movement toward research. There is only a limited science base. Professional communications for research in manufacturing have only recently been initiated. There are few educational programs in manufacturing engineering, and these often go by other names or are "hidden" within other programs. In comparison with Japanese and European manufacturing educational systems, the flow of new talent into U.S. manufacturing is inadequate in both quantity and quality.

Materials

Federal support for materials research has been roughly constant at about $1 billion per year, with the majority of this amount directed at the physical science aspects of materials. Additional funding for materials engineering is found within other engineering areas, especially at their interfaces, such as in the aerospace field and manufacturing.

The research support is spread across several agencies, and mechanisms for setting research priorities are neither well established nor consistent. The ratio of federal to industry funding is approximately 1:4—considerably smaller than the ratio of about 2:5 prevailing in other technical fields.

The best students are not entering the materials field, except in the area of electronic materials. Relatively few earn a Ph.D., even in high-demand areas such as ceramics (in which Ph.D. output is about 35 per year). Materials research is a highly interdisciplinary field, and one in which advanced research and teaching facilities such as clean rooms are relatively expensive and scarce.

Transportation

Except in the aerospace field, which benefits from substantial public-sector funding, transportation systems operate with far too little research input. Mission agencies involved in transportation devote less than 0.5 percent of their budget to research, compared with about 1 percent for other nondefense mission agencies. Highway research funding is relatively small, and almost no research is now conducted on guided ground (rail) passenger systems and marine transportation.

Although guideways and network control systems are seen as the government's responsibility, most of the vehicular components of transportation systems are traditionally viewed as a private-sector concern. Industry does conduct a substantial amount of applied research on certain vehicles (i.e., automobiles, trucks, and aircraft) and associated systems, but it is reluctant to support the intermodal and mid-and long-range engineering research that is needed to improve existing transportation systems and to create new ones.

Table 2 Relative Percentage of Expenditures, by Performer, on all R&D, Research, and Basic Research

Performer	R&D	Research	Basic Research
Industry	73	50	20
Federal government	13	16	13
Universities	8	25	50
Others[a]	6	9	17

[a] Includes state and local governments and nonprofit foundations.

SOURCE: National Science Foundation. National Patterns of Science and Technology Resources, 1984 (NSF 84–311).

Funding Issues

Support for R&D

It is appropriate to begin a discussion of funding for engineering research by examining the scope of funding for all R&D. The 1985 total national investment in R&D is estimated at $106.6 billion. Total federal support for R&D carried out in universities, federal laboratories, and by industry currently amounts to about $50 billion per year, or roughly 47 percent of the total. Industrial R&D expenditures surpassed those of the federal government in 1980, and now amount to about $52 billion (49 percent) (National Science Board, 1985). Table 2 shows the relative breakdown of funding, *by performer,* for R&D, all research, and basic research. Most (i.e., about two-thirds) of the research performed by universities is sponsored by the federal government.

Companies pay for roughly two-thirds of all R&D conducted in industrial laboratories; the federal government sponsors nearly all of the rest (National Science Board, 1985). Some 66 percent of the total national R&D outlay is spent for development. (In industry, 77 percent of R&D is spent on development.) Industry funding for basic research has increased in recent years, but still is only about 5 percent of the total industrial R&D expenditure (or about $2.7 billion in 1985). Industrial funding for applied research has risen sharply in amount and as a percentage of the national total, surpassing the federal outlay in 1980 and rising to 58 percent in 1985 (National Science Board, 1985). This shift reflects the administration's efforts to deemphasize near-term research not considered appropriate for federal investment.

Support for Engineering Research

In contrast to R&D funding, the Engineering Research Board found that it is extremely difficult to assemble consistent and comprehensive data on expenditures for engineering research per se. Most federal agencies and industrial researchers do not distinguish engineering research from other scientific research—indeed, in industry there are few data available to permit a distinction between funding for research and funding for development. Where such distinctions are made, they are not made consistently across or even within organizations. At the level of individual subsets of the field, the highly interdisciplinary nature of the research greatly complicates the picture.

The best available data are compiled by the NSF, which tabulates data on federal funding for various areas of science and engineering. Those data indicate that 1985 federal expenditures on engineering research were about $3.85 billion (National Science Foundation, 1984c). As Figure 3 shows, the total has remained nearly constant in recent years (the amounts have not been adjusted for inflation). Calculations by the board also demonstrate that funding for engineering research has kept pace closely with overall federal funding for research since the mid-1970s (Figure 4). Table 3 shows the levels of support provided by various federal agencies, and Table 4 shows the federal support for engineering research (both fundamental and applied) at universities and colleges.

As Figures 3 and 4 indicate, engineering research in general receives a relatively small proportion (about 25 percent) of federal research funds, compared with science research. Fundamental engineering research fares much worse, averaging around 5 percent of all research expenditures.

Part of this disparity results from the fact that science research is seen as clearly within the purview of government, whereas engineering research—including even that which is fundamental in nature—is viewed as bordering on development, and thus as a private-sector responsibility. Partly, too, the limited awareness among nonparticipants of the existence and role of engineering research, as described in the introduction, impedes support.

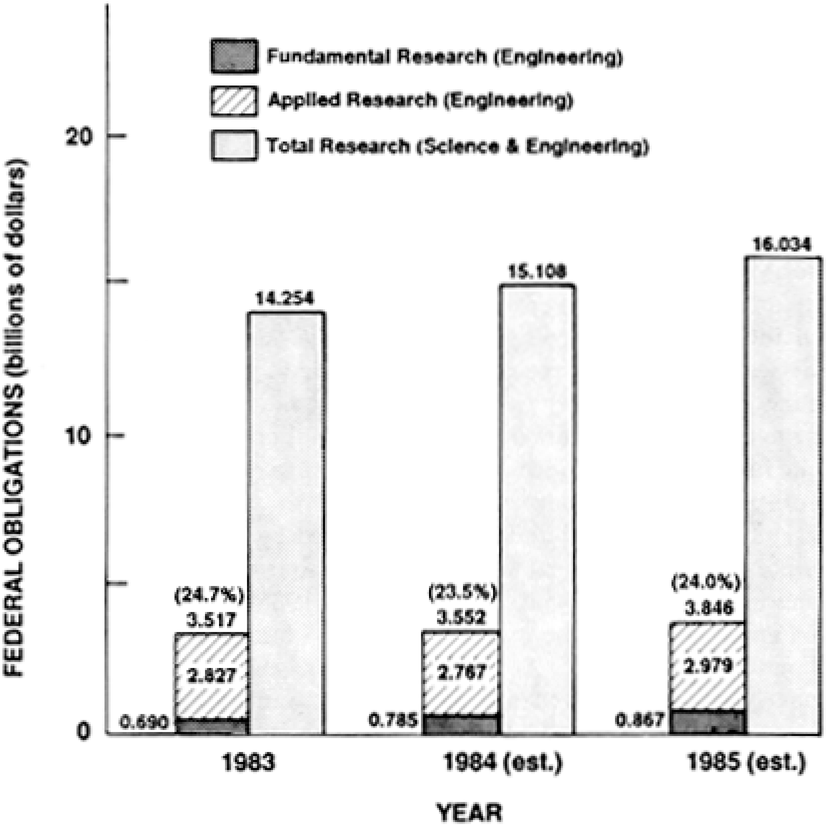


Figure 3
Engineering research funding (fundamental and applied) by the federal government has remained nearly constant in recent years, both in amount and as a percentage of total federal funding for research. (SOURCE: National Science Foundation, 1984c.)

It is imperative that the importance of engineering research in the exploitation of our national innovative capacity be recognized by the Congress and by the mission agencies concerned with technology development. Funding for R&D should be examined carefully to identify those points at which increased effort in engineering research can be applied to strengthen the knowledge base and leverage the overall federal effort in R&D.

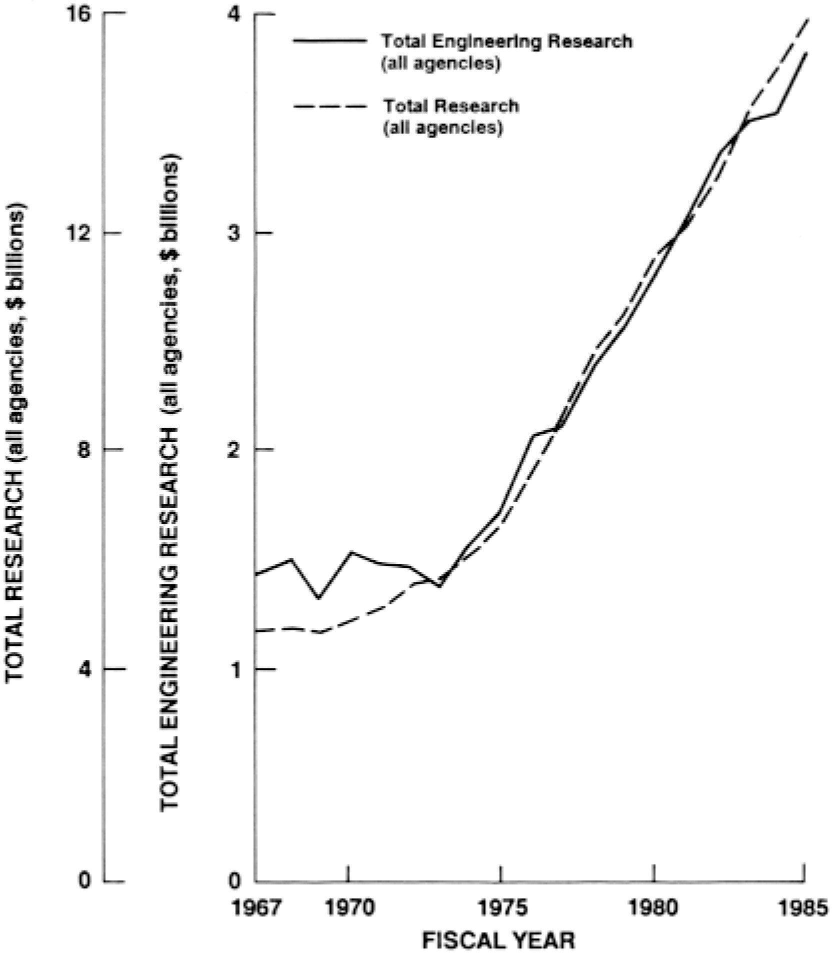


Figure 4
Federal funding for engineering research has accounted for about one-fourth of federal funding for all research over a period of many years. (SOURCE: National Science Foundation, 1984d.)

The nearly complete lack of data on industrial support of engineering research is troubling. It makes it difficult to ascertain whether adjustments might be needed in the philosophy that guides federal investment decisions. Traditionally, the federal government has restricted its involvement to "precompetitive" research, or to R&D in areas of essential public interest. However, questions of policy in the context of industrial competitiveness

Table 3 Federal Agency Funding ($millions) for Engineering Research (FY85, estimated)

Agency	Funding	Percentage of Funding
DOD[a]	1,650.9	42.9
NASA	1,006.3	26.2
DOE	479.5	12.4
NSF	203.6	5.3
NRC[b]	168.4	4.4
Interior	78.1	2.0
Transportation	57.5	1.5
Agriculture	55.0	1.4
NIH	45.4	1.2
EPA	41.7	1.1
Commerce	29.8	0.8
Other	29.3	0.8
Total	3,845.4	100.0

[a] This funding includes research that is strongly defenseoriented and applied in nature.

[b] Nuclear Regulatory Commission.

SOURCE: National Science Foundation (1984c).

Table 4 Federal Funding ($millions) for all Engineering Research at Universities and Colleges

Agency	Funding	Percentage of Total
DOD	169.0	39.3
NSF	121.8	28.3
NASA	43.8	10.2
DOE	37.1	8.6
NIH	27.0	6.3
Agriculture	9.8	2.3
NRC[a]	5.1	1.2
Other	16.4	3.8
Total	430.0	100.0

[a] National Research Council.

SOURCE: National Science Foundation (1984c).

require that information be available regarding the extent of industrial support for engineering research and for basic science research. For example, it might be possible to partly coordinate the programs of research in universities and federal laboratories with those in industry. As another example, the federal government might wish to consider supporting industrial cooperative research activities. Such information would also help us to better understand the impact of various kinds of research on innovation and on the effective commercialization of technology.

The board recommends that a study be made of U.S. industrial support for research—especially engineering research—to determine the amounts, the mechanisms, and the effectiveness of such research on an industry-by-industry basis.

The NSF's Role

The NSF is the federal agency responsible for supporting nondefense-oriented basic research. Support for engineering research is explicitly part of the NSF's charter, and as Figure 5 shows, it has carried out that responsibility well—particularly in its support of fundamental engineering research at universities in areas not covered by the mission agencies. However, the NSF's funding for engineering research has been much smaller, relative to all federal obligations for engineering research (NSF spent 4.7 percent of the total in 1984) than its relative funding for academic research in general (19.6 percent of the 1984 federal total) (National Science Foundation, 1984c).

That disparity appears to be changing. The NSF is giving much greater recognition to the role of engineering research. The budget of its Directorate for Engineering is expected to increase from $150 million in FY85 to $163 million in FY86. Still, the FY85 figure represents only about 0.3 percent of the total federal R&D obligation of $50 billion. Given the fact that the NSF is the primary sponsor of the long-range engineering research and (indirectly) the graduate education in engineering that are necessary to keep the United States ahead of its technological competitors, this is a miniscule amount. It is a role that is not fulfilled broadly enough or deliberately enough by the mission agencies.

A National Academy of Engineering committee estimates that, to meet pressing national needs for engineering research and human resources, the budget of the NSF's Directorate for Engineering should increase to around $400 million by FY90 (National Academy of Engineering, 1985). The board concurs generally with the assumptions on which this estimate is based, and applauds the NSF's effort to strengthen its commitment to engineering research.

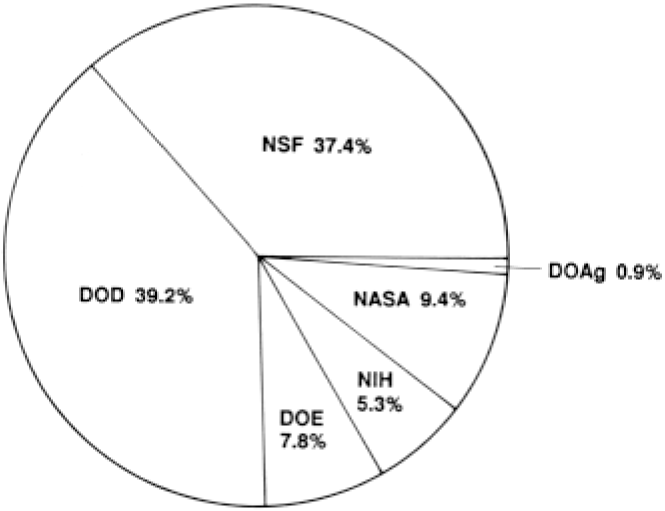
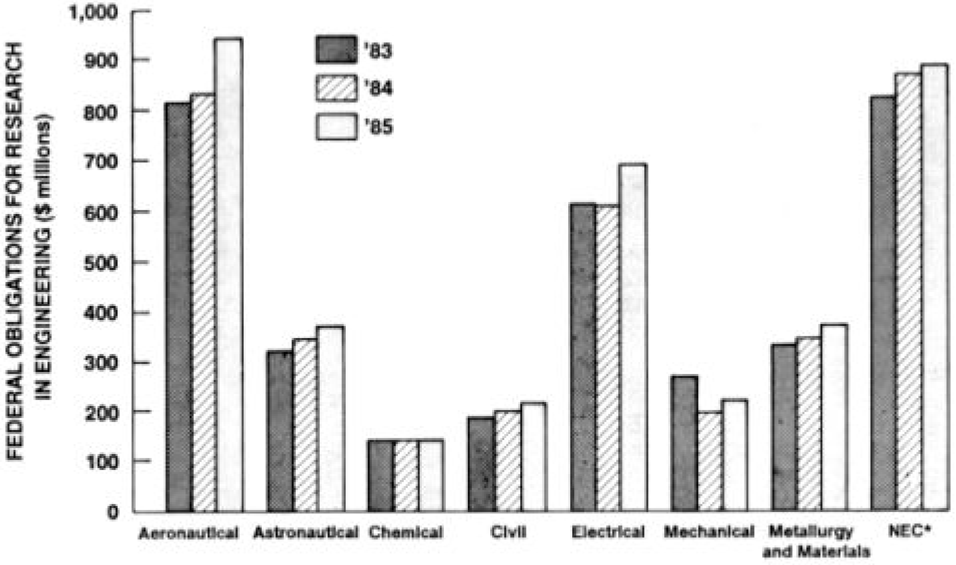


Figure 5
Federal support for fundamental engineering research at universities and colleges, by federal agency (total funding: $340.3 million, FY94, est.).
(SOURCE: National Science Foundation, 1984c.)

Factors Affecting Support

As Figure 6 attests, the degree of support for engineering research varies greatly across the subsets of engineering defined by the board. For example, the information, communications, computation, and control (IC^3) area (partly represented in the figure by electrical engineering) is relatively well funded, whereas funding for construction research (a subset of civil engineering) is extremely limited. Low funding in the biochemical engineering (biotechnology) area of bioengineering is reflected in the low funding for chemical engineering.

The ratio of federal to industry funding of engineering research also varies across fields. The Materials Systems Research Panel estimated that about 80 percent of materials research, for example, is funded by industry, compared with less than 60 percent in other



*Not Elsewhere Classified (inc. agricultural, industrial and management, nuclear, biomedical, systems, and ocean engineering)

Figure 6
Funding for research in traditional categories of engineering varies considerably from field to field. (SOURCE: National Science Foundation, 1984c.)

engineering fields. Whether a field is perceived as a private-or public-sector concern greatly affects its support, as does its current level of "fashionability."

Support can also vary within a field for a number of reasons, including those just mentioned. In the area of transportation, for example, engineering research in the aerospace field is consistently well funded; research on railroads, however, is very limited. As another example, structural design receives far better support than related areas of construction. Materials processing research is underfunded compared to materials characterization research.

One factor that undoubtedly affects funding for engineering research and graduate study is the relative lack of awareness on the part of the general public about the role and importance of engineering research. The public is generally aware of advances that occur in science; much of science appeals to the imagination in a direct way. Engineering research is more difficult to explain and present in a dramatic way, but it can be done. Recommendations for increased funding of engineering research would be greatly strengthened if they rested on a base of broad national acceptance. Organizations such as the NSF or the National Academy of Engineering, which have the resources to stimulate media interaction, should consider how this might best be accomplished.

Research Facilities

Engineering research is rapidly becoming far more complex. In some cases it is more specialized and in others it is larger in overall scope and scale than in the past. These changes have brought with them a corresponding increase in the sophistication of the equipment needed for modern research. For the most part, however, the nation's primary infrastructure for research—universities—does not reflect this new reality.

It is now widely recognized that university laboratory resources for engineering education and research are perilously inadequate. Simply put, these labs are too old, too small, and in some cases too dangerous. The useful life span of engineering laboratory equipment is currently said to be about 10 years (and decreasing). Yet the average age of lab equipment in engineering schools is estimated to be 20–30 years (National Research Council, 1985a). Only 18 percent of that equipment is said to be "state of the art";

25 percent of it is so obsolete it is not even used (National Science Foundation, 1984a).

Two factors are involved: First, we are now paying for the fact that buildings and equipment have not received adequate attention from university administrators or government funding agencies during the past two decades. Second, the explosion of knowledge in some engineering fields during the past decade has caused an accelerated obsolescence that was not anticipated. The enormous advance in sophistication of modern testing and measurement devices and instruments is accompanied by extraordinary increases in equipment costs. The cost of even small-scale, specialized facilities for engineering research quickly runs into the millions of dollars.

The cost and the urgency of the need are both greater in the fast-moving, high-tech areas of engineering such as computers, communications, electronic materials, materials processing, computer-assisted design/manufacturing, and robotics. Semiconductor processing research facilities, for example, cost $7–$8 million each; a composite processing laboratory for metals, ceramics, or polymers costs about $5 million. (It should be noted, however, that the costs of larger scale research facilities tend to be quite small in comparison to major facilities for science research. A synchrotron radiation facility, for example, costs between $70 and $160 million, depending on its size.)

Because adequate facilities are essential to both research and teaching, the lack of this equipment harms the knowledge base as well as the production of highly qualified new researchers. It also impedes the training of engineers who are familiar with cutting-edge research and with the use of modern industrial tools. The movement toward sharing facilities, either on a national/regional or local basis, is a positive one. The trend toward cluster facilities, whether they are supercomputers or surface science laboratories, ought to continue. Recent gifts from industry to universities for use in upgrading equipment have also made significant inroads on the problem.

Yet the problem is still very serious, and more needs to be done. *Gifts of laboratory equipment to engineering schools can be facilitated through legislation affecting taxes at the state and federal levels* . It may require a concerted effort on the part of industry, schools, and professional societies to bring about such changes in the tax laws. In addition, funds are sorely needed to operate and maintain the donated equipment. In the case of buildings, federal and industry funds, matching state funds, and private gifts could all be applied. *Finally, we concur with the finding of the National Academy of Engineering that it would be appropriate for the NSF's Directorate for Engineering to include in its budget substantially greater funding (i.e., at least $30 million per year) for research equipment and instrumentation (National Academy of Engineering, 1985).*

Adequacy of Personnel Resources

Perhaps the single most critical factor in the health of engineering research is the availability of qualified and highly motivated researchers. From the standpoint of U.S. industrial competitiveness, the availability of practicing engineers able to "translate" research advances into innovative applications is equally important. Graduate education is essential for both of these resources. The output of new Ph.D.s, the availability of faculty to train them, and the quality of existing research talent in these fast-changing fields of engineering are all important elements in determining whether a subset of the field is able to progress rapidly and to retain a strong competitiveness with respect to the engineering efforts of other nations.

Ph.D. Production

As Figure 7 shows, the output of doctoral degrees in engineering, which had decreased sharply beginning in 1973, began to increase again in 1979. By 1983 it had passed 3,000, and in 1985 it reached an estimated 3,400 (National Research Council, 1985b). This is substantially fewer Ph.D.s than were graduating annually at the end of the 1960s. Although it is more than twice the number of engineering Ph.D.s granted in Japan (1,290 in 1984), it is nevertheless a far smaller proportion of all doctoral degrees granted (9.3 percent of all U.S. Ph.D.s in 1984, compared with 17.8 percent in Japan) (National Science Foundation, 1986).

Given the emergence of new fields and the general increase in the research-intensiveness of engineering since that time, the current output of doctoral-level engineers in the United States probably represents an overall shortage of new researchers and engineering faculty, as well as of highly educated practitioners who are well equipped to carry the results of engineering research into practice.

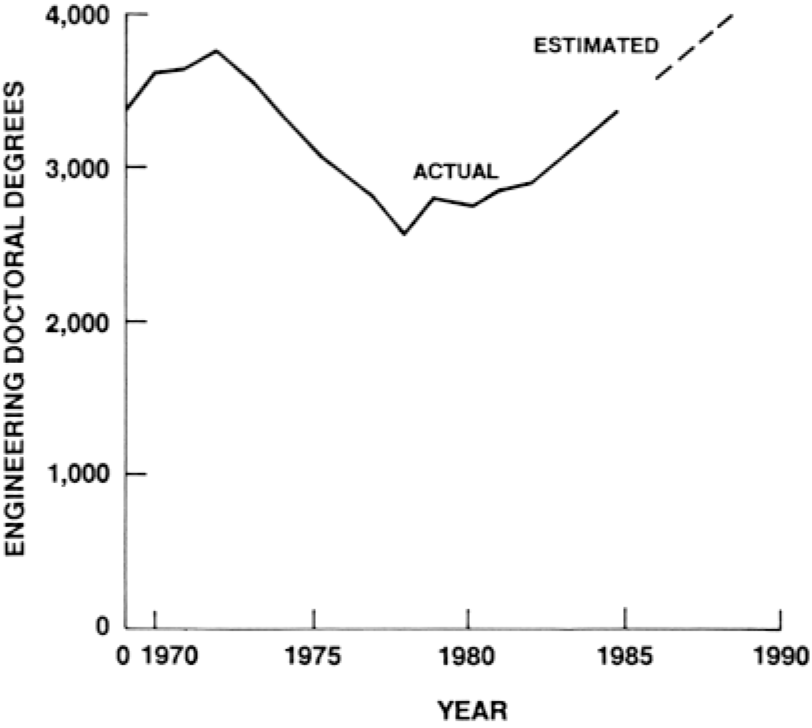


Figure 7
Actual and estimated engineering doctoral degrees per year.
(SOURCE: National Research Council, 1985b.)

In the case of funding, however, the picture varies from field to field. Indeed, funding for research is a major factor in determining the number of doctoral candidates. High funding tends to be associated with rapid growth in the corresponding industry, sparking interest among undergraduates and a large output of B.S. graduates into lucrative industry jobs. Paradoxically, the supply of Ph.D.s may be particularly inadequate in such areas; although there are higher absolute numbers, the demand is proportionately even greater. The IC^3 area and the currently "hot" areas of materials (electronic materials and ceramics) are examples.

By the same token, the supply of Ph.D.s in underfunded fields may exceed the demand. For example, in construction there appear to be adequate numbers of doctoral candidates at present, due to the shortage of jobs in civil engineering and limited funding for research. Doctoral study may become a "holding pattern" under such circumstances.

Limited research funding probably has a more straightforward effect in fields such as biotechnology and manufacturing, where there are severe shortages of Ph.D. researchers. About one-third of the biotechnology firms surveyed in 1984 by the Office of Technology Assessment reported shortages of Ph.D.s. Some 20 departments of chemical engineering that have biotechnology programs graduate fewer than 60 M.S.s and Ph.D.s combined; the annual need nationally is estimated to be 2–3 times that large. Ph.D. production is also declining in the areas of energy, mineral resources, and the environment as overall funding drops.

Newly trained researchers are the lifeblood of contemporary American technology. *It is essential that we encourage more of the best engineering students to continue into doctoral study*. This is just as important in areas that are presently little emphasized as it is in the growing high-tech fields. Without a strong capability to conduct energy research, for example, the nation will be much more vulnerable to a future energy crisis.

Doctoral study must first be made more attractive. The board endorses the recommendation that is now frequently made, that doctoral stipends should be increased to at least one-half the starting salary of a B.S. engineer in the relevant industry. Progress toward this goal is already occurring in the form of industry fellowships for engineering doctoral candidates—as many as 200 per year—that carry stipends of $14,000 plus a departmental grant, with no repayment requirement. In addition, the use of "forgive-able loans" from industry is an excellent idea. An example is the General Electric Company's program, in which loans of up to $5,000 are made to Ph.D. candidates and are forgiven if on graduation the student pursues an academic career for at least 4 years.

One factor that is frequently cited as a problem relating to graduate study is the growing prevalence of foreign-born students on temporary visas. Such students now account for well over 40 percent of all engineering Ph.D. candidates at American universities (National Research Council, 1985a). (The proportion is even higher in certain high-demand areas, such as manufacturing [59 percent] and microelectronics [52 percent]—Holmstrom and Petrovich, 1985.) However, these individuals have been extremely valuable both on engineering faculties and in industry. They now represent some 25 percent of all junior faculty in engineering, and they contribute their substantial talents toward the competitiveness of U.S. industries—especially in the most advanced areas of R&D. To discourage them from staying in this country (or, even more so, from coming in) would work to the disadvantage of the United States. *Nevertheless, the greatest emphasis must be placed on attracting a far larger fraction of our best domestic engineering students into Ph.D. programs.*

Faculty

With record high enrollments in most engineering disciplines over the past decade, the "faculty shortage" has been a continuing problem. The board finds that it is an especially severe problem in IC^3, because of very high enrollments and high industry demand for researchers. In bioengineering, manufacturing, and some areas of materials research, faculty are also in short supply and will remain so for some time, even with appropriate incentives, because these fields (or their advanced applications) are fairly new. Figure 8 depicts the results of one recent survey of recruitment in these fields; one-half of the respondents reported unfilled faculty positions. Yet in fields that are presently feeling the pinch of reduced funding (e.g., transportation and environmental engineering), some faculty members are shifting their research and teaching to other fields.

In fast-changing fields obsolescence is also a problem. A high-level official of the NSF told the board that as many as 30–40 percent of all engineering professors nationwide are not up to date; yet these are the faculty who teach most of the undergraduate engineering students.

POLICY ISSUES REGARDING SUPPORT OF ENGINEERING RESEARCH

The Engineering Research Board believes that strong, internationally competitive engineering research capabilities are essential to the United States' domestic and international strength. In our judgment, U.S. engineering research in most fields is still at or near the forefront by worldwide standards, but is not nearly the dominant force it was from after World War II to about 1970. Capable foreign competitors now are challenging U.S. strength in many engineering fields, notably in bioengineering, materials

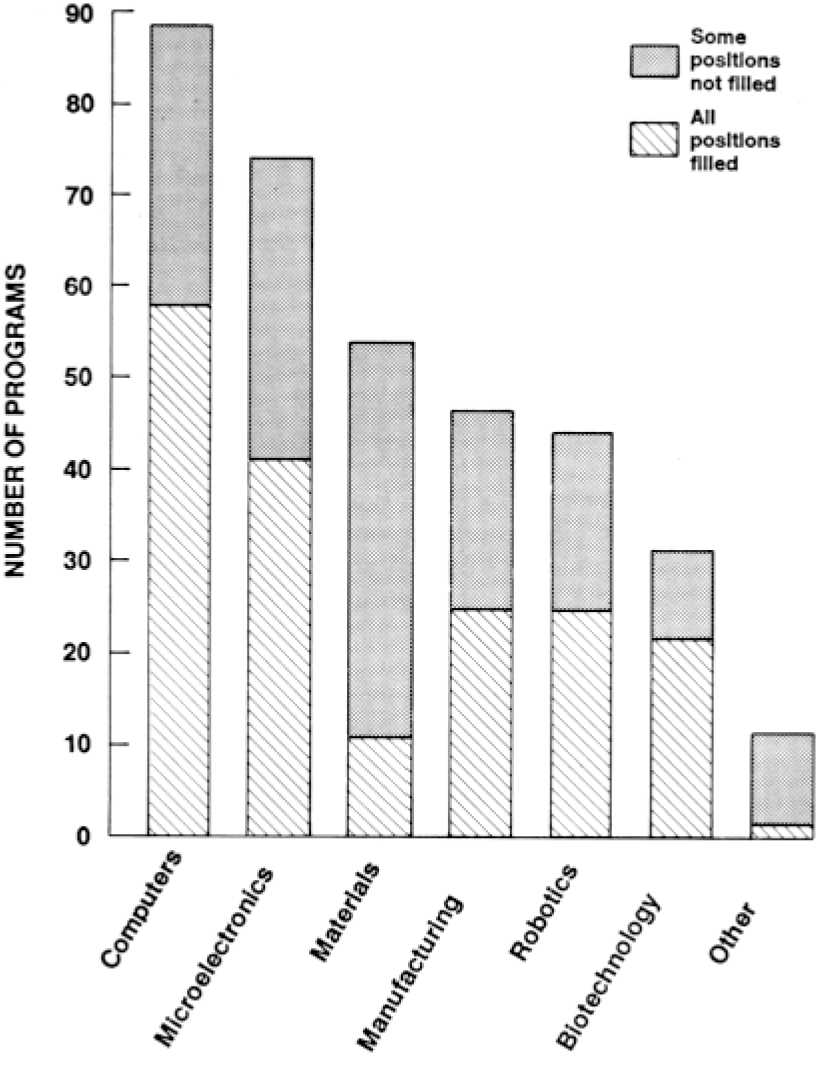


Figure 8
Of programs in emerging engineering areas recruiting faculty in 1983–1984, roughly half of those surveyed were able to fill all available positions. (SOURCE: Holmstrom and Petrovich, 1985.)

engineering, manufacturing engineering, transportation systems, construction, and many areas of communications, computation, and control systems engineering. Most daunting is the growing speed and success with which Japanese industry translates engineering research advances (from their own laboratories or those of others) into high-quality manufactured products.

Policy changes and other actions by the U.S. government, industry, and universities are needed to strengthen engineering research, particularly in emerging and interdisciplinary fields, and to improve our record in transferring the fruits of research to our own industries.

We sense that there is now a consensus in government, industry, and universities that the challenges described previously can be better met by expanding the scope of cooperative activity among these three parties and by improving the linkage between engineering research and practice. Accordingly, we also discuss this topic and make a number of recommendations aimed at fostering greater interactions among these sectors.

Federal Government Policies

Basis for Federal Support of Research

Over the past three decades the nation's commitment to the support of scientific and engineering research has provided extraordinary benefits to society and, in the process, has established the United States as a world leader in science and technology. The government's involvement in research derives from its responsibility for national security and from its obligation to provide for the general health and welfare of its citizens.

Government has assumed its active role because the scale of research in emerging scientific and technological fields is often too large for private companies to undertake; and in general there is too little incentive for industry to support long-range research. Therefore, the great majority—about two thirds—of the nation's basic or fundamental ("precompetitive") research is supported by the federal government. In addition, areas of general public health and welfare such as safety, medicine, defense, public transportation, and environmental quality are not natural targets of research by industry. Thus, the government has also become strongly identified with support of these types of research at all levels.

Most of this federal support is directed at universities. Although universities perform only some 25 percent of all research in the United States, they perform about half of the basic research (see Table 2) (National Science Foundation, 1984b). This research is generally long term, innovative, diverse, relatively inexpensive, and open to all. A very valuable by-product of university research is the training of graduate students for research and teaching, as well as for applying research results to practice within industry. Furthermore, universities foster a much closer interaction between science and engineering than is likely to occur in industry, with its lesser emphasis on fundamental research. This interaction mutually stimulates science and engineering, and is beneficial for the nation as a whole.

Thus, there are compelling reasons why the federal government must continue to support research at universities and elsewhere. Earlier sections of the report have given reasons why engineering research must be an important and growing component of this support. The following discussion points out several specific areas in which changes in policies, practices, and attitudes will make federal support of engineering research more effective and successful.

Need for Stability

This issue of stability is raised first for a reason: Nearly every panel report submitted to the board identified it as among the most serious—if not *the* most serious—difficulties that engineering research faces. The problem is that the federal government's support of engineering research often varies greatly within a given field across relatively short periods of time. New issues, sudden crises, and changing expectations alter the research priorities of federal agencies and bring about erratic changes in emphasis. Mission agencies are the most subject to the shifting political winds.

Although the engineering research community has long recognized the magnitude of this problem, it has tended to believe that it was simply unsolvable —that political realities would inevitably dominate—and to hope that things would somehow work out. Yet the lack of continuity in programs continues to result in an enormous waste of national resources, energy, and productivity. University research programs and planning are regularly devastated by the "on-off" control that has been applied to their funding in specific areas. The board believes that we must somehow find a way to graduate from the crisis management that has characterized so many of the decisions regarding support of university engineering research.

The crises that frequently stimulate research may be so compelling that they cannot be ignored. Indeed, agencies should be alert and ready to accommodate new thrusts. *However, such "quick-response" initiatives should be undertaken as add-ons to continuing and stable support of both basic and applied research.* One possible mechanism would be to have a set-aside reserved for this purpose.

Long-term research needs in these areas might be better served if the NSF were to take a greater role in those basic and exploratory research programs that the mission agencies do not see as part of their objectives. The mission agencies, for their part, need to recognize that the training of researchers over the long term is not exclusively the province of the NSF; each agency also has a responsibility here. *Therefore, another very useful step would be for mission agencies to allocate, on a multiyear basis, a fixed percentage of their budget to university engineering research in fields appropriate to their mission.*

These measures should not be construed as preventing the termination of lines of research that ultimately prove unfruitful. However, such shifts should be effected on a gradual and deliberate schedule to permit an orderly redeployment of the researcher population affected.

Need for Better Coordination

Engineering research is highly interdisciplinary. In addition, because it is linked to economic markets, public demand, and political pressures, the nature and objectives of research change fairly rapidly—many fields are quite new. In addition, federal mission agencies tend to operate independent of one another, with a focus on their own near-term development objectives. For these and other reasons, there is a prevailing lack of coordination with regard to federal spending in many areas of engineering research.

Support for research is often spread across several federal agencies and within otherwise-unrelated programs within each of those agencies. Given the differing mission perspectives on that research, this decentralization is appropriate and necessary. However, needed areas of research are missed; there is inevitably also some wasteful duplication of effort, coupled with a loss of opportunities to achieve greater progress through the synergy of combined strategies. The net effect is that findings are often not efficiently assembled, analyzed, or put into practice. The bioengineering and materials fields in particular are strongly affected by this lack of priorities and cooperation.

More coordination would benefit all the engineering research fields, and thus the nation's overall technology development effort. Mechanisms for setting priorities should be established on both the interagency level and within agencies. This will require the establishment of interagency coordinating committees, perhaps at the Office of Science and Technology Policy. The Committee on Materials of the Federal Coordinating Council for Science, Engineering and Technology is one example of the kind of interagency coordinating group that can be helpful, although this group has not had enough overall impact. NSF's recently formed Office of Biotechnology Coordination is an example of the kind of activity that other agencies could initiate internally to address cross-cutting new areas within their purview.

Mission Orientation and Overmanagement

Because of the increased competition for limited research resources nationally, government agencies involved in supporting engineering research have begun to shift from a philosophy in which research grants are seen as instruments for investing in future needs and research talent, to one in which grants are a means of procuring an identified product or of solving an immediate problem. As a result, well-defined, "theme-oriented" basic research projects (perhaps more properly termed engineering development) are beginning to crowd out unsolicited basic research of broad applicability. Lost in this change in orientation are many of the imaginative ideas that can solve tomorrow's problems; thus, the real loser is the long-term health of R&D itself.

The board agrees with the need to focus on practical results. The needs of the domestic economy and our international competitiveness demand this emphasis. Nevertheless, there is a clear need for fundamental engineering research to add to the knowledge base in every field of engineering. *Within agency research budgets,* *there must be ample room for high-risk, long-range research as well as the more immediate, product-oriented research.* Ways to ensure this balance might include

- earmarking and protecting a sufficient pool of funds for fundamental research from which no immediate products are expected; and
- setting aside awards, based mainly on a junior or senior investigator's past performance, that provide opportunities for more speculative research on an adequate scale.

Related to the increasingly product-oriented, near-term nature of agency-sponsored research, but on a more individual level, is the growing tendency of mission sponsors to "micromanage" engineering research projects. The problem is most noticeable at universities, but industry researchers also report having encountered it in large government-funded development projects. This practice of overdirection reduces the chance of success and should be avoided.

The board believes in accountability. *In general, the direction and methods of a particular research project are better defined by the researchers involved, rather than by detailed planning at the funding agency. Changes in direction should be allowed at the discretion of the researcher.* If overall reviews after a reasonable period of time (often 3 years or more) indicate a lack of progress or loss of focus, then funding should be gradually phased out while allowing graduate students to finish their thesis work. University research that has researcher training as a major goal is not equipped to deal with sudden major shifts in direction. A 5-year project supporting 2 students can be much more cost effective in educational terms than a 1-year project supporting 10.

Importance of the Individual Project Grant

Over the past few years the NSF has begun to emphasize the sponsorship of projects that feature partnerships between universities and industry. This trend is evident in, for example, the Engineering Research Centers (ERC) program. Because such centers provide a solid basis for the type of systems-oriented research recommended in this report, the ERC concept merits the board's strong approval.

Nevertheless, the board is concerned about the continuity of funding for new, innovative research investigations in the traditional disciplines, whose scale is modest by ERC standards and that generally involve the efforts of individual investigators and just one or only a few graduate students. The very nature of engineering research is such that many long-range advances have been made only through the vision of individuals who are not allied with the mainstream of the industrial process or the current conventional wisdom. This type of research is a key to the health of the overall engineering research environment, and it is not likely to be sustained by "trickle-down" support filtering through the large, heavily funded activities. *Consequently, the board urges that the general scheme of NSF sponsorship should continue to provide a major explicit emphasis on encouraging the individual engineering researcher, in balance with the new thrusts emphasizing cross-disciplinary research.*

Role of the Federal and National Laboratories

About one-third of all federal R&D expenditures are made either in government or in government-supported laboratories (NSF, 1984c). These facilities include the federal (in-house) laboratories, such as those operated by NASA, DOE, and DOD; and the national (federally funded) laboratories, such as the Los Alamos National Laboratory, the Sandia National Laboratories, the Jet Propulsion Laboratory, and the Lincoln Laboratory. Not only is much valuable work done by the labs themselves, but universities and (lately) industry have benefited considerably from access to the state-of-the-art equipment and expertise resident within them. The federal and national labs have unique expertise in the development of sophisticated instrumentation.

The role of the federal and national labs in carrying out part of the nation's R&D effort is an important one, and one that has generated controversy in recent years (see, for example, Office of Science and Technology Policy, 1983). The board did not attempt to address this subject in any detail, because of its scope and complexity. *However, the board believes that the government needs to clarify the relative research roles of the universities and the federal and national laboratories to ensure that the division of effort between them is reasonable. The issue does require further study, and guidelines to this end should be developed.* Universities, in addition to producing fundamental research ideas and results, also produce the new research engineers and scientists needed to maintain a strong national research establishment and national economy. Thus, their role is unique and indispensable. For this reason, the board would urge Congress to avoid protecting the federal and national laboratories at the expense of university research during budget reductions in a given area. University research has little defense against the political pressure that a concentrated, large-scale research effort in a single national laboratory can bring to bear.

The DOD: Policies Toward Research

The DOD has been a dominant source of support for basic and applied research since World War II. Through offices of research at the departmental level and in each service, the Defense Advanced Research Projects Agency, and other service laboratories, the DOD has pursued advances in many of the most exciting technologies of our time. Electronics, communications, manufacturing, materials, and aerospace systems could not have come to their present levels of development without this support.

In recent years, short-term defense requirements had begun to dominate the defense research agenda. However, the most recent budgetary allocations indicate a renewed realization (on the part of Congress as well as the DOD) of the need to enhance fundamental engineering research as an essential element of our national security. Between 1980 and 1985, DOD support for fundamental research in universities grew at an average annual rate of 10.5 percent. University performance of basic research accounted for half of all DOD's basic research in 1985, up from 34 percent in 1976 (National Science Board, 1985). Current DOD plans call for further substantial support of high-risk basic research, multidisciplinary centers, research equipment, and research fellowships. The trend toward greater involvement by DOD in basic research—particularly on university campuses—has been controversial. However, the board welcomes this enlightened outlook and support, and hopes it will continue.

There is concern that attempts within DOD to restrict the dissemination of unclassified research might not only impede communication among U.S. researchers, but would in effect exclude universities from participating in research. This would seriously harm both engineering research as well as the relationship between DOD and the universities. *Clearly understood and implementable guidelines must be established and adhered to by all concerned if we are to ensure that research expenditures benefit from broad intellectual input and produce trained researchers while satisfying the legitimate needs of national security*.

Encouragement of Research in Industry

Although universities have the intellectual resources, the environment, and the incentives to lead in fundamental engineering research, a great many of today's breakthroughs in that research are occurring in industrial laboratories. Abundant equipment and other resources are part of the reason; federal policies have also played a major role in facilitating industry research. For example:

- Most of the major DOD contractors use Independent Research and Development funds to pursue high-risk research on their own initiative.
- The congressional tax acts have provided a number of tax incentives to promote industry R&D. Perhaps 10 percent of the increased industrial R&D funds are used to conduct fundamental engineering research.
- Antitrust restrictions have been eased, permitting companies to form cooperative research consortia in different fields.
- Direct contractual support has been increasingly extended.

The federal government is able to leverage its fiscal resources very effectively by means such as these, and should continue to do so. The current congressional tax acts should be extended, and should encourage gifts of cash and equipment to universities even more vigorously than in the past.

Because industrial development in certain fields (especially electronics and biotechnology) is producing and will continue to produce a vast array of new products and techniques, attention is needed to the question of patent and property rights. Innovations in software, for instance, are already giving rise to major alterations in the concept of intellectual property. The lack of adequate policy in this area may retard industrial research in some fields.

University Policies

The foregoing discussion makes it clear that universities play a crucial role both in performing engineering research and in educating new generations of researchers as well as practitioners able to apply the results of that research. For that reason, university policies and practices can have a great impact—positive as well as negative—on the health of engineering research and graduate education. Four areas of special importance are

1. encouraging faculty flexibility;
2. removing impediments to cross-disciplinary research and education;
3. maximizing the use of available research facilities; and
4. establishing policies on graduate study.

Encouraging Faculty Flexibility

Junior Faculty

Universities have had difficulty filling vacant engineering faculty positions, particularly when they have sought specialists in emerging new areas or with particular interdisciplinary competences. Though industrial research laboratories frequently and successfully employ new Ph.D. graduates for work quite far from their thesis topic, the common pattern at universities for new faculty appointed shortly after completing their Ph.D. work is to continue along the same path they pursued in their thesis research. Otherwise they find it very difficult to secure outside research funding. Some campuses have successfully alleviated this problem by providing new assistant professors with the resources and encouragement necessary for them to develop specific new areas for their research.

To succeed in such redirection, universities (with the help of government and industry) must find a way to provide support for research initiation . This must include research funds, a reduced teaching load, aid in developing needed personal interactions with researchers in the new field, and a fair and clear standard for advancement and promotion to tenure. Federal programs for research support award funds based on demonstrated competence in the proposed research area, as evaluated by peer review. New faculty need a few years to develop a new research area before they can expect to receive research support in that area from these traditional sources.

Senior Faculty

Universities should also do more to encourage senior faculty to develop new areas of research expertise as their established lines of research become less relevant to current needs. A faculty member well established in research is strongly tempted to continue working in one area through a full 30-to 40-year career, if possible. Given the rapid rate of change in engineering technologies, this is not a workable approach. Changes in a university professor's research emphasis should occur on a much shorter time scale. Industrial leaves, permitting senior faculty members to spend a year or two in industry to get started in a new research area, can be very effective. A full-year sabbatical leave at another, carefully chosen university also can be effective. A program of fellowships for senior faculty specifically aimed at research redirection could be an effective complement to industrial and university sabbatical leaves. Faculty salary policies can offer an effective incentive if significant rewards are permitted to accrue to those who are successful in developing productive research and teaching in new technical areas.

Cross-Disciplinary Research and Education

Every panel represented within the Engineering Research Board's scope of study is profoundly cross-disciplinary in nature. Indeed, engineering systems research in all areas with economic and technological importance cuts across the established disciplinary boundaries. Industry must and does operate in a cross-disciplinary systems mode, from applied research to development to design and production. Engineering students therefore should be educated to perform well in the cross-disciplinary mode within a systems environment. This requirement in turn calls for those who teach them to understand and (on occasion, at least) to participate in group efforts that cut across disciplinary lines.

Universities have been criticized for resisting integration of their engineering specialities into a whole that should serve both themselves and their clients (government as well as industry) better than current alignments do. Part of the problem is that cross-disciplinary research is not easily encompassed within the traditional reward system for university faculty, or within the academic department structure. Faculty who affiliate with a cross-disciplinary activity outside the departments have no natural constituency within the departmental structure that controls promotion and tenure. When young faculty members participate in research activities that are viewed as not being "intellectually tough," their publication record in these areas is frequently discounted. Thus, it is important for them to have another major suit.

One solution is for untenured faculty to have joint appointments in the traditional discipline and the new activity. There are limitations to this approach, however, because the individual has to do "double duty" in terms of departmental citizenship; and there is a constant risk of diluting faculty research output by dividing it between the two activities.

Probably the best solution is to maintain such high standards in the interdisciplinary programs that they are above reasonable criticism by the faculty. At the same time, the program participants should strive to create a better sense of understanding among the nonparticipating faculty regarding the mission and goals of the activity. *Fellowships specifically targeted to encourage Ph.D. graduates in one discipline to do postdoctoral research in another would facilitate communication among disciplines and "seed" the faculty with individuals who are experienced in the cross-disciplinary approach. Such fellowships, extended by industry and government, should carry stipends equal to those of beginning assistant professors of engineering*. Normal postdoctoral appointments, with their modest stipends, attract ample numbers of science Ph.D.s but almost no domestic engineering Ph.D.s.

The problems associated with cross-disciplinary research and education must not be downplayed. Optimal tuning of what might be called the specialist/generalist axis is still—especially in the university—a highly nonlinear endeavor. The integration of talent that has often worked so well in industry task forces has worked because there was something to integrate in the first place. In university research the correct balance is equally important, but perhaps harder to discern. It must in any case ensure that students receive a thorough grounding in the fundamentals of specific disciplines.

The basic exposure now offered in rigorous undergraduate engineering curricula will continue to serve the nation's needs in the future. Indeed, if we omit these basic studies we will soon encounter a new kind of crisis in engineering education.

What is needed is more exposure in the curriculum to the application of these skills to compound and cross-disciplinary problems. This will happen only if the members of the faculty acquaint each other with problems requiring multidisciplinary approaches. Then, as students progress through their necessarily somewhat specialized curricula, they can be exposed to more comprehensive problems and issues. A valuable by-product of that exposure will be a more flexible national pool of engineering researchers and practitioners who are able to move within and across fields to meet the nation's changing technological needs.

The board believes that it is much too early to tell whether the results of disciplinary engineering research or of cross-disciplinary research will have the greater impact on future engineering practice. Moreover, we believe that there is no need to resolve the question—if indeed resolution in the abstract is possible. Both modes are likely to contribute substantially to the future economic well-being and industrial competitiveness of our nation. In addition, both modes are investments in the future with a guarantee of substantial economic return in the aggregate, despite the uncertainty of success of any single engineering research program.

It is for this reason that we urge more cross-disciplinary research with a systems orientation, through such vehicles as NSF's ERCs, because so little fundamental engineering research at universities is now done in that way. We also urge continued attention to and support of those engineering researchers who prefer to pursue high-quality work in a single discipline as individual investigators or in very small groups. They have in the past and will in the future make significant contributions to the knowledge base on which industry will build.

Maximizing the Use of Facilities

Meaningful engineering research and effective education of doctoral-level students require progressively more sophisticated and expensive equipment, facilities, and support staff. The need to expose a large number of graduate engineering students to the advanced technology they will encounter in industry means that first-rate facilities should be available at many schools across the nation. A handful of the largest engineering colleges have kept current in selected research areas, but at the cost of substantial fund-raising efforts by faculty and alumni.

However, as described in the section "Issues that Determine the Health of Engineering Research," most engineering colleges have been unable to remain up to date in research facilities and instrumentation, or in providing the support staff to maintain and operate costly experimental facilities. *Costs are so high that a majority of engineering colleges with graduate programs will have to rely on shared facilities and equipment for a portion of their experimental research*. Examples are already evident: the National Research and Resource Facility for Sub-micron Structures at Cornell University, NSF's newly established ERCs, and the four new supercomputer centers encourage participation by researchers from many institutions. Collaborations between universities and industry, and universities and government laboratories, are also very useful means of sharing access to costly research facilities, and should be actively pursued.

We welcome the trend toward broader access to these scarce resources. However, successful conduct of research in an environment of shared facilities will require more collaboration between senior researchers than has been common in engineering in the past. *University policies must be modified to support, evaluate, and reward success in collaborative research*. The fact that other successful fields of university research, such as high-energy physics and astronomy, have out of necessity operated with shared facilities for years gives hope that engineering research also can succeed in this mode.

Graduate programs in engineering are expensive to operate. Because of the need to educate future practitioners in research methodologies, these programs should be considered more akin to medical science programs (as contrasted to programs in the physical and natural sciences) in terms of their need for equipment and facilities. *To provide more funds, university equipment and facilities should be formally depreciated over lifetimes comparable to those used by industry. Contrary to widespread university practice, depreciation charges should be allowed as a normal operating expense and should accrue toward renovation and replacement of equipment and facilities*. Of course, in most cases this will require the approval of the sponsor.

Policies Toward Graduate Study

Attracting High-Quality Students

University policies and practices concerning graduate students must be modified to induce more of the nation's most able engineering undergraduates to continue into M.S. and Ph.D. programs. As we recommended in the section "Issues that Determine the Health of Engineering Research," supporting stipends for graduate students need to be at least half the engineering salary offered by industry to graduates with B.S. degrees. Some fields, such as materials and manufacturing, may need to offer especially attractive fellowships or assistantships in order to attract the numbers of high-quality students they seek. Students are also strongly discouraged from pursuing doctoral studies if facilities and equipment available for their use are below industry standards.

New Programs

Given the changing nature of technology and of industry's demand for engineering researchers, it is difficult for academia to keep up. The development of high-quality graduate research programs takes considerable time and effort. The relative scarcity of programs in biotechnology and manufacturing, for example, has been noted. Universities are of necessity conservative institutions—they cannot afford imprudent change. Having seen the decline of student interest in programs that were once fashionable (recent examples would include environmental and nuclear engineering), they are reluctant to innovate quickly.

This conservatism is much assuaged, however, by tangible support. Industry offers to support the establishment of needed new programs would be a strong inducement to universities. One suggested mechanism would be the use of matching-grant programs at either the state or federal level, with the government matching industry funds provided for this purpose.

The board believes that, for new programs to be most effective, they should generally be targeted at particular fields. Given the time and resources required to establish high-quality, broad-based programs, it is unlikely that such programs will be able to compete with established programs for full-time graduate students. Without an adequate supply of full-time students it is difficult to develop a strong, broad-based research program.

Universities often feel pressure from industry to offer part-time graduate study programs. However, the university community believes that whereas part-time programs for the master's degree may be acceptable, part-time doctoral study is in no way equivalent to a high-quality, full-time Ph.D. program and cannot be relied on to produce first-class research personnel. Full-time cooperative programs with industry do have promise and should be developed further.

Policy Issues for Industry

Increased Support of Fundamental Research

Industry performs about half of all science and engineering research carried out in the United States, but only about 15–20 percent of the basic research (National Science Foundation, 1984a). Basic research accounts for just 5 percent of all industry R&D expenditures (National Science Board, 1985). It is appropriate that industry should devote most of its effort to relatively near-term research and product development; this is to be expected. *However, in the interest of its long-term health and competitiveness, particularly on the international scene, industry should give greater attention to fundamental engineering research, both in-house and at universities*.

In the manufacturing industries, the trend toward moving "offshore" with production may tend to deflect attention away from fundamental engineering research that could improve competitiveness over the long term. In other industries (e.g., construction, shipping, and railroads) there is little support for near-term research and virtually no long-term research. It is obvious that research must compete with other priorities, only beginning with short-term pressures on the bottom line. However, enlightened managers must come to realize that an appropriate emphasis on engineering research is in the long-term best interest of any technology-based company.

In the dual interest of increasing fundamental engineering research and improving the supply of engineering talent, industry should substantially increase its interactions with universities. These interactions can take several forms:

- contracting for basic research;

- increasing equipment donations (including funds for its operation and maintenance);
- providing matching funds for "bricks and mortar";
- offering consulting contracts to faculty and summer jobs to students; and
- arranging personnel exchanges and encouraging joint research.

More of this kind of interaction would be highly beneficial, as it would help to close the existing gap between engineering research and practice. It is not only support in the form of funds and equipment that is important; the personal involvement of graduate students and faculty with their industry counterparts is also extremely valuable. Management support for such interactions is essential.

Responsibility for graduate research education rests largely with those universities having strong research programs. The interaction of graduate students with research faculty is essential and provides the best possible training environment. NSF and the federal mission agencies have heretofore been the primary supporters of graduate education. Now, industry is being increasingly drawn in. In addition to the measures noted previously, innovative programs such as the ERCs and the Presidential Young Investigator Awards are attracting industry sponsorship. Faculty fellowships of various kinds, sponsorship of doctoral students, and other such activities also deserve the full support of industry.

Professional Development

In addition to academic researchers, the national pool of research talent also includes large numbers of experienced researchers in industry. These individuals are a valuable resource that must be conserved and nurtured.

There are two primary mechanisms by which this resource can be efficiently used. *First, industry managers should ensure that the company is making optimum use of its engineering research talent. For example, it is important to subject the research program to periodic review so that unproductive lines of research can be weeded out*. In addition, opportunities should be provided for continuing growth of responsibilities and salary in the context of technical activities, through "dual-ladder" structures (i.e., technical management paralleling corporate management) and other means.

Second, the effective lifetime of researchers can be extended through continuing professional development and education. Japanese engineers, for example, are said to receive very effective continuing training after being employed in industry. They appear to obtain an excellent theoretical education in the university, which is then augmented by rigorous and substantive practical training on the job. U.S. industry should support attendance at technical meetings, short courses, and sabbaticals at academic centers. Universities can organize part-time, weekend, and evening courses in cooperation with local industries. In addition, industrial researchers can be brought into closer contact with academic research through joint university-industry research contracts awarded by government agencies.

Finally, industry research engineers could also contribute significantly to the nation by advising the government on research planning. Such advice would help to stabilize fields of engineering research and coordinate advances in technology across related fields.

Cooperation

In the interest of the overall health and competitiveness of industry, companies could afford to be much more open with their more fundamental engineering research data (e.g., in manufacturing), by making it available to the technical community at large. Companies should also take the initiative to form new cooperative consortia along the lines of the Microelectronics and Computer Technology Corporation to advance the state of the art in lagging industries. Such joint research ventures can provide excellent mechanisms for industrial investment in needed fundamental and applied research.

Improving Interaction Among the Sectors

Each of the sectors contributing to the technology development process—government, industry, and universities—focuses primarily on its own role and its own goals. This "three-legged" approach has worked well, and has been the basis for our nation's past technological successes. Cooperation among the sectors has always been a feature of that process. However, closer coordination and stronger links are now greatly needed. If, as was urged in the introduction to this report, we are to "begin to capitalize faster and more effectively on our breakthroughs in scientific and technological knowledge," we must deliberately strengthen the interactions among the sectors.

An important step will be to improve the linkage between engineering researchers and practitioners. This will require fundamental changes in attitudes and orientations. Traditionally, many university researchers have been reluctant to interact closely with their industry counterparts and to attend in a direct way to long-range industry needs. Many practicing engineers in industry, for their part, have been poorly equipped to understand the content and implications of university research findings; after entering the work force, they have had little opportunity to learn how to do so.

It is imperative that engineering researchers and practitioners alike begin to work consciously toward a mutual understanding of each other's work, needs, and goals, so that the transfer of technology from research to practice can become more effective and efficient. To this end, a crucial step will be to increase the numbers of engineers in industry who are able to understand and utilize the results of research. Exposure to research—beyond what is possible at the undergraduate level—is essential. The M.S. degree clearly will come to be a requirement in many areas of engineering practice. Some practicing engineers will also hold the Ph.D. These highly educated practitioners could do much to bridge the gap between engineering research and practice.

Cooperative research activities have recently been the center of much attention in engineering, and have been a good step in the direction of improving the linkages among sectors. With the help of government, industry and the universities have developed a number of new approaches to research collaboration. For example, the NSF has established 20 university-industry cooperative research centers, and its ERC program has had high visibility. DOD is establishing a parallel program, and other federal agencies are considering similar actions. The Semiconductor Research Corporation, founded in 1982 with a long roster of corporate members, has already organized centers of excellence with long-term thrusts at three universities. In addition, a number of states have initiated successful programs involving joint state, university, and industrial participation in technology centers of excellence. Individual engineering schools have also begun to stress improved interaction with industry through joint research and other programs.

Cooperative research programs involving university personnel with their counterparts in industry (and in government laboratories) can be fruitful in many ways. They can broaden the base of support for university teaching and research, give (two-way) access to research skills and equipment not otherwise available, and develop in students and faculty as well as those outside academia an awareness of opportunities and constraints as seen from various perspectives. We have emphasized the importance of instilling in students a sense of the flavor, attitudes, and approaches of engineering in the real world. Early contact with the engineering world is the best way to impart that awareness. *A tradition must develop in which university people—faculty and students alike—participate on a long-term and continual basis in both the research and facilities of industry and government*.

Mutual expectations should be reconciled at the outset of such cooperative research ventures. Each party must try to understand the other's objectives and needs. For example, the conflict between short-term pressures and long-term goals sometimes causes problems in industry-supported university research. Milestones for evaluating progress are one potential solution. Two-way exchanges of personnel for varying periods are a feature of many successful cooperative research programs.

Conflicts over rights to inventions and other intellectual property sometimes have blocked otherwise promising research relationships between industry and universities. In reality, only a tiny fraction of university research projects result in economically significant patents or other intellectual property. It is questionable whether, in the aggregate, the realizable value from secured intellectual property exceeds the costs incurred in the prospective attempts to cover all contingencies. Worse, the atmosphere of open exchange that is an essential aspect of university research programs is poisoned when students and faculty become highly sensitized on matters of rights to intellectual property. *Thus, we favor university and industry policies that seek research payoffs in the form of new knowledge (available in the public domain) and well-educated graduates, rather than emphasizing patent rights and royalty payments*.

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Bioengineering Systems Research in the United States: An Overview

EXECUTIVE SUMMARY

There is no more exciting and challenging field of science and engineering today than the study of living organisms from the entire body through its subsystems down to cells and subcellular processes, and the application of this knowledge to the development of products and technologies for the benefit of mankind. Already, bioengineering is using the advances made by molecular biologists, geneticists, and biochemists to generate products ranging from simple molecules to complex proteins. Advances in our understanding of human physiological systems have led to entirely new technologies for diagnosing disease and repairing or replacing damaged systems. The potential of these technologies for enhancing human health, food production, and environmental quality is enormous.

Bioengineering encompasses those disciplines that seek to apply engineering knowledge to (1) the development of new and improved devices for health care, (2) the advancement of our understanding of living systems, and (3) the scale-up and production of new products derived from advances in biology. It includes the fields of biomedical and biochemical engineering. (The latter encompasses the engineering aspects of biotechnology, including genetic engineering.) By its very nature, bioengineering is interdisciplinary. It makes use of virtually every traditional engineering discipline as well as those sciences related to agriculture, biology, biochemistry, medicine, and public health.

The uses of bioengineering research represent potentially significant economic opportunities for the United States. An estimated $40–$100 billion worth (in 1984 constant dollars) of biologically derived products annually could be created by the year 2000 through biochemical engineering alone (National Academy of Sciences, 1984). The overall market for biomedical engineering devices and systems is estimated to be $11 billion for 1987. Because most engineering-intensive segments of that market have been growing at annual rates ranging from 10 to 25 percent, the potential size of the market by the end of the century is on the same order as that for biochemical engineering. To achieve the potential that both fields offer, however, more science and engineering research is required.

American researchers have clearly established the United States as the world leader in the fundamental fields related to bioengineering. Nevertheless, several European countries and Japan are currently devoting a significantly higher proportion of their national resources to important elements of bioengineering research than is the United States. The economic stakes alone are considerable. To assure U.S. leadership in this economically vital field, and to realize bioengineering's full potential for improving the quality of human life, more government support for research is needed.

Support for bioengineering research is relatively small and scattered throughout the federal government. The National Science Foundation (NSF) and the National Institutes of Health (NIH) are the two principal agencies of the U.S. government that fund bioengineering research. Yet the National Bureau of Standards (NBS), the National Aeronautics and Space Administration (NASA), and the Veterans Administration, among other agencies, also have programs or an interest in various aspects of bioengineering research.

There is currently no interagency mechanism by which the programs of these various agencies are coordinated. Moreover, bioengineering research projects are typically spread among a variety of offices and programs within an agency. The NSF, for example, funds bioengineering research through its programs on biochemical and biomass engineering, biotechnology, and aid to the handicapped. Other NSF programs also support bioengineering as a part of larger research studies. As a result, it is difficult to determine with certainty how much NSF spends on bioengineering research alone; however, the research budgets of the NSF divisions that support bioengineering total more than $12 million, of which $2.2 million is earmarked for the newly established bioengineering research center at the Massachusetts Institute of Technology (MIT).

The lack of a specific focus on bioengineering is particularly evident at NIH, where only an estimated $11 million out of a $660 million intramural research budget currently supports projects with a significant bioengineering component. Whereas NSF has recently created an Office of Biotechnology Coordination, no such mechanism exists within NIH. As the principal agency of the U.S. government for biomedical research, NIH emphasizes projects that focus on fundamental biological or medical science. Although it is estimated that about one-fifth of NIH's extramural grants include bioengineering studies, usually as a minor component of larger projects, those studies account for only about 3 percent of the extramural research budget. Few engineers sit on the committees that rank and fund research proposals.

More generally, there has been a shift in government policy in recent years with respect to research funding. As a result of budgetary restrictions, the philosophy has changed from one that viewed research as an investment in the future to one in which it is considered explicitly as a means to product development. Whereas this approach is beneficial in many ways, it tends to overlook imaginative but speculative research proposals that could achieve significant long-term results.

Industry involvement in bioengineering has become considerable, particularly in the burgeoning field of biotechnology. However, the activity is mostly limited in scope and focused on near-term product development. Sponsorship of fundamental research at universities has not been either substantial or consistent, nor is this situation likely to change soon. In the biomedical field, the market for many devices (e.g., prostheses) is small, and falls within an area in which government has traditionally held responsibility. (The situation in this respect is analogous to that for "orphan drugs.") Consequently, federal support of both basic research and the fundamental engineering research needed to realize the useful potential of basic discoveries in bioengineering will continue to be essential.

This report identifies 11 areas within the field of bioengineering that could produce important research results in the years ahead. Eight areas involve biomedical engineering research. These are systems physiology and modeling, human rehabilitation and neural prostheses, biomechanics, biomaterials, biosensors, metabolic imaging, minimally invasive diagnostic procedures, and artificial organs. Three areas of biochemical engineering were also selected—bioreactors and biocatalysis, separation and purification, and bioprocess instrumentation and control.

These research areas illustrate the potential for bioengineering research and point out where financial support is urgently needed to achieve significant results. They are not, however, the only research areas worthy of government support. In addition, the listing of eight biomedical engineering and three biochemical engineering topics does not signify any relative priority between the two subfields of bioengineering. Both subfields are essential.

Although each of the research areas presents its own specific needs and opportunities, certain ones are common to more than one area. For example, more research is needed to extend our knowledge of the physical and chemical properties of living tissues, cells, and subcellular components as well as their relationship to each other. Such research is of particular importance to our understanding of systems physiology, but could also be applied to neural prostheses, biosensors, biomaterials, and artificial organs. More also needs to be known about the characteristics of materials used in biomedicine and how those materials interact with living tissue. This information would be important in developing both new prostheses for disabled persons and new minimally invasive techniques for diagnosis and treatment. In this regard, mathematical modeling is important for learning how to represent the properties and behavior of tissues, cells, and subcellular components so as to simulate the response of a living system.

Certain topics hold special importance. Improved biosensors, for example, could convert biological signals more quickly and reliably into electronic responses that can be processed and used for medical diagnoses, as well as providing the input needed for the control and optimization of industrial bioprocesses. Research on biosensors could also help scientists and physicians to better understand the body's natural sensors. To be useful, however, biosensors must be compatible with the human body and its signal processing systems.

Similarly, advances in metabolic imaging could also aid diagnosis. This field has already produced positron emission tomography and nuclear magnetic resonance imaging, among other new technologies. Further developments, however, may depend on basic research on tissue properties, both physical and biochemical, and on integrative systems analysis.

A third area in which bioengineering research can make particularly significant contributions is the development of artificial organs. Organ replacement is still in its infancy, and great strides can be expected in the future. At present, air-driven artificial hearts are the focus of attention; work is also progressing on electrically powered devices that would be implanted to assist the heart for several years rather than replace it. However, many of the body's organs besides the heart are potential candidates for replacement or assistance by engineered devices. Multidisciplinary efforts combining biochemical and biomedical engineering should eventually lead to synthetic systems able to replace a range of human organs and all the functions a single organ performs.

In the expansion of biomedical technology, there is a promise of vastly improved health care delivery, possibly even at reduced costs (as illustrated in this report in connection with percutaneous transluminal angioplasty). Greater attention needs to be paid to the costs and benefits associated with the introduction of new technologies and new devices to encourage their effective and efficient use and to discourage costly and wasteful practices.

To carry out these research programs and projects, more trained bioengineers will be needed. Although the total number of bioengineering students in U.S. graduate schools has increased significantly during the last decade, the number relative to all engineering graduate students has remained about the same (roughly 2 percent). Greater support for university programs from NSF and NIH would encourage more engineering graduate and postgraduate students to go into bioengineering.

Studies indicate that there are not enough biochemical engineers in the United States today to meet existing needs. In addition, there is a shortage of the qualified faculty members needed to expand graduate enrollments and courses in the future.

Fewer than 20 U.S. colleges and universities now have meaningful biochemical engineering programs. In part, this is because biotechnology is still a relatively new field of study, and one that is highly interdisciplinary. In order to realize the vast potential of this field for improving the quality of life, and to meet the strong competitive challenge that other nations are mounting to capture the large market for products of biotechnology, it is imperative that we provide the incentives and the research programs needed to increase the cadre of research talent.

Recommendations

The following recommendations of the panel are excerpted from the section "Conclusions and Recommendations" at the end of this chapter. See that section for accompanying conclusions and rationale, which are in turn based on the report itself.

1. The panel recommends that the coordination of research programs in bioengineering throughout the federal government be improved and that the coordination include the relevant work of NIH, NSF, NBS, and other agencies interested or involved in bioengineering research. To ensure its effectiveness, it is essential that this interagency coordinating effort receive the full recognition and support of upper management in each of the participating agencies.
2. The coordination of bioengineering research *within* supporting agencies should also be improved. For example, NIH could create an "office of bioengineering research" to review and coordinate its investigations. Such an office would be one way of focusing attention on the careful development of this important field.
3. NIH should give careful attention to the need for large-scale, focused research in biomedical engineering. To achieve the required scale, NIH should consider creating a center for such research, comparable in concept and size to the Engineering Research Centers being created by NSF. Any such NIH center should encourage links between academic research and clinical practice.
4. Both NIH and NSF should devote a greater share of their research budgets to supporting bioengineering programs at U.S. universities and colleges. NIH in particular should expand its programs that encourage medical students to go into research so as

to include bioengineers as well. Stipends awarded for postdoctoral study should be sufficiently high to offset the attractiveness of salaries offered by industry.

5. Those who rank and award grant proposals within NIH and NSF should consider funding research projects that have a great potential for significant results, but also a high risk of failure. The panel applauds the strong current trend in this direction at NSF.
6. Bioengineering in all its aspects is advancing rapidly. To ensure the long-term health of this commercially important field, a permanent advisory body should be created within the Executive Branch. This body would assess bioengineering research opportunities and needs, review the relevant research programs of NIH, NSF, and other government agencies, and identify needs for new programs or changes in directions.

INTRODUCTION

Rationale for a Study

Bioengineering is associated with questions that have generated some of the most intense interest, excitement, apprehension, and debate ever seen in the context of American science and technology. Within the last decade, some of its newer and more revolutionary elements have captured the attention of the public fully as much as they have the interest of the research community. From heart transplants and reproductive technology to genetic engineering, these are topics that fascinate people because they deal so intimately with life itself.

Apart from (or perhaps partly because of) its intrinsic interest to researchers and the public, bioengineering also has tremendous economic potential. It is one of the fields on which our most formidable international industrial rivals are placing great emphasis; it is also a field that holds the promise of providing the new technologies needed to supply our increasingly sophisticated medical care at minimum costs for benefits received. Thus, the Engineering Research Board considered it to be essential to identify research needs in this field and to assess the health of the environment within which it is conducted.

Scope of the Study

The Panel on Bioengineering Systems Research was formed to address the status of research in the bioengineering field.* In particular, it was charged with identifying new or emerging areas of research. To establish boundaries, the panel defined three broad areas of research as falling within the scope of its study. These are

- the application of engineering knowledge and concepts to understanding the human body and other biological systems and the interaction of humans, machines, and the environment;
- new and improved biomedical devices for use in maintaining health and treating disease; and
- the manufacture of products using the techniques of the so-called "new biology," including molecular and cellular biology.

Because of the need to set boundaries, certain significant areas could not be addressed; agriculture and food processing and military bioengineering are examples of such areas. In addition, those areas falling primarily within the purview of other panels of the Engineering Research Board were not specifically addressed.

It is worth noting that considerable confusion exists over the terms used to describe the field of engineering that seeks to apply the biological and medical sciences. "Bioengineering," "genetic engineering," "biotechnology," and "biomedical" or "biochemical" engineering all have their adherents; often the terms are used almost interchangeably.

This report uses "bioengineering" to encompass those disciplines that seek to apply our understanding of living systems to the engineering of useful products. Bioengineering uses the knowledge gained through research to develop new and improved devices for use in maintaining health and treating disease as well as for the manufacture of products using the processes of modern biology. Thus, bioengineering can be said to include both biomedical and biochemical engineering. The latter terms are used, where appropriate, when referring to those specific areas. In the three areas listed previously as being within the scope of bioengineering research, the second refers to biomedical engineering and the

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* The word "systems" is used here to denote, not "systems engineering," but the organismic, physiological, and hardware systems on which this research focuses. For a more detailed definition of this concept, see the report of the Engineering Research Board.

third refers to biochemical engineering. The first item on the list is relevant to both disciplines.

Biomedical engineering research investigates (1) the structure and properties of cells, tissues, organs, and whole bodies; (2) the growth and repair of living tissues; (3) strength and tolerance; (4) the behavior of electrically excitable tissues; and (5) relevant surface and flow phenomena. All of these topics apply to the diagnosis and treatment of disease, as well as to rehabilitation, prostheses, artificial organs, aging, and trauma-related injuries.

Biochemical engineering includes the engineering aspects of biotechnology. Biotechnology was defined in a recent study (Office of Technology Assessment, 1984) as the "use of living organisms (or parts of organisms) to make or modify products, to improve plants or animals, or to develop microorganisms for specific uses." Thus, there is clearly a substantial overlap with the "new biology."

By its nature, bioengineering research is broadly interdisciplinary in scope. It relies heavily on the life sciences: biochemistry, biophysics, biology, medicine, public health, and agriculture. It makes use of virtually every branch of engineering, as well as subdisciplines within the various engineering fields, such as structures, fluid mechanics, thermodynamics, circuit and field theory, and signal processing. Bioengineering also draws from mathematics, chemistry, and physics. Probably no other field of engineering embraces so many spheres of human inquiry.

Background

Bioengineering depends heavily on information gained from the biological and biomedical sciences. It differs from them by focusing on living organisms as systems that can be used for the benefit of mankind. Bioengineering research seeks to discover and organize the information needed to develop new products and processes. In this sense, bioengineering is concerned with applying basic advances in our knowledge about the human body and other biological systems. To achieve its potential, more engineering research as well as scientific research is required.

Processes in biology and medicine, however complicated, ultimately obey the same basic principles as all the other natural sciences. Thus, an essential step in the engineering of biological processes is the development of mathematical models that describe and predict biological behavior. Historically, many important advances in the natural sciences have been founded on models based on experimental data. The structure of DNA is perhaps the best known example. Others include cardiovascular flow, the tribology of synovial joints, and musculoskeletal modeling. Together these models have clarified understanding and led to practical developments in diagnosis and therapy.

Bioengineering seeks not only to make new devices, but also to understand them more fully. For example, the first artificial heart valve required engineering research. The development of heart valves designed to last a lifetime must be based solidly on the principles of fluid mechanics, physiology, and biomaterials research. Correspondingly, anatomy, physiology, and pathophysiology—interpreted by bioengineers at the system and cellular level—are being illuminated in new and important ways that lead to novel and less invasive diagnostic methods, replacement joints and organs, and other processes and products that improve medical care and enhance human health. Advances in basic science drive the engine of invention and improvement. Breakthroughs in computer science and information science, for example, with their attendant engineering advances in hardware and software, have many potential applications in bioengineering. (See the report of the Panel on Information, Communications, Computation, and Control Systems Research for a discussion of breakthrough research areas in these fields.) These technologies offer solutions to long-standing problems in biology, medicine, and health care delivery, as well as improvements to current practices. In addition to the basic capability for computer modeling, promising examples would include the availability of "smart" microprocessor-based analytic instruments in biology, expert systems in support of medical diagnosis, new imaging diagnostic technologies, and various clinical and medical information systems to enable effective and economical health care delivery.

Similarly, there have been tremendous advances in the basic sciences relating to gene manipulation. Genetic engineering uses recombinant DNA and cell fusion techniques built on a strong science base in molecular and cellular biology, biochemistry, and microbiology to construct biocatalysts that can produce, under controlled conditions, seemingly limitless numbers of potentially useful products. The ability to make biological products has created many research needs in the area of bioprocess engineering.

However, in contrast to the long-standing relationship between chemistry and chemical engineering, no comparable association yet exists between biology and biochemical engineering. Such a relationship is absolutely necessary to extend the practical implementation of genetic engineering.

There are many potential uses for the products of biotechnology. In the health field, new pharmaceuticals for humans and animals are being developed from naturally occurring molecules that are more effective and safer than the drugs they will replace. Monoclonal antibodies and genetically engineered enzymes are now being used to diagnose various diseases. In the area of food production, growth promoters can be used as a food supplement to improve animal nutrition. New crop varieties can be made that will be more resistant to adverse environmental conditions or disease. In addition, crop production will be more effectively controlled by the use of growth regulators. In the area of environmental quality, new and more effective ways can be designed to detoxify waste. Similarly, biocatalysis can be used to produce a wide range of chemicals now made from conventional feedstocks.

Apart from their obvious benefit to mankind, the application of these concepts through bioengineering also presents significant economic opportunities for the United States. Biochemical engineering could create an estimated $40–$100 billion worth of biologically derived products annually by the year 2000 (National Academy of Sciences, 1984). The market for biomedical engineering devices (including diagnostic imaging, therapeutic devices, medical laboratory instruments, and medical information and other communication systems) is estimated at $11 billion for 1987 (Attinger, 1984), and is growing at an annual rate of 10 25 percent, so that the potential size of this market in the year 2000 is similarly large. However, considerable research is still needed in a variety of areas to take advantage of these vast opportunities.

ESPECIALLY IMPORTANT OR EMERGING AREAS OF BIOENGINEERING SYSTEMS RESEARCH

The Panel on Bioengineering Systems Research has identified 11 subjects, divided into two broad categories, that represent es pecially important or emerging areas of bioengineering research. The areas selected emphasize the potential usefulness of the results of engineering research in this dynamic field. They are not, however, the only areas worthy of research or likely to yield important results; space limitations preclude a more complete listing.

These 11 subjects include 8 areas of biomedical engineering research and 3 areas of biochemical engineering research that require priority attention if advances in this field are to proceed rapidly and in a balanced fashion. The areas in biomedical engineering are

1. systems physiology and modeling;
2. neural prostheses for human rehabilitation;
3. biomechanics;
4. biomaterials;
5. biosensors;
6. metabolic imaging;
7. minimally invasive medical procedures; and
8. artificial organs.

The areas in biochemical engineering are

1. biocatalysis/bioreactors;
2. separation and purification; and
3. bioprocess instrumentation and control.

It is important to emphasize that no priority is implied by the ordering of either the major sections or the topics within sections. The panel stresses that biomedical and biochemical engineering research are both of vital importance for the health, well-being, and economic fortunes of the nation.

Biomedical Engineering

Biomedical research leading to new technology offers the promise of vastly improved health care delivery, possibly even at lower cost per patient (for example, through the automation and centralization of some aspects of health care). Yet with the continued growth of demand for health care services, the increasingly greater sophistication of biomedical technology, and an aging U.S. population, there is also the risk that costs can become exorbitant. As we introduce the biomedical engineering research topics identified as having high priority, the panel emphasizes that careful attention needs to be paid to the costs and benefits associated with the introduction of new technologies and new devices to encourage their effective and efficient use and to discourage costly and wasteful practices.

Systems Physiology and Modeling

Living organisms are immensely complex systems. A "mere" subcomponent such as the human brain cannot be rivaled by the largest currently imaginable supercomputer. Neither has even a small system, such as a single red blood cell, ever been fully understood in terms of its physical, chemical, and material properties or behavior. In addition, a red blood cell, with some 2,000 metabolic reactions, is less complex than growing, dividing cells or cells with excretory or contracting functions.

We now know enough basic facts in cell biology, biochemistry, and physiology, however, so that an integration of this knowledge is practicable. More information is needed to improve such integration and to translate the results into effective therapies. In particular, more data are needed on anatomy (tissue components and their relationships); on physical properties of tissues, cells, and subcellular components; on chemical and biochemical properties; and on regulatory, transport, and communication mechanisms (hormones, agonists and antagonists, receptors, and neural stimuli).

The integration of information, expressed quantitatively, can be tested for overall validity by formulating a mathematical model of the system. Such models can serve as repositories for and summaries of vast amounts of information. They can also be used to determine what is missing from our knowledge, because every assumption requires proof by evidence from observation.

Mathematical modeling of larger physiological systems will provide a basis for identifying their abnormalities and the ramifications for cellular, organ, or whole organism behavior, or even for ecology and economics. Mathematical modeling is also applicable to studies of plant cells and microorganisms. Some aspects of modeling may also give rise to spin-offs in other engineering sciences, as is already happening with certain numerical methods for solving partial differential equations and algorithms for parameter optimization.

Physiological knowledge, especially as expressed in models, has a wide range of potential applications in bioengineering. Considering the influence on biological function of mechanical factors such as force, pressure, and strain, or of kinetic and potential energy, entropy, or temperature (to take but a handful of examples) provides many opportunities for bioengineering advances in medicine. For example, the interaction between fluid mechanical forces and the endothelial cells that line the walls of arteries appears to play an important role in the development and spread of atherosclerosis. In another area, the study of load-bearing joints has revealed that friction-induced temperature increases produce direct mechanical effects on, and biological changes in, the bones and tissues of joints. This finding has opened research "windows" on the question of how arthritis occurs.

Neural Prostheses for Human Rehabilitation

Some 12 percent of the U.S. population suffers to some degree from physical disabilities. Many are victims of congenital defects, acquired diseases such as cancer, or trauma. The treatment for such chronic disability is technology dominated and systems oriented—the realm of the biomedical and rehabilitation engineer. Despite very modest funding for this research, the lives of handicapped persons have been significantly improved through a variety of technologies. These include computer braille, reading machines, and electronic mobility aids for the visually handicapped; hearing aids for the deaf; postamputation prostheses; and microcomputer-based communication for those who cannot speak.

Rehabilitation and prosthetic devices also contribute greatly to the quality of life of those who have suffered serious trauma. Each year, more than 80,000 Americans sustain permanently disabling but nonfatal injuries to the brain or spinal column (National Academy of Sciences, 1985). The direct and indirect costs to society of these injuries add up to an estimated $75–$100 billion a year. A new class of neural prostheses are being made possible by integrated electronic circuits, together with an improved understanding of stable and biocompatible electrodes. Such circuits connect directly with the central and peripheral nervous systems.

Several examples, some now available, illustrate the potential. The first ear implant to bring sound to the neurologically deaf via a simple electrode has recently received approval from the U.S.

Food and Drug Administration. Multichannel devices that can process sound promise in the future to enrich the hearing of the profoundly deaf, and ultimately to permit them to understand speech. Farther off are attempts to regain a semblance of vision by electrical stimulation of the occipital cortex. Neural prostheses even offer hope of restoring functional movement and bladder control to those who have suffered a stroke or spinal cord injury.

To achieve this potential, more research is needed on the systems physiology of movement, speech perception, and vision. Research is also needed on circuit design, transducer and electrode development, control and microprocessor systems, custom-integrated monolithic chip design, and fabrication packaging designed for the human body. Work in a variety of mechanical areas would also be useful. These areas include compact and quiet actuators, complex drives and linkages, transducers, automatic controls, systems integration and design, and power supplies.

Biomechanics

Biomechanics deals with the response of living matter to physical forces. Molecules, cells, tissues, organs, and individuals move, deform, grow, or atrophy as a result of these forces. Hence, they are subject to the laws of biomechanics. The objective of biomechanics research is to explain and reduce both trauma (as occurs in accidents and sports) and long-term deterioration (as seen in low back pain and osteoarthritis).

In recent years, research in biomechanics has improved our understanding of the nature of blood flow, joint movements, locomotion, trauma, and healing. It has guided the development of clinical diagnostic and treatment procedures, the design and manufacturing of prostheses for sick and disabled persons, and the invention of new medical instruments. Biomechanics research has led to the development of devices and methods that have improved human performance in the workplace, in sports, and in space, and has developed ways to improve automobile safety.

An important area of biomechanics research is directed toward preventing injuries. Injuries are the fourth leading cause of death in the United States. For ages 1–44, they are the *leading* cause of death, and for ages 5–44, they kill more people than *all* other causes combined (National Center for Health Statistics, 1983). More research is needed on the biomechanics of injury and disability. How does the human body, for example, respond to mechanical forces such as those imparted by automobile dashboards, bullets, and knives? In addition, how do different head movements cause injury? Research will improve our understanding of how permanent disability occurs. The recent discovery that the use of alcohol significantly decreases the strength of nerve fibers is an example of such research results.

In the future, biomechanics research could reduce the incidence of heart diseases, atherosclerosis, and stroke—the leading killers of humans—through an improved understanding of the interaction of blood flow and blood vessel walls. Research on stresses in the lung could be used in treating emphysema. Similarly, research on the spinal column could be used to prevent certain types of back pain. Research could also lead to new ways of preventing arthritis and joint degeneration, and to the development of permanent joint replacements.

In another area, biomechanics research is an integral part of the development of artificial limbs, heart valves, hearts, and kidneys. Future research will aim at developing permanent artificial internal organs that ameliorate the problems of biocompatibility. Such research will focus on the mechanics of living cells and biomolecules in order to understand how artificial materials interact with the cells of the body (see the next section). It will also focus on the relationship between the growth of cells and the physical stress caused by fluid flow and tissue deformation. Knowing the correct range of stresses to apply to the cells could provide a key to controlling the growth of living tissues.

Through biomechanics we seek a thorough understanding of the neuromuscular control system. Such an understanding is the key to developing artificial limbs and robotics, which could lead to ambulatory systems for the disabled. It is also important to enhancing productivity through workplace design.

Biomaterials

Many new opportunities to synthesize materials derive from the ability to manufacture and modify polymers and specific macromolecules. These materials can be designed for specific purposes. Notable examples include membranes that provide timed drug release by constraining diffusion, implantable pumps for de livering insulin over an extended time, and glucose sensors now being developed for diabetics.

To develop new biomaterials, bioengineering researchers are conducting basic studies on the interactions between different biological molecules and cells in various physicochemical environments. Increased knowledge and improved assays have paralleled the development of new clinical devices and implants. Yet, because of the complexity of the interaction between artificial biomaterials and living cells, our knowledge is still far from complete. Continued support of basic research is needed.

Particularly important biomaterials research needs are to:

- elucidate the physicochemical characteristics of materials to be used in cardiovascular devices;
- investigate how materials interact with bone, teeth, and other tissues; and
- pursue the development of encapsulation materials designed specifically for implanted devices.

Biosensors

Biosensors convert biological information into an electronic signal that can be processed and used in diagnosis, treatment, and in vivo control. Improved biosensors and instrumentation systems would permit earlier disease detection. They could provide better and more reliable data, allow more medical care to be accomplished outside traditional hospitals and clinics, make it possible to monitor patients at home, and thus reduce health care costs.

Research on biosensors should also help scientists to better understand the body's natural sensors and actuators. This could in turn lead to ways of obtaining signals from the body's natural sensors by using devices that monitor the nervous system. Smaller, more reliable, and more reproducible sensors might be built using micromachining technology adapted from the microelectronics industry. Such sensors could be directly integrated with signal processors or preprocessors to improve reliability.

To be useful, biosensors must be compatible with the human body and with signal processing systems. This aspect of the problem requires further study. Similarly, research is needed to develop noninvasive or minimally invasive sensors that would permit diagnostic and therapeutic monitoring of a patient at home (for example). The information from the sensor would be sent to a hospital computer, where it could be read by a physician, nurse, or technician.

An important emerging area of biosensor research involves chemical sensors for both laboratory and in vivo monitoring. A chemical sensor monitoring blood and urine chemistry, for example, could provide physicians with bedside information, thus making earlier diagnosis possible. Research is needed in areas such as membranes (allowing controlled charge transfer), adherence to semiconductor surfaces, and the coupling of biological molecules to traditional electronic and optoelectronic circuits. Such research could lead to better control of therapeutic devices, closed-loop pharmaceutical administration, artificial organs, and prosthetics.

Metabolic Imaging

Metabolic imaging involves the use of positron emission tomography (PET), nuclear magnetic resonance (NMR), x-ray computed tomography, ultrasound, or other technologies to obtain information on how the body chemically alters, uses, and eliminates food, drugs, hormones, and other substances. These techniques are safe and powerful means of seeing inside the body so as to determine remedial action more precisely or to avoid unnecessary invasion.

Recent developments in metabolic imaging include (1) rapid improvements in NMR technology, providing better chemical resolution and the potential for imaging cell pH, phosphorus in varied forms, sodium, and other atoms as well as protons (hydrogen); (2) higher temporal and spatial resolution by PET; (3) rapid advances in the kinetics of mass transport and transmembrane exchange; and (4) deeper insight into biochemical regulation in intact functioning organs.

The new technologies provide better data for interpreting high-resolution spatial images in terms of rates of transport across membranes, intracellular reactions, and cell functions. To better interpret images and understand their limitations, more needs to be known about kinetic modeling of physiological and biochemical events and about the biochemical changes that occur in disease. This field, highly dependent on basic research in tissue properties (both physical and biochemical) and on integrative systems analysis, is one that could lead to important scientific achievements as well as substantial industrial activity. High costs are a feature of most of these technologies. Cost reduction in health care is a long-range (5-to 10-year) proposition—an initial several-year period of intensive exploration often leads to a state of knowledge wherein the expensive procedures are used more selectively or replaced by specific and cheaper methodologies.

Minimally Invasive Medical Procedures

Minimally invasive medical procedures either replace or preclude the need for major surgery. One example is the treatment of coronary arteries whose interior walls have become covered by plaque. This deposit restricts blood flow through the arteries, so that less oxygen is available to the heart. Individual cardiac muscle cells die and, in severe cases (i.e., myocardial infarction), death occurs through arrhythmia or a failure of the heart to contract effectively. Treatment today involves open heart surgery and the replacement of obstructed arteries with segments of veins transplanted from other parts of the body. Such surgery, though no longer much more risky than an appendectomy, is still traumatic and very expensive.

Percutaneous transluminal angioplasty is a minimally invasive procedure in which a catheter is threaded into the restricted vessel from an artery in the leg or arm and a small balloon at the end of the catheter is inflated so as to dilate the affected blood vessel. The goal is to reopen the vessel, taking care not to weaken or tear it. Angioplasty not only reduces patient discomfort and recovery time, but also reduces the costs of treatment. At present, some 250,000 cardiac bypasses are performed annually at a cost of about $16,000 each, or more than $4 billion in total. Angioplasty costs about half that much. Other minimally invasive procedures could have similar cost savings. If they were to replace all bypass procedures, a possible savings of $2 billion per year could be achieved.

Experiments are currently being conducted on the use of lasers to remove plaque. The development of this new approach will involve analysis of the mechanical stresses borne by the artery, the desired hydrodynamic properties, and how the laser affects both plaque and healthy tissue. It will also require the design of new mechanical, electrical, and optical instruments. These advances will demand broad engineering skills and knowledge.

Artificial Organs

Most body organs perform many functions. They typically perform mass transport or mechanical action while separately releasing a substance into the blood that sends a chemical or molecular signal to other organs. Relatively normal physiology and biochemistry is needed for all organs to work in concert and produce a feeling of well-being. A single failed organ can cause disability or death.

Recently, medical science has turned to organ transplants and mechanical or synthetic devices to replace diseased organs. Organ replacement is only in its infancy, and great strides can be expected in the future. Current synthetic devices either perform an intermediary metabolic activity or replace a specific mechanical function. The artificial heart is an example of the latter. Air-driven artificial hearts have been implanted in a few patients to replace failed natural ones. Work is progressing on electrically powered devices that would be implanted to assist the heart for several years rather than replace it. These devices are undergoing careful testing to ensure their safety, effectiveness, and reliability. Clinical evaluation is anticipated in the next few years.

Whereas the artificial heart program is exceedingly expensive, the development phase will benefit selected individuals. Other organ replacements, such as implanted insulin-producing cells for diabetics, may be less costly. In the future, multidisciplinary efforts combining biochemical and biomedical engineering should eventually lead to synthetic systems capable of replacing a natural, multifunction organ in human beings.

Biochemical Engineering

Biological processes and organisms have been used commercially for centuries, but the degree of sophistication and the range of new uses and products have increased significantly in recent years. In fact, the principal limitation on the development of additional commercial products through biochemical engineering, or biotechnology, is the lack of a technology base that can address the following key challenges:

- more experience and better techniques in the use of large-scale cell cultures so that the full range of plant and animal cell systems can be used, rather than just simple microorganisms (e.g., bacteria and yeasts);

- a broadened understanding of enzymic catalysis in aqueous, nonaqueous, and mixed-solvent systems so that the chemical process industry can meet selectively and efficiently the increasing challenges posed by environmental impact, energy use, and (most important) process chemistry; and
- an expanded knowledge base to achieve large-scale processing, including the recovery and purification of complex, unstable biological macromolecules (e.g., antibodies and peptide hormones) from product mixtures, as well as the economical and efficient recovery of simple biologically derived organic compounds (e.g., ethanol and amino acids) from highly dilute and impure solutions.

The panel identified three specific areas of research aimed at meeting these challenges: biocatalysis/bioreactors, separation and purification, and bioprocess instrumentation and control. These research needs parallel closely those identified in 1984 by the Panel on Chemical and Process Engineering for Biotechnology in a briefing to the Office of Science and Technology Policy (National Academy of Sciences, 1984).

Biocatalysis/Bioreactors

Mechanically agitated reactors currently used for antibiotic fermentations are often poorly or not at all suited to meet the diverse demands of bioprocessing. New techniques are needed for large-scale culture of plant and animal cells as well as of the new microorganisms now being engineered for bioproduct synthesis. Fundamental knowledge of how physical and environmental factors influence intracellular biosynthetic pathways is essential to the development of such techniques. Bioreactor research will require joining such sciences as molecular and cellular biology, microbiology, and cell physiology with basic engineering skills, chemical kinetics, thermodynamics, fluid dynamics, heat and mass transport, and precise bioprocess control—a rare combination today in either industrial or academic laboratories.

An important parallel direction for bioreactor research is the development of methods for using free or immobilized enzymes, combinations of enzymes, or nongrowing whole cells as catalysts for biosynthesis. The effective use of such diverse forms of biocatalysts requires exploration of fluid bed, fixed bed, membrane, cell recycle, tubular, and other reactor types in which to carry out biosynthesis. The challenge is to translate the existing knowledge base for chemical reactors into biosystems in which strict asepsis, complicated biological regulation processes, enzyme and cell fragility, cofactor regeneration of enzyme activity, and maintenance of cell energy all add to the problem. Fundamental processes developed in benchtop systems must be tested in large-scale equipment to gain an understanding of the scale sensitivity of biosystems.

Separation and Purification

Once a bioreactor has done its job, the resulting mix must be separated and purified to isolate the desired product. For fragile, high-unit-value products intended for human or animal use, there is a premium on processes that minimize deterioration and maximize purity. For low-unit-value products, in which competition lies in nonbiological factors, the premium lies in energy-efficient recovery processes with low environmental impact.

Biotechnology can benefit from increased research on three aspects of separation and purification.

1. **Modification of conventional, large-scale industrial separation methods.** Ion-exchange chromatography, pressure-driven membrane separation, electrodialysis, and liquid-liquid extraction are now used in industry to recover or purify antibiotics and simple molecules, such as citric acid. These methods are generally too nonselective or too destructive to be used for processing fragile proteins. Refined techniques are needed to make them suitable for recovering the newer products of biotechnology.
2. **Adaptation of biochemical laboratory separation methods to large-scale bioprocessing.** Life scientists have developed extremely powerful and sophisticated tools, such as electrophoretic and affinity separation, that could be used for large-scale separations. However, much needs to be learned about the molecular mechanisms and kinetics of these processes before they can be used to make products.
3. **Novel separation and purification concepts.** New separation and purification concepts are needed that combine physical, chemical, and biological processes in unconventional ways. Several such processes are being studied in Japan and Europe. Some

concepts, such as aqueous two-phase separations that use water-soluble synthetic polymers and multifield fractionation, are based solely on physicochemical principles. Other processes combine biochemistry and cell biology with industrial chemistry and chemical engineering. Examples include separation based on modification of permeability, separation by selective enzymic transformation, and separation by genetically manipulated intracellular processes.

Bioprocess Instrumentation and Control

Sophisticated process control is required to successfully operate bioreactors and downstream processing equipment. This, in turn, depends on accurate measurement of critical process variables and the use of advanced estimation algorithms, process models, and control strategies. Current biosensors and control methods often lack the desired reliability or the capability to regulate process conditions with the necessary sensitivity.

Furthermore, the proper sensors often do not exist to monitor complex biological substances on-line. The use of enzymes, monoclonal antibodies, and even whole living cells as components of electrochemical and optical detectors could solve some of these problems.

Research on bioprocess models is needed to develop optimal control strategies and to extract the most useful information from measurements. Formulation of these models will require more advanced control algorithms and a greater knowledge of the effects of engineering parameters on cells and complex molecules.

ISSUES DETERMINING THE HEALTH OF THE FIELD

Bioengineering is a diverse field, and many elements of it are very new. Enormous social benefits are anticipated as a result of new knowledge, new procedures, and new products derived from research in both biochemical and biomedical engineering. However, an important secondary social benefit to be reaped from the explosive growth expected in this field lies in its economic potential. We in the United States can use these technologies to improve our lives; but we can also use them to help strengthen the nation as a whole by creating jobs and a positive balance of trade.

This is one of the main perspectives from which the Engineering Research Board, and in turn this panel, was asked to view the health and the future of engineering research.

The nation that leads in research has an edge in commercialization. American researchers have clearly established the United States as the world leader in fundamental research in most aspects of bioengineering, but this position is being challenged. The area of the field with the greatest untapped commercial potential is biochemical engineering, or biotechnology. Several European countries and Japan are devoting a significantly higher proportion of their national resources to this expanding area than is the United States. In addition, their support is better planned and coordinated.

Federal policies toward bioengineering and federal funding for it have a profound effect on its directions and rate of development. Industry involvement, both in the health care field with biomedical engineering and in the newly emerging biotechnology field, also affects the health of bioengineering. Finally, the availability of adequate numbers of qualified researchers in biomedical and biochemical engineering is critical to the continued development and maintenance of American preeminence in these important areas of engineering.

Impact of Federal Policy

To maintain U.S. leadership and to realize the full potential of bioengineering for improving the quality of mankind's life and health, more research of the kind discussed in the previous section is needed. Unfortunately, support for such research is at relatively low levels and is scattered throughout the federal government. One reason is the newness of bioengineering. Another reason is its intrinsically interdisciplinary nature. As we pointed out earlier, bioengineering encompasses and utilizes elements of several scientific and engineering disciplines. This diversity confounds the delineation of policy issues and research programs. Organizations, both within and outside government, perform best when they deal with well-defined and reasonably restricted bodies of knowledge, competencies, and interests. Because bioengineering defies such constraints, it lacks the organizational focus that biomedical science research receives, for example, at NIH or that oncology, to use a more specific example, receives at the National Cancer Institute.

Support for bioengineering research is spread across a number of government agencies, including the NSF, NIH, NBS, the Department of Energy, and the Veterans Administration, among others. The U.S. Department of Agriculture and the Defense Advanced Research Projects Agency support small programs of engineering research in biotechnology. Besides being spread across agencies, bioengineering research is often spread out among different units and programs within these agencies. Thus, the precise extent of government support for bioengineering research is difficult to determine.

NSF Support

Until the NSF recently created an Office of Biotechnology Coordination, it was hard to find reliable figures for bioengineering research in the principal agency through which the U.S. government supports scientific and engineering research. Most bioengineering research supported by NSF falls within its Engineering Directorate, which has programs in biochemical and biomass engineering, biotechnology, and aid to the handicapped. These three programs had a combined budget of $9.4 million in FY85. The Office of Biotechnology Coordination has now set up a scientific advisory committee and maintains a biotechnology information system for all of NSF.

In addition, NSF has created six engineering research centers (ERCs) at major universities in the United States. The ERC program is designed to promote fundamental engineering knowledge by focusing on cross-disciplinary research. One center, at MIT, is devoted to biotechnology. It specializes in research on genetics and molecular biology, bioreactor design and operations, product isolation and purification, and biochemical processing. NSF has awarded the center at MIT, known as the Biotechnology Process Engineering Research Center, $20 million for an initial 5 years ($2.2 million in operating funds in 1985). In addition, NIH has given the center an initial grant of $100,000.

NSF also provides funds for biomedical and biochemical engineering research through various other engineering divisions, as well as through its Industry-University Cooperative Research Project. Together, these funds make up nearly another $1 million, making NSF's total annual support for bioengineering research more than $12 million.

NIH Support

NIH is the principal agency of the U.S. government in biomedical research. Yet, despite its overall budget of $5.5 billion, NIH provides only limited support for bioengineering research. The groups that rank research proposals (the study sections) have very few engineering representatives; the groups that award grants to non-NIH researchers (the National Institute Councils) contain no engineers. As a result, although one-fifth of the 15,000 outstanding extramural grants are thought to have some bioengineering components, those components are estimated to account for about 3 percent of the overall extramural program (or 2 percent of the overall NIH budget), according to NIH internal program documentation.

Bioengineers fare no better in NIH's Intramural Research Program, which funds research by NIH's own in-house investigators. Of the $660 million intramural research budget, the Biomedical Engineering and Instrumentation Branch receives only $11 million. In addition, probably less than 5 percent of the 5,000 advanced-degree personnel who conduct research at NIH are bioengineers or from a related discipline (from NIH program documentation).

NIH's support for biochemical engineering per se is roughly equal to that for biomedical engineering. Each is represented by about 400 grants per year and, at a little over $50 million per year apiece, each represents about 1 percent of NIH's overall budget (W. J. Holliman, personal communication). However, according to a recent article in *Science* (Culliton, 1985), NIH provides an estimated $600 million (or about 15 percent of its overall budget) for basic research and training "directly related" to biotechnology. This work is pursued as a small part of larger research projects in cancer, genetics, clinical immunology and allergy response, vaccine production, and other areas of medical research. Little of the work is applicable to the development of biochemical engineering as an organized discipline. Indeed, the question of whether NIH ought to support biotechnology research in nonmedical fields such as agriculture, thus directing its basic research effort in this area more explicitly toward development of the U.S. biotechnology industry, has recently become a subject of controversy (Culliton, 1985).

The National Academies

The controversy with regard to NIH's role in biotechnology is a reflection of the fragmented character of current bioengineering research. This same difficulty in dealing with bioengineering is evident in the national academies. Neither the National Academy of Sciences (NAS) nor the National Academy of Engineering (NAE) has yet identified biophysics or bioengineering, respectively, as fields in their own right. In addition, in the Institute of Medicine (IOM), whose charter mandates that 25 percent of its membership be nonphysicians involved in medically related areas, engineers and non-M.D. scientists* constitute just 2 percent of the membership. Of the 22 regular and senior members of the engineering group, only 5 are non-M.D. biomedical engineers.

The sparse representation of bioengineers in the academies does not, however, connote a lack of interest or effort. The Commission on Physical Sciences, Mathematics, and Resources of the National Research Council—the National Research Council is the principal operating arm of the NAS and NAE —and the Board on Health Sciences Policy of the IOM are attempting to promote research collaboration between the engineering and biological sciences. The Board of Health Sciences Policy regularly ranks such collaboration at or close to the top of its priorities, including an interest in enhancing the physical underpinnings of a medical education.

Changing Emphasis in Funding

Because of the increased competition for limited research resources, government agencies involved in supporting bioengineering research have tended to shift from a philosophy in which research grants are seen as instruments for investment to one in which grants are considered a means to procure a product. The losers in such a shift are the imaginative but speculative ideas. Possible remedial steps include

- earmarking funds for basic research from which no immediate products are expected;
- setting aside awards based mainly on an investigator's track record to provide opportunities for more speculative research; and

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* Mathematicians, physicists, and chemists.

- extending the length of grant awards to 5 or 7 years to ensure stability and the opportunity to bring a project to a successful conclusion.

Industry Involvement

Biotechnology

The worldwide market for biologically derived products could be as high as $100 billion annually by the year 2000* (or about 15 percent of the estimated total annual market for chemicals) (National Academy of Sciences, 1984). Since about 1978 there has been a virtual stampede of private investment in new, entrepreneurial companies entering this field. Venture capital has been an important source of private capital, funding well over 100 biotechnology firms —many of which have not survived in this highly speculative field in which few products have yet come to market. R&D limited partnerships, only one source of funds, totaled $1.5 billion in 1984 alone (Office of Technology Assessment, 1984).

Many of these small entrepreneurial firms focus on producing one or a handful of products derived from academic research. As such, their challenge is to perform the engineering research necessary to make an idea commercially viable, because this is the type of research that is not often sponsored by federal agencies at universities. Because the work is limited in scope (and highly proprietary), it is not of great use in the overall advancement of the field. Of greater potential value in this regard is the engineering research performed by established industries.

The pharmaceutical industry was the first to seek applications of the new biology on a large scale. Biologically engineered products have traditionally accounted for roughly 23 percent of annual sales of pharmaceutical products (Drew, 1985). With domestic expenditures for health care now exceeding 10 percent of the gross national product and the world market for human and animal drugs exceeding $35 billion, this is likely to remain one of the most active areas of research in biotechnology.

The key problem for this and other industries involved in

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* In 1984 constant dollars.

biotechnology is likely to be performing the engineering research necessary to capitalize on the explosive growth in the biological sciences. According to Drew (1985), the current focus of engineering research on process development and scale-up to manufacture has kept pace with new product discovery, but the margin of comfort in completing process development before licensure has dwindled. The trend toward more complex product chemistries, higher product purities, and increased product stability will only worsen the problem and place process economics under greater constraints.

Biomedical Engineering

Most of the research in biomedical engineering to date has been carried out within the academic research community through government funding, private philanthropy, and limited industrial cosponsorship. In recent years, as the commercial potential of a line of research has become clear, industrial research in some cases has brought the basic work beyond the proof-of-concept stage to product development. Yet, here again, the fundamental work leading to the proof-of-concept stage is the key. Despite efforts to increase industrial sponsorship of such research at universities, little additional support is anticipated. The nature of this industry dictates that investments in R&D are made only after the concept has been proven and the market potential shown to be adequate to provide a return on investment.

One drawback to the advancement of this field appears to be a prevailing belief, on the part of the medical community, that industry should be responsible for medical instrumentation R&D. In fact, industry does not support R&D on medical instrumentation or equipment that is used in research per se, but only that which is addressed to the clinical market.

These factors will likely combine with a limited market for many types of instrumentation and devices (e.g., prostheses) to keep industry's investment in research small. Because the public health and welfare are traditionally a responsibility of government, an adequate research base in biomedical engineering should continue to be maintained through federal support of university researchers.

Bioengineering in Other Countries

As part of its study, the Panel on Bioengineering Systems Research examined bioengineering research and education in the United States, Western Europe, and Japan. In particular, the analysis pointed up major differences in the magnitude and organizational structure of government-supported research and development efforts in biochemical engineering. West Germany, Japan, and Great Britain each have three government institutes that support biotechnology exclusively. These nine institutes bring together academic and industrial investigators, feature cross-disciplinary activities, and have impressive operating budgets.

In West Germany, for example, the Gesellschaft für Biotechnologische Forschung had an operating budget of $14 million in 1983 and the Institut für Biotechnologie II in Julich had a budget of $4.3 million for biochemical engineering alone. The last figure nearly equals the NSF's entire annual budget in biochemical engineering. West Germany also has nearly double the amount of space and equipment for bioengineering research found in the United States.

In Japan, the Ministry of International Trade and Industry 10-year plans have strong R&D components in biotechnology. Three of the nine areas emphasized are bioreactors, animal cell culture, and membrane separations. The Japanese government budgeted $20 million in support of membrane separation research and development alone in 1983. NSF, by comparison, contributes less than $1 million for similar research in the United States.

Thus, countries such as West Germany and Japan are laying a foundation of research and trained personnel as part of their strategy for meeting the intense international competition in biotechnology. Given the potential size of the worldwide market described earlier, the economic rewards for success are likely to be very great. Getting into these markets first will be critically important in international competition. Major shares will be captured by countries that have the needed national capability in research and personnel.

Availability of Bioengineering Research Manpower in the United States

Because bioengineering is a relatively new field, and one with great potential for research breakthroughs and commercial devel opment, there is a clear need for training young engineers who understand the major principles of biology, medicine, and other relevant scientific disciplines, and who can communicate readily with researchers in those fields. This human resource will develop only with long-term support for bioengineering research. Funding from NIH, NSF, and other government agencies must be stable, continuous, and sufficiently large to train and support an adequate number of new investigators in bioengineering.

Biomedical Engineering

Although the total number of biomedical engineering graduate students in U.S. colleges and universities has increased by about 50 percent during the last 10 years, the number relative to all engineering graduate students has remained about the same.* Over the past decade, biomedical engineering students have averaged less than 2 percent of all engineering students enrolled in both master's programs and doctoral programs. Recent trends in enrollment of graduate students in biomedical engineering specifically show a relative increase in master's and a decrease in doctoral students. The decline in Ph.D. candidates may reflect a loss of students to medical schools or other engineering disciplines with competitive popularity or better research funding.

Although Ph.D. biomedical engineers are the primary resource for future research in this field, they are not the only resource. Interdisciplinary doctoral programs in biomedical engineering are relatively new. Indeed, most of the current leaders in the field are doctoral-level researchers in another engineering field (e.g., electrical, mechanical, chemical, or even civil) or in a medical speciality who became interested in applying their expertise to developing new knowledge, techniques, or equipment in the biomedical field. Often pairs of Ph.D. researchers with complementary, in-depth knowledge of engineering and medicine can offer an especially powerful capability for creative research in biomedical engineering when they work together on research of mutual interest. For this reason, the ability to understand and communicate with investigators in related areas of science and engineering is as important as formal interdisciplinary education in producing a pool of highly qualified bioengineering researchers.

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* According to calculations made by the panel, based on data published by the American Association of Engineering Societies over the period 1975–1985. (See, for example, Engineering Manpower Commission, 1985.)

Biochemical Engineering

Studies indicate that there are not enough biochemical research engineers in the United States today to meet existing needs. The Office of Technology Assessment (OTA) and the Committee on National Needs for Biomedical and Behavioral Personnel of the IOM surveyed the biotechnology industry in 1983 (Office of Technology Assessment, 1984). The OTA-IOM survey found that more than 80 percent of the existing firms began operations in 1978 or later. About one-third reported shortages of Ph.D.s in one or more specialities. In bioprocess engineering, for example, 80 Ph.D.s were employed in the 138 firms reporting. They expected an increase of over 50 percent in the following 18 months.

Fewer than 20 departments of chemical engineering at U.S. universities and colleges have meaningful biochemical/biotechnology programs. Collectively, they are graduating fewer than 60 doctoral and master's students* annually (National Research Council, 1984). The annual need for graduate-level biochemical engineers over the next decade will average two or three times that number. Moreover, biochemical engineering students need to have better exposure to the biological sciences to improve their ability to manipulate and control cellular biosynthesis. They must also be taught the techniques and methods of the life sciences for solving critical, large-scale bioprocess problems. Learning the "new" biology is important to improving the relationship between the life sciences and biotechnology.

There are likely also to be shortages in the faculty needed to train biochemical engineers. Given the powerful allure of industry research in biotechnology, it cannot be expected that a large percentage of the few new doctoral graduates will join university faculties. Yet these are precisely the people who are needed to train tomorrow's researchers, because older faculty are not as conversant with the new technologies or as likely to produce new ideas in research.

There is little reason to expect that this situation will solve itself. Demographic studies show that the overall number of science and engineering students will decrease by 25 percent over the next 15 years. The loss of tuition that this reduction represents will pose a financial strain on already hard-pressed universities. Many will probably respond by curtailing hiring and laboratory

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* Few of these students terminate their studies at the master's level. Most go on to obtain the doctorate.

improvements. Already the proportion of science and engineering faculty who are recent graduates has decreased from 40 percent in 1968 to 20 percent by 1980. An expanding program of research support (particularly for young investigators) is urgently needed to ensure that university bioengineering research continues to keep the United States in the forefront of knowledge in this field.

CONCLUSIONS AND RECOMMENDATIONS

Despite significant advances in the biological and biomedical sciences in recent years, much remains to be learned from bioengineering research. Our basic understanding of biological systems and our ability to use that knowledge to produce economically products that can enhance the quality of life and permit the delivery of better health care can be greatly furthered by such research.

However, the focus of attention and funding for bioengineering—as opposed to biomedical science—research in the U.S. government is scattered among several agencies, as well as among different divisions and programs within those agencies. To date, little effort has been made to coordinate the work of those agencies and programs. One coordinating body established in the past has not been very active and consequently has been relatively ineffective because, in the panel's judgment, it has not received sufficient support and attention from upper-level management in the participating federal agencies. Thus, the Panel on Bioengineering Systems Research recommends that:

- The coordination of research programs in bioengineering throughout the federal government be improved and that the coordination include the relevant work of the NIH, NSF, NBS, and other agencies interested or involved in bioengineering research. To ensure its effectiveness, it is essential that this interagency coordinating effort receive the full recognition and support of upper management in each of the participating agencies.

Similarly, there is little coordination of bioengineering research within those agencies that support most of the relevant research programs within the U.S. government. This is particularly true of

NIH, which has primary responsibility for biomedical and related research in the government. Thus, the panel recommends that:

- The coordination of bioengineering research *within* supporting agencies should also be improved. For example, NIH could create an "office of bioengineering research" to review and coordinate its investigations. Such an office would be one way to focus attention on the need for careful development of this important field.

NSF has recently created six ERCs to promote cross-disciplinary studies and to encourage the more rapid implementation of research results into commercial products. One ERC focuses exclusively on biotechnology. Although NIH supports that center, it has not established similar centers for biomedical engineering research. Thus, the panel recommends that:

- NIH should give careful attention to the need for large-scale, focused research in biomedical engineering. To achieve the required scale, NIH should consider creating a center for such research, comparable in concept and size to the NSF's ERCs. As is true of the ERCs, any such center created by NIH should encourage links between academic research and clinical practice.

There is demand for a wide spectrum of researchers across the different specialties of biomedical and biochemical engineering. Certainly the demand for doctoral biochemical engineers in particular is now large and will increase. Because bioengineering is a relatively new field with great potential for research breakthroughs and commercial development, there is a clear need for training young research engineers who understand the principles of biology, medicine, and other relevant scientific disciplines, and who can communicate readily with researchers in those fields. Therefore:

- NIH and NSF should devote a greater share of their research budgets to supporting bioengineering programs at U.S. universities and colleges. NIH, in particular, should expand its programs that encourage medical students to go into research so as to include bioengineers as well. Stipends awarded for postdoctoral study should be sufficiently high to offset the attractiveness of salaries offered by industry.

Although it is understandable, too often government and industry support of research at universities aims at achieving practical results that can be turned into commercial products. This is not to be discouraged, because the U.S. economy and competitiveness in international markets require such efforts. Nevertheless, there is also a need for basic research that does not necessarily lead to commercial products but, in the aggregate, creates the basis for practical results by increasing our understanding of biological systems. Thus, the Panel on Bioengineering Systems Research recommends that:

- Those who rank and award grant proposals within NIH and NSF should consider funding research projects that have a great potential for significant results, but also a high risk of failure. The panel applauds the strong current trend in this direction at NSF.

The need for a continuing study of bioengineering research programs and issues will not end with this panel's report or the work of the Engineering Research Board. The subject will be with us for many years to come. Thus, the Panel on Bioengineering Systems Research recommends that:

- To ensure the long-term health of this commercially important field, an authoritative and permanent advisory body should be created. This body would assess bioengineering research opportunities and needs, review the relevant research programs of NIH, NSF, and other government agencies, and identify needs for new programs or changes in direction.

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APPENDIX RESPONSES TO THE ENGINEERING RESEARCH BOARD'S REQUEST FOR ASSISTANCE FROM UNIVERSITIES, PROFESSIONAL SOCIETIES, AND FEDERAL AGENCIES AND LABORATORIES

Requests for assistance were sent by the Engineering Research Board to a number of universities, recipients of Presidential Young Investigator Awards, professional societies, and federal agencies and laboratories in order to obtain a broader view of engineering research opportunities, research policy needs, and the health of the research community. Some of the responses included comments on engineering research aspects of bioengineering systems; these responses were reviewed by the panel to aid in its deliberations. The panel found the responses helpful and wishes that it were possible to individually thank all those who took the time to make their views known. Because that is not practical, we hope that this, albeit small, acknowledgment conveys our gratitude.

Responses on aspects of bioengineering systems research were received from individuals representing 47 different organizations, which are listed in Table A-1: 28 universities (including 6 represented by recipients of NSF Presidential Young Investigator Awards), 12 professional organizations, and 7 federal agencies or laboratories. Some comments covered specific aspects of the panel's scope of activities, whereas others provided input on a variety of subjects.

Although most of the responses addressed priority research needs, several respondents did touch on policy issues. Many of the research needs and policy and health issues addressed by the respondents were similar to those noted by panel members. The broadened perspective provided by the responses to the survey was most beneficial in the panel's deliberations.

Table A-1 Organizations Responding to Information Requests Relevant to Bioengineering Systems Research

UNIVERSITIES	PROFESSIONAL ORGANIZATIONS
California Institute of Technology	American Chemical Society
Case Western Reserve University	American Institute of Chemical Engineers
Cornell University	American Society of Agricultural Engineers
Johns Hopkins University	American Society of Civil Engineers
Lehigh University	American Society of Mechanical Engineers
North Carolina State University	Biomedical Engineering
Northwestern University	Society Council for Chemical Research
Oregon State University	Institute of Industrial Engineers
Purdue University	Industrial Research Institute
Rensselaer Polytechnic Institute	Society of Engineering Science, Inc.
Rutgers University	The Institute of Electrical and Electronic Engineers, Inc.
San Diego State University	U.S. National Committee for Biomechanics
Syracuse University	AGENCIES AND LABORATORIES
Texas A&M University	
University of California, Davis	
University of California, Los Angeles	Air Force Office of Scientific Research
University of Georgia	Army Research Office NASA
University of Illinois	Goddard Space Flight Center
University of Illinois—Urbana/Champaign	NASA Jet Propulsion Laboratory
University of Kansas	NASA Langley Research Center
University of Michigan	Naval Research Laboratory
University of Minnesota	Oak Ridge National Laboratory
University of Pennsylvania	
University of Pittsburgh	
University of Rochester	
University of Texas at Austin	
University of Utah	
Wayne State University	

Construction and Structural Design Systems Research in the United States: An Overview

EXECUTIVE SUMMARY

This study viewed construction and structural design as principal components of the profession of civil engineering. A review of the history of each component discloses that research has received less attention in construction than in structural design, although there are a number of instances in which past research accomplishments have profoundly influenced construction practice. Traditionally, the role of the federal government in sponsoring research in the fields covered by this panel has been disappointingly limited; recent trends in this support are very promising.

In further probing the status of research support in these fields, the panel found that the key factors influencing the degree of support relate to the nature of the product (i.e., it is one of a kind), the university status of the discipline (which is generally weak for construction), the relatively small amount of industrial support, and the indifferent attitudes on the part of the public and in the industry itself concerning research on construction and structural design. The question of the adequacy of new research talent is also explored. It is found that, overall, the supply of research-oriented graduates is sufficient to carry out the current level of research. As we look at research areas directed toward the use of advanced technologies such as robotics, however, it is evident that there is an undersupply of research-oriented graduates. Moreover, a general shortage of research talent would result if funding for research increased to levels comparable to those in other engineering fields and to levels needed to meet the challenges posed by important new research opportunities.

Within this generally discouraging climate, a number of important research thrusts are not receiving adequate attention. Those identified by the panel as being of highest priority (see the next section) are (1) construction robotics, (2) computer-aided design, (3) rapid excavation, (4) mixed structural systems (involving the use of a combination of structural materials—e.g., structural steel and reinforced concrete—acting together to resist loads), and (5) marine construction.

These research areas encompass a broad range of applications, in some cases overlapping the goals and technological scope of other panels of the Engineering Research Board. They hold out the promise of advances in the efficiency, rationality, and competitiveness of modern construction and structural design practice in the United States. Such advances would not only improve the quality of life of most Americans, but would also enhance the position of American companies in the increasingly competitive and technology-intensive construction industry worldwide. Finally, the panel believes that the research thrusts identified in this report offer a realistic hope of obtaining the support of public agencies and industry.

RECOMMENDATIONS

Measured against annual U.S. construction expenditures of some $200 billion, federal support of construction and structural design research is extremely limited. Therefore, the panel recommends that:

- Federal funding for engineering research in construction and structural design should be substantially increased. Such research should be supported through new programs within the federal mission agencies as well as through the National Science Foundation (NSF). A suitable approach would be to establish centers for construction research, perhaps along the lines of the NSF's Engineering Research Centers. The existing capacity of federal and national laboratories for construction-related research should also be more fully utilized.

The nature of research is such that achievements of profound long-range importance often result only from the vision of an individual who is not allied with the mainstream of the industrial process or current thinking. Relatively small but highly innovative research investigations are essential to the health of research in this or any other field of engineering. Therefore:

- The funding of large research centers should not disrupt the continuity of funding for novel, individual research efforts. In particular, the general scheme of NSF sponsorship should continue to provide mechanisms to support this type of research.

The construction/design industry is fragmented, as it comprises a very large number of (mostly small) firms. Given this structure and the "one-of-a-kind" nature of projects, there has traditionally been little support for long-range research within the industry. Yet the advent of new technologies and increased international competition for the global and domestic construction market offer both an opportunity and a challenge that demand more attention to research. As a result, the panel recommends that:

- The professional societies and trade associations should inform their membership as to the need for research and should attempt to organize sponsorship and support for industry-wide collaborative research efforts. Such research might be performed at regional R&D centers and could include applied research aimed at making research advances more directly applicable to the industry's needs.

In general, the supply of doctoral researchers in construction and structural design is sufficient to carry out the current level of research. However, there is a shortage of researchers able to adapt new technologies such as robotics to the special needs of the field. In addition, if research funding were to increase significantly, the supply of researchers would quickly prove inadequate. Therefore:

- More students—especially U.S. residents—must be attracted into the high-technology aspects of construction and structural engineering research. The primary means of doing this would be to increase the funding of research. In order to attract more students into graduate study in the field, graduate student stipends should be raised to approximately $15,000 per year, in 1986 dollars.

Part of the reason for the traditionally low support for research in this field is the widespread public perception that design, construction, and maintenance are unsophisticated, low-technology endeavors. Thus, the panel recommends that:

- The professional societies and trade associations should increase their public relations effort devoted to informing the public of the intellectual challenges and achievements represented by modern construction projects of every kind.

The panel identified the following important or emerging areas of construction and structural design research as being especially worthy of support:

- construction robotics—extending and expanding the functions and capabilities of industrial robots to meet the needs of the construction environment;
- computer-aided design—achieving the potential that computer-aided design offers for structural design in such areas as improving nonlinear behavior and analysis, modeling geometrical complexity, enabling better coordination of analysis and experiment, improving realism in design analysis, advancing interactive computer graphics, and extending computer-aided design through the fabrication phase;
- rapid excavation—increasing the speed of tunneling and improving the systems by which ground is classified;
- mixed construction—improving our basic understanding of the characteristics and uses of mixed construction (especially reinforced concrete in various combinations with structural steel); and
- marine construction—finding new and better ways to build coastal structures of all kinds and to protect them from the severe coastal environment.

INTRODUCTION AND BACKGROUND

The fields of construction and structural design are complementary elements of the profession of civil engineering. Construc tion is strongly associated with the management of people and resources and deals with the actual environment, whereas modern structural design is founded in mathematical analysis and is based on a modeling of the presumed environment. These fields are drawn together at the interface between construction and design, and again during the subsequent construction process whenever the model and reality are at odds. Moreover, the trend in related engineering research—driven by developments in computers, sensors, and robotics—is toward a greater unification of the design/construction process than has heretofore prevailed.

Construction has had, on the surface, less of a tie to research than has structural design. Yet certain research has affected construction profoundly. Concepts of ready-mixed concrete and prestressed concrete, of cold-formed, light-gage steel, and of connections in steel, to mention just a few, were transformed into routine practice only after theoretical and laboratory tests had established their feasibility, criteria, and operational procedures. Nevertheless, measured against the vast annual U.S. construction expenditures—estimated by some sources to be $200 billion per year—construction research budgets are miniscule. In addition, there are only a handful of construction research centers in the United States, such as the U.S. Army Corps of Engineers' Construction Engineering Research Laboratory and Waterways Experiment Station at Vicksburg, Mississippi; the National Bureau of Standards' Center for Building Technology at Gaithersburg, Maryland; and the U.S. Navy's Naval Civil Engineering Laboratory at Pt. Hueneme, California.

The relatively low level of construction research in the United States can be compared with the much higher level in some other countries. For example, each of the six largest architectural/engineering/construction firms in Japan maintains a large engineering laboratory; there are no U.S. counterparts to these laboratories. Reportedly, the Japanese government suggested to these companies that they apply a certain percentage of their revenues to research. The largest of these laboratories has a work force of 400–500 people and excellent research and testing equipment. The rapid use of its own R&D makes the parent company more receptive to the developments of others. Each major company has a technical staff that offers a client all required services, from initial concept to completed structure.

Research by Japanese government agencies is mainly of an applied nature. However, the government also devotes significant efforts to fundamental research, and the results of this research are implemented rapidly. New construction research national laboratories have recently been built in Japan. One large construction research facility was built at Tsukuba during the late 1970s. This facility covers research on buildings, bridges, etc., and is prepared to do large-scale testing. The facility is reported to have cost $350 million. In addition, in the early 1980s the Nuclear Power Engineering Test Center was constructed in the city of Tadotsu, in Shikoku. The dominant feature of this facility is a large-scale, high-performance vibration table, which can shake a specimen weighing 1,000 tons with accelerations comparable to strong earthquakes. This facility is reported to have cost in excess of $200 million. It is anticipated that construction research carried out in these two Japanese facilities will have an important influence on the construction industry in Japan, as well as on the field of structural engineering generally.*

Structural design research, on the other hand, has a long history of activity, even if organized support for it does not. It is built on centuries of experience and empiricism, predating Roman times, so that even the most modern structure rests in part on this early "research" in design. Design thus involves a combination of experience and innovation, with (today) experiment and mathematical analysis providing both inspiration and backup.

Modern structural design analysis had its beginnings early in the nineteenth century. Since that time, progress has advanced on a wide front, both geographically and topically, and there are many research groups. Important contributions can be made by such small groups and individuals, so thousands of individuals labor at structural design research throughout the world. The role of mathematics and computation in structural engineering has progressed from one of just analysis into an active role in design, for example, in the proportioning of members. Yet it is widely recognized that the latter capability is in its infancy when consideration is given to the overall goal of efficient, reliable, and economical design. Now, as in the past, the first steps in design—the choice of type of structure and construction material—are the keys to good

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* National Research Council. Earthquake Engineering Research—1982. Washington, DC: National Academy Press, 1982.

design. Concepts of design theory and expert systems that could permit improved preliminary designs have barely surfaced.

The history of research sponsorship in construction and structural design has been similar for both disciplines. Prior to the First World War, research in both areas was essentially unsponsored. Achievements were recorded by individuals in universities or private practice out of intellectual curiosity, as part of the expected work of a professor, or in pursuit of a potentially profitable idea. After that time, industry took a greater interest through trade and professional associations such as the Reinforced Concrete Research Council, the Welding Research Council, the Structural Stability Research Council, the American Iron and Steel Institute, the American Institute of Steel Construction, the Prestressed Concrete Institute, and the Portland Cement Association. Government also got involved through such agencies as the U.S. Army Corps of Engineers, the Federal Highway Administration, the National Bureau of Standards, the Bureau of Reclamation, the National Aeronautics and Space Administration, and the research offices of the respective branches of the armed forces. Government and industry R&D was a mixture of work supported at universities and internal research in units such as the Portland Cement Association Laboratories, U.S. Steel's Monroeville Laboratory, and Bethlehem Steel's Homer Laboratory.

It is fair to say that none of these sponsoring units can be classified as large in either the size of their research expenditures or the intrinsic value of the supported projects. Much of the work that was done was also supported indirectly by universities or individuals.

A major source of support—at least for structural design—has arisen in the past 20 years via the NSF. Still other support has come from such agencies as the Office of Naval Research, the Air Force Office of Scientific Research, and the Army Research Office, although to a more limited extent than in fields directly related to the missions of these agencies.

In construction, an extensive amount of R&D has been done by individual companies, often on a relatively short-range basis. In order to solve an immediate problem, companies frequently develop innovative ideas "on the job." Lightweight, nonshrink, expansive structural concrete, the slurry trench wall method of supporting sidewalls during excavation, tunneling machines, and the application of prestressing and pretensioning to concrete piles were all developed in this manner by individual American companies. Although this pragmatic approach suffers from high costs and a high incidence of very expensive failures, the disadvantages have often been offset by the advantages of immediately transfering R&D into actual practice.

As noted previously, structural design supports subsequent construction activity on projects such as tall buildings, manufacturing plants, dikes, roads, dams, bridges, tunnels, and fixed marine structures. The role of structural design in such fields as automotive, ship, and aerospace design is extensive; work in these areas has often been the starting point for research that eventually proved to be critically important to other applications, including construction. Moreover, the structural design of mechanical devices for operation under severe environments and loading conditions has spawned research with a beneficial side-effect on structural design related to civil engineering construction.

This panel has, for the most part, limited its attention to the traditional applications of structural design; it has also sought, however, to identify research issues and directions that are broadly applicable to structural engineering. The panel did this because it supports the objective of the Engineering Research Board to emphasize, among many needed specific research directions, a limited number of major thrusts. The panel received suggestions from over 100 individuals and organizations, including engineering deans, research laboratory directors, and Presidential Young Investigators. These external inputs described specific needs and also some research avenues of key importance to the vitality of construction and structural design research in general; thus, these inputs were valuable aids in identifying specific major thrusts.

POLICY ISSUES REGARDING FEDERAL SUPPORT OF RESEARCH

Laboratories of government agencies, or laboratories with close ties to the federal government, such as those cited earlier, are well-established contributors to research in construction and structural design. When specific major thrusts in engineering research are adopted as national objectives, the policy should be to effect the full participation of existing federal laboratories whose research emphases are relevant to those thrusts.

A different role is played by federal executive agencies, known as "mission" agencies (e.g., the Department of Defense and the National Aeronautics and Space Administration) in their interface with independent research laboratories and universities engaged in research. Unfortunately, construction and structural design have had low visibility within the mission agencies. This may be due to the fact that, whereas many agencies are involved in construction, construction is not the central mission of any agency. Therefore, there is less sponsorship from the mission agencies for construction research than there is for other fields of research.

It should be emphasized, however, that the mission agencies have demonstrated the effectiveness of their support of construction research—especially in the case of projects whose success depended on the field experience and data collection opportunities that can be found only on-site. Results from these research projects eventually had, in many cases, a strong impact on prevailing construction procedures.

During the past few years the NSF, whose role in the support of basic research is vitally important, has given emphasis to the sponsorship of projects that feature partnerships between universities and industry. This is currently evidenced in the Engineering Research Centers (ERCs) program. Because such centers are the basis for the type of major programs recommended in this report, it follows that the ERCs concept has the panel's support. Indeed, one or more ERCs directed specifically at research in construction and structural design would do much to fill a notable gap in the nation's overall engineering research effort.

Nevertheless, the panel is concerned about the continuity of funding for innovative research investigations that generally involve the efforts of an individual investigator and just one or only a few graduate students. As this report was being written, word was received of a new NSF program, entitled "Expedited Awards for Novel Research," established to fund innovative research. This program provides modest funding for such investigations on a 1-year basis without having to go through the normal proposal process. This is an excellent initiative and the panel hopes it will be given every chance for success.

The nature of research is such that certain accomplishments of profound long-range importance often result only from the vision of an individual who is not allied with the mainstream of the industrial process or current thinking. The continued existence of a mechanism to support this type of research is a key to the health of the overall research environment. Consequently, in respect to federal policy issues, it is urged that the general scheme of NSF sponsorship continue to provide mechanisms that encourage the individual researcher.

Furthermore, the clustering of research resources to programs of the type described the section on "Especially Important Areas of Construction and Structural Design Systems Research" should be designed so as not to eliminate opportunities for a nationwide range of institutions to contribute to the success of engineering research. With emerging interactive techniques for networking, teleconferencing, etc., there is ample opportunity to involve multiple institutions in clustered, synergistic research efforts.

ISSUES THAT DETERMINE THE HEALTH OF THE FIELD

The Adequacy of New Research Talent

As in other fields, the health of research in construction and structural design is fundamentally dependent on the existence of the human talent needed to identify research projects and successfully complete them.

Civil engineering is the primary discipline from which researchers and practitioners in this field are drawn. Undergraduate enrollments in civil engineering have been declining, relative to virtually all other branches of engineering, for the past 10 years. Recent indicators show a decline of enrollments in absolute terms as well. This might suggest that the future availability of research manpower is threatened. However, that is not necessarily the case. In engineering, when job opportunities in a certain field become less attractive at any time, there is a tendency for students either to go into another branch of engineering or to continue on into graduate studies rather than enter the work force. As a consequence of the latter tendency, graduate enrollments in civil engineering have risen in recent years.

The most significant rise has been in master's degree enrollments. Yet, although graduates at this level contribute to research to a certain extent, in general they do not contribute to the pool of research talent per se. The necessary exposure to research takes place mainly at the next level, as a result of doctoral studies. The number of doctoral degree recipients in construction and structural engineering has increased moderately. It can be said that, as a result of this increase and the decrease in undergraduate enrollments, the "crisis" seen in other fields (e.g., electrical engineering) with regard to the number of Ph.D.s available for research and teaching is not present in civil engineering. The supply of doctoral researchers would appear to be sufficient to carry out the current level of research in construction and structural design.

However, an examination of the adequacy of new talent in a field that is based principally on the overall output of advanced degree recipients and on trends in the size of that output misses a key point. The research directions defined in the next section identify specific technologies in which research specialists are scarce. A field such as construction robotics, for example, obviously demands expertise in both construction and robotics. It is well known that graduate education is producing too few research specialists in robotics, among whom are only a miniscule number whose interests (let alone their studies) are oriented toward construction. Thus, the challenge is to attract more students into the special, high-technology aspects of construction and structural engineering research. Fundamentally, the basis of attraction must be the existence of programs that hold out the promise of exciting careers. The research recommended the next section would certainly be a basis for such programs.

Concomitantly, the attitudes of civil engineering students toward advanced research of this type need to be shaped in a positive way. The long-term health of the field demands that the education of civil engineers include extended experience with computers and with technology related to computers, not merely computer programming. Given the one-of-a-kind nature of constructed systems, civil engineers must have sufficient knowledge of the new technology to use and modify it in ways appropriate for design and construction. Curricula should be designed to enable the next generation of civil engineers to make appropriate technical choices and to develop solutions that integrate computers and operational processes (e.g., in construction robotics, to have sufficient knowledge of the state of the art in semiconductors and microelectronics). Greater familiarity with advanced technologies should enable civil engineers to take the lead in defining research needs in areas such as construction materials, "intelligently constructed" facilities, and construction in challenging environments such as space or the Arctic.

The adequacy of talent is also clearly relative to the demand for researchers. If funding for research in construction and structural dynamics is increased to levels necessary to address the research opportunities described in this report—and certainly if it were to increase to levels comparable to those in other engineering fields—a shortage of research talent would quickly be apparent. Emphasis would have to be placed on rapidly "growing" the needed talent from within civil engineering.

Another point must be raised in connection with the research talent in construction and structural design, a point incidently that is shared with nearly all other engineering branches. There is a predominance of foreign graduate students in the current enrollment profile. Indeed, such students are even more prevalent in civil engineering because of the significant role that this field plays in less developed countries, which are the main source of foreign graduate students. The flow of these students back to their home countries is large, given the opportunities that exist there in construction and related disciplines.

To resolve the abovementioned problem while maintaining an adequate supply of researchers, the conditions of engineering graduate study must become more economically acceptable for recipients of U.S. baccalaureate degrees. The average starting salary in industry for graduates with B.S. degrees greatly exceeds graduate student stipends. More important, stipends are below the level needed to sustain a reasonable standard of living over the requisite 3–5 years needed to earn a Ph.D. Means must be found to raise the stipend level, on a 12-month basis, to approximately $15,000 in 1985 dollars. In addition, the approach to providing this funding should incorporate a scheme for cost-of-living adjustments in future years.

Factors Affecting Research Support

Beyond the issue of available talent, the panel identified five other factors perceived as underlying research sponsorship. These are

1. the nature of the product;
2. the discipline's status at universities;
3. industry sponsorship of research;
4. attitudes of practitioners and of the public; and
5. the perception of a permanent U.S. market dominance.

These factors, which are listed without any implication as to their order of importance, are discussed in the following sections.

The Nature of the Product

Civil engineering structures are largely one-of-a-kind items. With some exceptions, there is no time or money allocated in a design or construction contract to conduct desired research. This characteristic of the construction product contrasts with many mass-production items that do command allied research efforts, as well as with certain limited-production endeavors (as in the aerospace industry) in which research allocations are a natural and inescapable part of the design process.

The Discipline's Status at Universities

With regard to the discipline's status within universities, there are notable differences between construction and structural design. Both, however, confront substantial difficulties.

Construction research is in a decidedly unhealthy condition. Very few universities have viable research programs in construction per se. Existing programs have to justify their academic credentials and credibility continually in terms of the criteria for evaluating research and the selection of proposals. It is becoming increasingly difficult to find a niche of academic respectability for education and research in construction. A lack of understanding of the realities of design and construction and submission to the prevailing mores of the institution on the part of civil engineering faculties both seem to be at fault. To a certain extent, with respect to construction, the familiar charge that faculties are becoming "too theoretical" demands consideration. Many features of construction lie beyond theory. The separation between the university and practice—at least design and construction practice—has become very great, and it is difficult to see how the conflicting demands for elegance and practicality can both be met.

Structural design systems research, as opposed to structural behavior research, is also unhealthy, for reasons similar to those outlined for construction. It is generally viewed either as "computer science" research disguised as civil engineering or as a practical development not worthy of research.

Some components of construction and structural engineering research are relatively healthy. Notable among these are mathematical modeling of phenomena (e.g., computational mechanics) and experimental work conducted to understand phenomena. They are "healthy" in the sense that adequate, generally accepted criteria are available for evaluating research and selecting proposals. One may argue that these criteria are external to the field, that is, that they are based more on the mathematics and experimental physics used in the research than on the relevance of the research to the field. Nevertheless, some aspects of construction and structural design research do enjoy recognition and support.

Industry Sponsorship of Research

Industry research sponsorship has grown in recent years in such fields as manufacturing systems, materials science, and electronics. No such growth can be discerned in construction and structural engineering. Moreover, the existing level of sponsorship is relatively small. Indeed, given the current economic difficulties of the construction industry and other related industries (e.g., steel), industrial research sponsorship might very well decline in the future. A noteworthy exception is the recent emergence of research support by the Business Roundtable through the Construction Industry Institute at the University of Texas in Austin.

The abovementioned situation results, in part, from the fragmentation of the industry. Although the structural engineering construction business as a whole is huge, it is widely dispersed and most firms are quite small; there are very few large contractors.*

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* A recent article (Moavenzadeh, 1985) reports that the U.S. construction industry is composed of some 1,200,000 firms—of which 720,000 are so small they have no payroll.

The same is true to an even greater extent for structural design. It has been estimated that more than half of the steel structures built in the United States are designed in offices employing fewer than five professionals. Few organizations of either type have the resources to support a research staff.

Today, much attention is devoted to research in manufacturing, in expectation that the results will transfer directly into manufacturing practice. As we noted previously, one reason why this is a realistic expectation is that manufacturing practice includes large firms with high production rates. In construction, however, most of the small firms would be hard pressed to find innovative uses for the results of current research. However, further development and "packaging" of research results in a form appropriate to the needs of the small construction or structural design firm would be a useful step, as would be performing research directly applicable to those needs. The panel encourages the initiation of collective and collaborative R&D efforts along these lines, perhaps under the supervision of the professional societies and trade associations. Such efforts should be sponsored and strongly supported by the industry.

Attitudes of Practitioners and of the Public

Although structural engineers and contractors have the reputation of being conservative—that is, of doing mainly what has been done before and is already known to work—this reputation is undeserved, especially in construction. Large-scale construction projects are often creative and daring. The creativity is of a special type, wherein someone gets an idea, convinces him- or herself and the client that it will work, and does it—rather than the kind in which the idea is tested before being implemented. Advances in construction are apt to be made in this individualistic manner and not through systematic research. This "can-do" attitude of the profession is admirable, but it contains little sentiment for research.

In general, the public is ambivalent toward structural engineering and construction. On one hand, the nation took pride in celebrating the centennial of the Brooklyn Bridge, and there is widespread acknowledgment of the great importance of the country's "infrastructure" and recognition of the fact that it is currently in need of major renovation. On the other hand, the view is widely held that design, construction, and maintenance deal only with the brute manipulation of steel, concrete, and rock. There is little recognition of the intellectual challenges faced in such widely heralded projects as the New York World Trade Center, the Houston Astrodome, the Golden Gate Bridge, and earthquake-resistant tall buildings. Contracting practices in general leave little room for innovative approaches. Inflexible enforcement of building codes, and even the fear of aggressive malpractice claims, contribute to the inhibition of innovation in the industry. The wide public support enjoyed by other technological enterprises, and translated into action in the political sphere, is presently remote for research in construction and structural design.

Perception of a Permanent U.S. Market Dominance

The U.S. construction industry has always relied on an expanding domestic population and on its dominance of the international construction market—although relatively few U.S. firms participate in that market. That confidence in the ability and the future of the U.S. construction industry has contributed to the lack of any strong drive for research on the part of the federal government. Yet new ideas in construction are by no means the sole province of U.S. industry. The large tower cranes now seen in every U.S. city for building construction were commonplace in Europe much earlier; dramatic new developments in tunneling that have been made by the Japanese are giving them a beachhead in the United States. Our giant design/construction companies that depend on international markets face strong and increasing competition. Much of the engineering design for major international projects by U.S. concerns has moved offshore.

Indeed, foreign construction firms have already gained an increasing share of the overseas construction market. Of the total work done by the top 250 firms in the world, the U.S. share fell from 46 percent in 1980 to 31 percent in 1983 (*Engineering News Record*, 1984). Foreign construction firms are also beginning to enter the U.S. market, sometimes by buying U.S. construction companies. In the future, Japanese manufacturers of homes, using automated methods and new materials, may aggressively pursue the home construction market in the United States. The emergence of a highly competitive global market in construction, based largely on advanced technology, places a new and urgent emphasis on research in this field.

ESPECIALLY IMPORTANT AREAS OF CONSTRUCTION AND STRUCTURAL DESIGN SYSTEMS RESEARCH

As noted in the introduction, the panel benefited from a wealth of suggestions about lines of research that ought to be pursued. From its own deliberations, and taking these suggestions into consideration, the panel identified the following major thrusts:

- construction robotics;
- computer-aided design;
- rapid excavation;
- mixed construction; and
- marine construction.

The rationale for these thrusts, along with their detailed features, is presented in the following discussion.

Construction Robotics

The construction industry is one of the largest segments of the economy, yet it has chronically the lowest productivity rate among major industrial segments. This is because every facility built is not only custom-designed for its specific intended purpose, but also custom-built, largely on-site. Consequently, the industry is labor-intensive, and that labor operates in a hazardous environment, exposed to weather and other factors that all tend to reduce productivity and affect the quality of the work produced. With human labor still a major source of energy for such operations as lifting and installation, the size of components installed is governed by human physical capacity.

Construction robotics has the promise of significantly altering the construction workplace. The primary effect of construction robotics cannot be a simple one-to-one replacement of workmen by robots doing the same task. Rather, construction robotics must achieve several of the following objectives:

- extend vision (i.e., sensing) capabilities into new domains (e.g., underground, inside bulk concrete, inside small-diameter pipes);
- provide high-quality and highly controllable work (e.g., automatic welding with self-diagnosis and self-inspection capabilities, automatic drills with controls that minimize overshoot);
- extend the workplace of construction workers (e.g., underground, underwater, and into cold regions);
- increase the safety of and reduce health hazards faced by workers in all construction environments;
- provide mobility coupled with a very high degree of dimensional control over large, changing job sites;
- provide lifting and accurate positioning capabilities for very large payloads; and
- provide versatility over a wide range of functions, project sites, and project sizes.

Some of the requisite functions, notably sensing, control feedback, etc., are "inheritable" from industrial robotics. Many other functions are diametrically opposite to the needs of industrial robots (e.g., mobility, flexibility, and high payload-to-weight ratio). Therefore, significant research is needed to proceed from the present state of industrial robotics to the first generation of construction robots.

The first phase of research that will be needed is to identify, develop, and construct prototypes of classes of construction robots with the requisite capabilities. The next (or, perhaps, a concurrent) phase will be to investigate the feedback from such tools—that is, to develop modified design concepts and methods, as well as new construction materials and methods, that exploit the radically new construction processes that can be provided by robotics.

Computer-Aided Design

The phrase "computer-aided design" has been used so widely and loosely of late that it is now almost a cliché. This is unfortunate because computer-aided design is a serious activity that is becoming—and will remain—a central part of structural design. What has been accomplished so far is only the primitive beginning of what should eventually be a natural, engineer-controlled, computer-assisted design technology. Realizing the computer's potential for design will require a large amount of both goal-oriented engineering research and problem-oriented science research. The following discussion emphasizes engineering research.

Computer-aided design in structural engineering extends from a computerized sketch pad (a digitizing tablet and a dynamic graphics screen that an engineer and his client can use in exploring concepts and schemes) to the automated control of fabrication. Construction control and maintenance lie beyond this definition except to the extent that they involve redesign.

Among the reasons advanced for the use of computer-aided design are the following:

- it facilitates the study of alternatives;
- it frees the engineer from the burden of routine calculation and is essential to the use of advanced analysis;
- it can be an aid, through visualization via computer graphics, to the understanding of structural behavior;
- it can facilitate the use of experimental data and probabilistic considerations in actual design;
- it can be effective in the integration of the entire design process—from concept to the fabricated structure;
- it can be cost effective; and
- it can facilitate migration from present computation-oriented algorithmic approaches to future knowledge-based, reasoning-oriented heuristic approaches.

In general, these qualities remain pursuable goals rather than commonly achieved actualities. To make the potential reality, the following areas of research must be pursued.

Nonlinear Behavior and Analysis

Prior to failure, structures evidence nonlinear behavior due to plastic deformation of the material and large deflections. Much computer-related research is required before thorough nonlinear analysis of structures can become the profession's norm. Existing advanced methods of analysis can cope with nonlinear behavior, but they enjoy only limited application in practice. This type of analysis, however, has scope for further theoretical development, with the objective of making its results ever more faithful to reality.

Proportioning of Elements of Structural Systems

Many computerized design aids can be routine commercial developments, but others such as those that treat the effects of semirigid connections on system stability require bona fide research. Aids for the treatment of these problems would be sophisticated programs (probably with graphics) that would assist the engineer in understanding and translating reliable information on structural behavior into physical dimensions.

Coordination of Analysis and Experiment

Significant advances in design philosophy have been made in recent years; but more research is needed to produce truly rational designs. These designs must account for loads due to environmental conditions, such as wind and snow, that are not constant and uniformly distributed, but rather have a statistical, time-varying nature. The variation of size and material properties of the as-built structure also requires consideration. Probability theory must be brought to bear. Much of what is required can be described as computer-aided design research in which a comprehensive view of the whole analysis/design process is taken—including the facts that certain types of design procedures go with certain types of analyses and that applied loads and the resistance of the constructed structure may not be completely independent variables.

Realism in Design Analysis

There is a continuing need to advance the art of three-dimensional analysis and our ability to model geometrical effects wherever they occur—from elements with unsymmetrical cross-sections, through cross-section distortional effects, to gross system geometry complexities. This applies to structures of every size. For example, no one has ever analyzed a tall building (e.g., one of 70 stories) in its entirety for the full effects of earthquake, wind, or even gravity loads. Further, the chances are that in the foreseeable future no design office will be able to do so, even with advanced analysis methods, given the costs of computation and the limits on the capacity of the computers available to practitioners. Such buildings are being built, however, even though their response to an event such as a major earthquake is not well understood. Therefore, it is important to society that as supercomputers become available at research centers, one of their applications should be the study of the behavior, up through failure, of very large three-dimensional structural systems. If current expectations are realized, supercomputers could become national resources that could yield an understanding of large structural systems unattainable in any other way.

Interactive Computer Graphics

This topic has been the subject of active research for some time. Still, it is desirable to reaffirm that interactive graphics should continue to be the key to much of the most effective computer-aided design techniques, and that further engineering research is needed to advance such areas as the visual interpretation of nonlinear and dynamic behavior and the use of color in three dimensions.

Fabrication

Carrying computer-aided design through fabrication (the structural engineering equivalent of manufacturing) is another vital activity. Many developments in this direction are taking place commercially. The engineering research component comes more under the heading of project-wide integration.

Synthesis of Alternatives

Analysis-based computer-aided design tools are usable only after promising structural alternatives have been identified. Yet the key decisions are made and the major innovations are introduced at the earlier, conceptual stages of design when alternatives are first synthesized. Concepts from the newly emerging area of Design Theory and Methodologies of Knowledge-Based Expert Systems can be brought to bear on this task. Research is needed to expand these concepts and methodologies to meet the requirements of structural design.

Rapid Excavation

The estimated annual cost of domestic tunneling is between $0.5 billion and $1 billion. The 90 largest tunnels planned or under construction worldwide represent an investment of at least $74 billion (Neustadtl, 1986). Improved techniques derived from research could lead to substantial savings. Research to increase the speed of excavation should be a key objective.* The main driver here are interest rates, which (although they are currently relatively low) have escalated from 4 to 12 percent and even higher in the past 30 years.

There needs to be an integrated approach to tunneling accomplished by blending the knowledge and objectives of engineering geologists, designers, experienced tunnel contractors, and equipment manufacturers. Presently there is a widespread lack of understanding across the boundaries of these four disciplines.

A central problem to be resolved is the classification of the ground, including its rock content. With the four groups of engineers working together, the ground can be classified with much greater certainty than it was as recently as 15 years ago. The designer can customize his or her ideas of permanent support or lining according to the ground conditions. This can be done so that the system of construction designed for the ground by the machinery contractor can be used. Machinery construction system designs to fit classification of ground can be foreseen that will remarkably speed up tunnel construction.

Preconstruction classification of ground into 6–10 categories can be visualized with designs and contractor's systems and machinery to fit them. Thus, in rock-tunneling machines with greater thrust, the speed of rotation will be dependent on the size of the tunnel and the degree of jointing; faster machines can reduce support problems. Much time is lost in installing supports during excavation. Preclassifying the ground, and customizing the design, system, and machinery is an answer to this problem. Various means have been developed to install the tunnel liner immediately behind the advancing shield or excavation. Precast tunnel liners are mechanically installed in some Japanese systems. In other cases, fiber-reinforced concrete is extruded, or shotcrete applied, by a robot "gun."

In order to develop such a classification system, a major research effort is needed, entailing contributions by university laboratories, engineering geologists, and machinery manufacturers and

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* For example, the use of the new "earth-pressure-balance" method of tunneling enabled a Japanese firm to bid $5 million below the engineer's estimate for a sewer contract in San Francisco, and to complete the tunnel 3 months ahead of schedule (Moavenzadeh, 1985).

tunneling contractors. Projects must be selected to test the classification system. For best efficiency this program will require the guidance of a central laboratory or research center.

Mixed Construction

Mixed steel/concrete structural elements and systems are not new. Ordinary reinforced and prestressed concrete structures are, of course, forms of mixed construction. Concrete-filled pipe columns, composite bridges, and composite floor decks have been around for a long time. Turbine pedestals in conventional power plants sometimes consist of a structural steel framework embedded in rather massive concrete foundations. In less developed countries one finds crude latticeworks of light, open steel beams and columns that provide staging support during construction and integral reinforcement for the completed building after encasement in concrete. In Japan this type of building has become a highly developed method of engineered construction.

Recently, however, mixed construction—that is, systems that involve reinforced concrete and structural steel components that together resist all gravity and lateral forces—has taken on a new connotation. It is being used in new, innovative ways in high-rise buildings, bridges, and offshore structures to take advantage of the best attributes of both reinforced concrete and structural steel. Mixed construction possesses structural efficiency and economy, versatility, as well as structural stiffness and flexibility (in both the structural and architectural senses) where required. An example is the use of concrete shear walls or framed tubes for lateral load resistance, combined with steel floor framing.

Modern mixed construction is an example of practice outpacing theory. Creative engineers and builders have seen the advantages of mixing structural steel and reinforced concrete and are using this approach. Developments in research, codification, and professional and academic organizations have lagged. Much thinking is still conditioned by administrative, financial, and emotional ties to "concrete" (meaning ordinary reinforced or prestressed concrete) or "steel," without realizing that each is just a building material to be used naturally where it best meets the needs of a design problem.

A primary need is to break down the artificial barriers that impede the development of mixed construction. This goes well beyond research, but research is an important factor, for there are uncertainties in the interaction of steel and concrete that can be resolved only by intensive study. Dr. H. Iyengar has listed the main research needs for mixed construction in buildings. The following is an outline of his list, contained in a paper presented before the 1984 Tall Buildings Council Meeting:

- development of inelastic design methods for mixed systems in seismic areas;
- development of design criteria for concrete shear wall-steel space frame systems;
- mechanism of local transfer between steel and concrete in compression elements;
- evaluation of relative shortening between steel and concrete vertical elements;
- behavior of connections, and design methods for proportioning them; and
- development of a superior, rational design method for steel-concrete composite columns, such as the Structural Stability Research Council Subcommittee's method.

In addition, the durability of steel and concrete as construction materials requires research attention. The durability of construction materials is a broad issue that affects all types of structural designs. Progress in construction requires making the best use of steel and concrete in any situation and in any combination. A large body of engineering research must be accomplished before this can be done rationally and with confidence.

Marine Construction

The pace of construction along the coasts of the United States is accelerating, because of such factors as population growth, demographic changes, and urban concentration along the coastlines. At the same time, the yearly damage to existing structures (both public and private) from storms, tsunamis, and landslide/erosion is many billions of dollars. If the sea level should rise, the severity of this damage will increase exponentially.

Coastal construction is growing rapidly in terms of numbers of structures, exposure to more severe environments, and costs. It includes wastewater outfalls, power plant intakes and discharges, petroleum pipelines and terminals, fishing and recreational piers, seawalls, breakwaters, groins, etc.

The coastal environment is the most severe of all, in that both land and sea elements interact in a highly dynamic fashion. Coastal engineering and construction is emerging as a more critical national issue than offshore construction, because spectacular developments have already taken place in the offshore industry.

A host of research challenges have been identified, encompassing needs in such related fields as structures, geotechnical engineering, hydrodynamics, and naval architecture. The most outstanding of these needs are

- resolution of the problems of sand infill and sediment transport;
- prediction of the response of floating structures and pipelines on the ocean floor in the shallow-water and surf zones;
- the identification of approaches to the stabilization of beaches during and after construction;
- placement and surveying techniques for rock placement;
- the extension of drilling techniques for placing pipelines and cables through the surf zone;
- material and structure problems of special concern in marine construction, such as the behavior of confined concrete under overload and the response of structures to impact loads from ship collisions and ice; and
- the improvement of seafloor soils, which involves both construction techniques and an understanding of geotechnical behavior. Examples of this are cement stabilization and freezing.

APPENDIX RESPONSES TO THE ENGINEERING RESEARCH BOARD'S REQUEST FOR ASSISTANCE FROM UNIVERSITIES, PROFESSIONAL SOCIETIES, AND FEDERAL AGENCIES AND LABORATORIES

Requests for assistance were sent by the Engineering Research Board to a number of universities, recipients of Presidential Young Investigator awards, professional societies, and federal agencies and laboratories in order to obtain a broader view of engineering research opportunities, research policy needs, and the health of the research community. Some of the responses included comments on engineering research aspects of construction and structural design; these were reviewed by this panel to aid in its decision process. The panel found the responses most helpful and wishes that it were possible to individually thank all those who took the time to make their views known. Because that is not practical, we hope nevertheless that this small acknowledgment might convey our gratitude.

Responses on aspects of construction and structural design were received from over 100 individuals representing 37 different organizations (Table A-1). Whereas most of the responses addressed priority research needs, several respondents did reflect on policy issues. Although many of the research needs and issues of policy and health addressed by the respondents were similar to those noted by panel members, the broadened perspective provided by the responses to the survey was most beneficial in the panel's deliberations.

Table A-1 Organizations Responding to Information Requests Relevant to Construction and Structural Design Systems Research

UNIVERSITIES
Clarkson University
Cornell University
Duke University
Illinois Institute of Technology
Lehigh University
North Carolina State University
Northwestern University
Old Dominion University
Princeton University
Purdue University
Texas A&M University
University of California, Los Angeles
University of Hawaii
University of Houston
University of Illinois—Urbana/Champaign
University of Kansas
University of Michigan
University of Minnesota
University of Oklahoma
University of Pennsylvania
University of Texas at Austin
PROFESSIONAL ORGANIZATIONS
American Institute of Aeronautics and Astronautics
American Institute of Chemical Engineers
American Society of Civil Engineers
American Society of Mechanical Engineers
Institute of Industrial Engineers
Industrial Research Institute
Society of Engineering Science, Inc.
AGENCIES AND LABORATORIES
Air Force Institute of Technology
Air Force Office of Scientific Research
Army Materials and Mechanical Research Center
Brookhaven National Laboratory
NASA Goddard Space Flight Center
NASA Jet Propulsion Laboratory
NASA Langley Research Center
Office of Naval Research
Oak Ridge National Laboratory

Energy, Mineral, and Environmental Systems Research in the United States: An Overview

EXECUTIVE SUMMARY

Energy, mineral, and environmental resources are critical to the domestic economy, to national security, and to both human welfare and the quality of life in the United States. These resources are fundamental to other technologies as both input (energy and minerals) and output (environmental effects). As such, they form the base on which virtually all other economic activities are built. Environmental quality is determined to a large extent by the way in which energy and mineral resources are recovered and used, and as a result, environmental considerations often play a major role in the development of energy and mineral resources. It is thus essential that a sufficient level of fundamental engineering research be maintained in these three resource areas so that the United States will be in a stronger position to cope with crises and needs as they arise.

Fundamental engineering research on energy, mineral, and environmental systems is conducted to varying extents by universities, federal and national laboratories,* and industry. This type of research has been and should continue to be funded largely by the federal government, with supplemental support from the private sector. The resources at issue here are of such broad significance that no single industry or economic sector would be the major beneficiary of technological advancements in these fields. This is particularly true of the environment, which is in essence a public good. Thus the federal government has the chief responsibility for long-range research in these areas, carried out chiefly by universities and the federal and national laboratories.

Federal support for engineering research in energy, mineral, and environmental systems (both fundamental and applied) has tended to be erratic and highly responsive to immediate concerns, public opinion, and changing national priorities. Indeed, instability of funding has characterized this area more than any other examined by the Engineering Research Board. Federal funding for engineering research in the fields covered by this report is, in most areas, substantially lower than it was just a few years ago. When such reductions in funding occur, fundamental research, which already represents only a small percentage of expenditures, is often cut back to very low levels. However, this is exactly the type of research that should be maintained at a high level at all times to build the knowledge base that will be needed in the future when problems once again become critical. Such periods will surely come. Thus, national interests in these fields will be best served, instead, by a national commitment to a long-term, stable research environment.

Because of the critical importance that energy, mineral, and environmental resources have for meeting national goals, and in order to counter the instability of funding, the panel recommends with a sense of urgency that federal funding for engineering research in these fields be increased at least to the levels (in equivalent constant dollars) of 5 years ago. As a first priority, such increases should go to universities in order to preserve their dual role in long-term fundamental engineering research and in educating tomorrow's research talent.

A commitment needs to be made within mission agencies, as well as within the National Science Foundation (NSF), to stable funding of university engineering research in the energy, mineral,

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* A federal laboratory is an in-house laboratory of the federal government; a national laboratory, although essentially supported by federal funds, is independently administered by an industrial firm, a university, or another nonprofit organization.

and environmental fields so that excellence in research and education can be maintained and the knowledge base in these fields can be expanded. The nation's interests would be well served if mission agencies allocated, on a multiyear basis, a fixed percentage of their budget to this research. "Quick-response" initiatives should be undertaken as add-ons to this funding base. Increasing the NSF's budget for basic and exploratory research in these areas is another step that would greatly improve their long-term outlook and stability.

Environmental issues will continue to play a major role in industrial development. Significant environmental problems may be associated with the high-technology fields as well as with the chemical, mining, transportation, and energy industries. The most important engineering research need in this area is for long-term research on the movement, fate, effects, and control of contaminants in the air, water, and soil. Such research is needed in order to optimize new and existing industrial processes and to improve the technological basis for proper environmental regulation. As a subset, this requires fundamental research on physical and chemical processes (especially combustion), on biotechnology, on sensors and measurement techniques, and on the environment's capacity to assimilate the broad range of chemicals and other materials that are hazardous to humans and ecosystems.

Critical areas for *energy* research are those that could enable the United States to become reasonably self-sufficient in energy, so as to insulate it from the disastrous consequences of a loss of imported energy for any reason. A comprehensive program of continued engineering research—fundamental as well as applied—must be maintained to better develop and utilize all indigenous energy sources, including coal, oil, shale, nuclear and solar power, and natural gas. To ensure a broad range of future options, the primary areas recommended for engineering research in this field are the development of alternative fuels and technology, as well as continued efforts to improve the efficiency of energy conversion devices. Examples within these areas with high potential include the direct combustion of coal for power generation and process heat; the liquefaction, gasification, and beneficiation of coal; the use of coal and oil shale for transportation fuels; improved photovoltaic devices; improved energy storage techniques; and improved energy efficiency in industrial processes, buildings, and transportation systems.

At the next level of priority, engineering research should continue to ensure that the environmental consequences of energy utilization are adequately addressed; risk assessments and control technology development should be integral parts of energy-related R&D. This should especially include research into the accident potential of and damage mitigation at nuclear power plants. The development of integrated environmental control systems addressing liquid, solid, and gaseous effluents from coal utilization technology is also essential.

The most critical area of engineering research for *mineral resources* is on processes for the economic recovery of minerals from low-grade ores. As mineral resources are expended, increasingly lower grade ores will need to be used. Engineering research to meet this future requirement is inadequate and should be expanded, especially in areas such as (1) the development of sensors, instrumentation, and equipment for exploration, remote mining, and mineral processing control; (2) new separation technologies for improved mineral recovery; (3) colloidal, biological, and electrochemical processes for mineral concentration; and (4) interfacial behavior of mineral fines in processing streams.

INTRODUCTION

This report of the Panel on Energy, Mineral, and Environmental Systems Research is one of seven prepared in support of a major study conducted by the Engineering Research Board. The report addresses issues "in connection with those areas of research critical to the future development, utilization and protection of energy sources and air, water, and mineral resources of the United States." Thus, it examines engineering research needs in a wide range of major resources.*

Given its enormous breadth of coverage, and given the limited time and budget available for its preparation, this report is not comprehensive, nor was it intended to be. Its primary purpose is to provide an overview of engineering research needs in these three

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* Certain important resources—most notably agricultural and forest resources—were not included in the study and are not considered in this report.

broad fields and, secondarily, to suggest ways of strengthening the nation's engineering research effort in them. For the most part, the report addresses fundamental engineering research that provides the basis for solving many of the long-range problems that industry and society face in connection with energy, minerals, and the environment. Such research is conducted by universities, federal and national laboratories, and industry, as well as by certain other nonprofit, nonacademic research institutions.

Background

The three areas of research examined here are closely interconnected. Together, they bear directly on matters of critical national importance. Our national security depends on the continued availability of energy and mineral resources. Our domestic economy as well as our performance in the world economy also are both strongly dependent on energy and on materials derived from minerals. The quality of life in the United States, which derives in large part from the strength of our economy, also is greatly affected by the quality of the environment, in particular the vital air and water resources. In turn, the quality of these environmental resources increasingly depends on how we use the nation's energy and mineral resources.

In a very real sense, energy, mineral, and environmental resources form the base on which virtually all economic activities are built. They are (in the case of energy and minerals) the raw *input* and (in the case of environmental impacts) the ultimate *output* of human economic activity. It is for this reason that political and social attention and pressure focus so intensely on matters connected with them. Changing economic circumstances, changing national priorities, and changing social attitudes all combine to alter the directions of research in these fields.

Changes in relative prices and increased worldwide availability of crude oil have, for the time being, reduced concern about petroleum supplies; these changes have not removed the long-term vulnerability of the United States to a cutoff of imported oil. Both a diversity and balance of energy sources are needed to ensure a dependable supply of energy in the future. Complete energy independence may not be attainable; greater self-sufficiency would reduce the nation's vulnerability to unpredictable external events, however. The goal of reasonable energy self-sufficiency for this country requires a substantial R&D effort toward developing innovative means of energy production, distribution, and end use, along with the definition and acceptable control of any associated environmental problems. In addition, in the case of nuclear power, major changes in public perception are required before this energy source can contribute more substantially to the nation's energy needs.

As is true of petroleum, support for research on the extraction of minerals is also influenced by market forces and by changing degrees of access to mineral resources in international markets. Given the varied quantity and quality of domestic supplies, the United States currently imports certain strategic minerals, along with many others of broad commercial importance. Both technological and economic factors drive us to rely on external sources. Because our access to the full range of needed mineral resources depends on our ability to maintain often-tenuous international arrangements, it is prudent for the United States to act in ways that ensure the strongest possible knowledge base from which future energy and mineral resource exploitation technologies can be developed. Here again, improved technology can reduce the nation's vulnerability to external forces.

New research needs are constantly appearing in the energy, mineral, and environmental fields because they are so closely interlinked with every other area of scientific, technological, and economic development. The opportunity to achieve success in promising new technological areas can be severely compromised if we are unable to deal effectively with the energy/mineral/environmental resource infrastructure in which these technical advances must function. It is therefore essential to the continued health of technology development in the United States that we continue to increase our fundamental understanding of these matters, so that we will be able to cope successfully with the challenges and problems that new technologies in any field tend to create.

Scope

In this report we examine a number of key issues affecting the health and effectiveness of engineering research in energy, minerals, and the environment.* At the heart of the report is the

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* Engineering research is research conducted to expand our useful knowledge about the man-made and natural worlds in order to discover engineering principles by which significant improvements can be obtained in the processes of engineering design and production. (For further definition, see the chapter on "Engineering Research in the United States: An Overview").

identification of important areas of research now needing attention if our capabilities and knowledge are to progress in a balanced fashion commensurate with emerging needs. The panel's scope of coverage encompassed engineering research:

- to provide an information base and methods for assessing tradeoffs among resource utilization, environmental protection, and economic development;
- on alternatives to petroleum as an energy source, including nuclear fission and fusion, other fossil fuels, solar power, and other renewable energy resources;
- on new or improved technologies for petroleum recovery, including economical assisted-recovery techniques;
- on new or improved technologies for the production, distribution, and storage of electric power;
- on new or improved technologies for more efficient end use of energy in its various forms;
- on the exploration, mining, and processing of mineral resources;
- on new or improved technologies for the utilization and protection of air and water resources; and
- on the reduction, control, and management of hazardous materials.

This scope includes a mixture of fundamental and applied engineering research areas, but emphasizes long-term, fundamental work.

POLICY ISSUES

Basis for Federal Policies on the Support of Research

Over the past three decades the nation's commitment to the support of basic and applied research has provided extraordinary benefits to society and, in the process, has established the United

States as a world leader in science and technology. The federal government's involvement in research derives from its responsibility for national security and from its obligation to provide for the general health and welfare of its citizens. The scale of research in emerging technological fields is often too large for private companies to undertake; in general there is also too little incentive for industry to support extensive long-range engineering research. In addition, *areas of general public health and welfare such as environmental quality are not normally targets of industry research. Yet the public interest demands that this type of research be pursued*. Therefore, regardless of where it is performed —at universities, in federal or national laboratories, in industry, or elsewhere— the majority of research in these fields is funded by the federal government. This federal support is especially prominent in the case of fundamental research.

A basic premise on which the federal government must plan for research in the energy field in particular is that the demand for energy in the United States will continue to grow over the long term, notwithstanding very significant efforts in the direction of conservation and efficient energy use. Recent studies have shown that whereas total U.S. energy consumption is no longer directly coupled to the gross national product (GNP), there is still a demonstrable direct correlation between the consumption of electrical energy and the GNP (Whittaker, 1984). At the same time, the mix of basic energy resources available to support economic activity and growth may change. Clearly, it is in the national interest that aggressive engineering research continue on ways to utilize energy more efficiently in a variety of forms, and in a wide range of industrial, commercial, residential, and transportation applications. Improvements in this area could partly compensate for the growth in energy consumption brought about by an expanding population that aspires to a higher, more energy-intensive standard of living. On a worldwide basis, the growth in the consumption of energy in less developed nations, with their exploding populations and rising expectations, is likely to be even more dramatic.

Perhaps the most important implication of this energy-demand growth is that a range of energy options must be maintained, so that disruptions in the availability or economics of any particular fuel do not leave the United States in a vulnerable position. Globally as well as nationally, the impact of long-term energy growth *on the environment will also be significant, and must be carefully addressed*. Further steps must be taken in assessing the adverse environmental consequences of energy production, as the nature of some "side effects" is still unknown. For example, "acid rain" and the "greenhouse effect" currently are potentially major environmental concerns associated with fossil fuel utilization; the resolution of these issues is particularly important for determining the relative role that coal will play in the future. Likewise, disposal of radioactive wastes remains a major problem with nuclear energy. All three of these concerns are characterized by serious uncertainties about the severity of the problems, and by the fact that there are political as well as technical dimensions to their solutions. Although these particular problems differ significantly in terms of their geographical spread and time scales of concern, they all could potentially alter future costs and patterns of energy utilization in this country and elsewhere. Prudence thus demands that vigorous R&D continue on alternative energy sources (including nuclear fusion and solar power), to ensure as wide a range of future options as possible. Industry must continue to be a strong partner with government in the support and performance of this area of R&D.

The same reasoning applies in the case of mineral resources, for which new technology could give the United States a wider range of sources and options. At present, adequate technology for efficient and safe mining and processing of low-grade, finely dispersed domestic ores does not exist. There is very little engineering research being done either to develop such technology or to build the fundamental knowledge base required for its development.

The range and potential size of these problems, and the difficulty of solving them, suggests that viewing energy, minerals, and the environment as separate and distinct concerns is no longer a workable approach. *Now more than ever before there is a need for the federal government to identify tradeoffs and achieve balance between energy and resource utilization, on the one hand, and environmental protection on the other*. Developing the needed technical information and tools for assessing such tradeoffs will require input from the various engineering disciplines as well as a number of scientific fields.

Unfortunately, government policies in a specific area (e.g., energy, environment, etc.), once officially promulgated, often are interpreted in an absolute sense that provides little room for tradeoffs among the environment, energy supply, jobs, economic development, national defense, quality of life, and so on. The federal government's support of research should be structured so as to provide an informed basis for making those tradeoffs and balancing the many competing interests and requirements that converge on these vital areas.

Need for Long-Term Continuity in Support of Research

In energy, minerals, and the environment, the federal government's support of engineering research sometimes seems chaotic—indeed, this is more often the case in these fields than in any other. New issues, sudden crises, and changing expectations frequently alter the research priorities of federal agencies and bring about erratic changes in emphasis. These shifts occur at the expense of the knowledge base needed to address future problems. The mission agencies are most subject of all to the shifting political winds. Although it may be politically attractive to "force feed" selected areas of research in hopes of achieving quick fixes, *national interests in energy, mineral, and environmental resources will be best served by a national commitment to a long-term, stable research environment*. The crises that frequently stimulate engineering research in these fields may be so compelling that, for political reasons, they cannot be ignored. However, such "quick-response" initiatives should be undertaken as add-ons to continuing and stable support of both fundamental and applied engineering research.

There are undoubtedly many ways to ensure a more stable commitment to research. One possible means would be to increase the NSF's research budget in these areas. *Long-term research needs in these areas might be better served if the NSF were to take a greater role in fundamental and exploratory engineering research programs that the mission agencies do not see as part of their objectives*. The mission agencies, for their part, need to recognize that the training of researchers over the long term is not exclusively the NSF's province; each agency also has a responsibility here. Therefore, *another very useful step would be for mission agencies to allocate, on a multiyear basis, a fixed percentage of their budget to university engineering research in appropriate fields*. This approach would improve stability in both education and research, and would help attract the best available talent to these fields.

Rethinking Roles

The role of the government in supporting large-scale research facilities also needs to be carefully considered. In many areas, federal participation in scale-up projects, along with significant private support where obtainable, can substantially accelerate the development of new technology. Also of special urgency is the need to address the relative research roles of the universities and the various federal and national laboratories working in the energy, mineral, and environmental fields. Universities not only provide diverse fundamental and applied research ideas and results, but are also essential to the education of the research engineers and scientists needed to maintain a strong national research establishment and to enhance industrial innovation. For this reason, the tendency to protect the federal and national laboratories at the expense of university research during budget reductions (see, for example, Office of Science and Technology Policy, 1983) must be resisted. The importance of this issue has been confirmed in many recent reports, including a series of studies by the Energy Research Advisory Board of the U.S. Department of Energy (1985).

Consideration might also be given to emulating more widely the Japanese process of "bottom-up" (or participative) planning of R&D, at least to the extent of gathering information and shifting through the various ideas, rather than allowing major policy decisions on R&D activities to be dictated by the most recent perturbation in the budgeting process. Although this kind of activity is already going on to some extent, it should be more widely and systematically adopted for energy, mineral, and environmental R&D planning.

ISSUES DETERMINING THE HEALTH OF ENERGY, MINERAL, AND ENVIRONMENTAL SYSTEMS RESEARCH

The health of engineering research in energy, mineral, and environmental systems can be assessed by identifying the objectives of that research and asking how well they are being achieved. In energy research, primary objectives are focused on improving the scientific and technical understanding of energy conversion processes in order to: develop a wider range of alternative energy resources, increase energy efficiency, lower costs, and reduce environmental impacts and other hazards of energy use. Research objectives are similar with respect to mineral and environmental research: that is, greater cost-effectiveness and safety in extraction and processing, better and less expensive means for controlling pollutants and reducing risks to the health of the environment (including human health), and a broader range of options. Secondary objectives in all three cases include the training of scientists and engineers to ensure a future resource base of skilled personnel.

Given such goals, the general question to ask is, how well are we doing? Is there a healthy research environment leading to recognizable improvements in these areas? Is new knowledge being generated and is it resulting in new and better processes for the utilization of energy and for the extraction and processing of domestic mineral resources? Is the supply of new talent entering the field adequate to ensure its future vitality? With regard to environmental research, a key question is whether the current research climate is conducive to identifying and resolving the critical environmental problems of our times—particularly man-made problems over which we have the greatest control. In addition, are we anticipating potential future problems and taking steps to address them? Is the level of research consistent with identified needs, and have recent historical trends been in the right direction?

Clearly, it is much easier to pose these questions than to answer them definitively. A very wide range of activities are encompassed by the areas under consideration here, and there is invariably some degree of subjectivity in selecting specific measures for addressing them. In general, however, it is clear that engineering research over the past several decades has been vital to the development of energy technology, mineral resource utilization, and improved environmental quality, and that the maintenance of a healthy and aggressive research environment continues to be a high priority for the continued well-being of the nation. *Given this importance, our principal concern is that a growing sense of complacency appears to be entering the national mood with respect to the importance of energy, mineral, and environmental research.*

Such a trend will, if sustained, have adverse consequences for our national economic development and well-being.

The research areas of concern here are distinct from many others in that they deal to a large extent with matters of the public welfare. Thus, they demand a particularly strong governmental role in many facets of R&D. Although industrial and other private-sector support is important, the extent of government involvement must be considered a critical determinant of research "health." Federal agencies support research in these areas by providing funds to a variety of performers, including universities, federal and national laboratories, and private companies. Intramural research centers such as those operated by the National Aeronautics and Space Administration (NASA) and the Departments of Defense (DOD) and Energy (DOE) perform a significant amount of research in these fields.

In addition, a substantial portion of the federally supported research in these areas is carried out by the national laboratories. These include, for example, the Argonne National Laboratory, the Brookhaven National Laboratory, the E. O. Lawrence Berkeley Laboratory, the Lincoln Laboratory, the Los Alamos National Laboratory, the Jet Propulsion Laboratory, the Oak Ridge National Laboratory, the Sandia National Laboratories, and the Solar Energy Research Institute.[*] They are all involved in research on questions of energy generation, storage, and/or use—and, as in the case of the intramural research centers active in these areas, receive their principal support from the DOD, DOE, and NASA. Improvement of existing fossil fuels and nuclear power systems is a common focus, along with the effort to develop alternative energy sources. Most of the laboratories also pursue some research on the mitigation of the environmental impacts due to energy generation through use of environmental control technology and on the technologies that permit the development and conservation of water and energy resources. These programs of engineering research are for the most part reasonably well funded, and in content run the gamut from the very fundamental to the directly applied.

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[*] The panel received input from several of the national laboratories in response to the Engineering Research Board's request for information and suggestions (see Appendix). These responses were valuable in identifying pressing research needs.

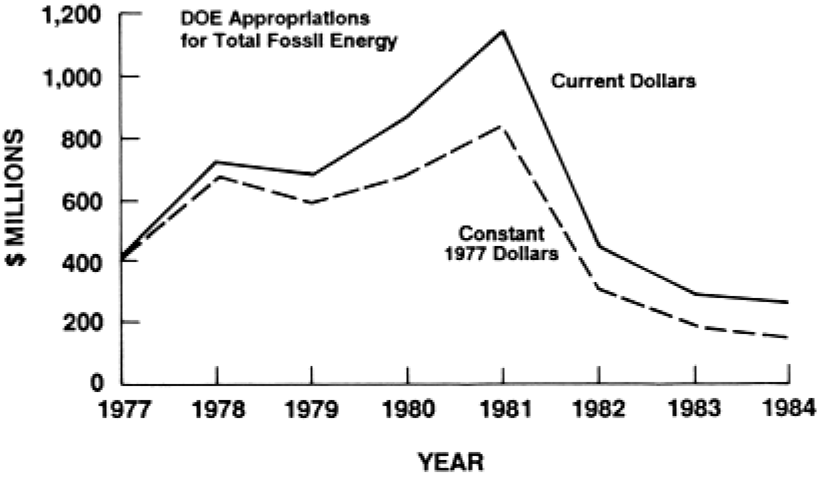


Figure 1
Federal fossil energy appropriations peaked in the early 1980s and have since been sharply reduced. (SOURCE: National Research Council, 1984.)

Current and comprehensive data regarding overall national support of research in energy, minerals, and the environment, both federal and private, are not readily available. Thus, it is difficult to generalize about the spread of research efforts across specific areas of research. However, federal agency funding statistics do offer a rough measure of relevant trends.

Funding Trends

Energy

In the energy area, federal policy over the past 5 years has sought to shift applied research and demonstration projects to the private sector, focusing government activities solely on what are defined as "long-term, high-risk, high potential payoff" projects. Figure 1 illustrates one example of the impact of this policy on funding. In general, nondefense R&D expenditures by the DOE have decreased significantly since 1980, with R&D on fossil fuels, nuclear fission, energy conservation, and solar power being curtailed most sharply [National Research Council, 1984; Science, 1985]. Still-deeper cuts are slated for FY86, as seen in Table 1. Other fundamental engineering research in energy is sponsored by DOD and NSF; however, it is difficult to extract the needed energy-related information here as it is intertwined with other research areas.

Table 1 Department of Energy R&D Trends

	Budget ($millions, current year)			Percent
Category	1984 (actual)	1985 (est.)	1986 (est.)	change, 1985–1986
Defense R&D (weapons)	1,380.1	1,810.1	1,868.2	3
Energy supply R&D	2,059.9	1,833.2	1,669.6	-9
Supporting research and technical analysis	342.4	446.9	426.6	-4
Magnetic fusion	469.1	434.0	390.0	-10
Nuclear fission	697.7	416.3	371.8	-11
Environment	229.4	230.9	228.3	-1
Solar power and other renewables	218.8	211.8	175.6	-17
Other	102.5	93.3	77.3	-17
General science and research	634.6	728.3	685.4	-6
Fossil R&D	342.4	347.1	242.7	-30
Conservation R&D	176.6	176.0	145.1	-18
Total	4,593.6	4,894.7	4,611.0	-6

SOURCE: Chemical and Engineering. News, February 18, 1985, p. 14.

Environment

In the environmental field, recent federal priorities for research have tended to focus on high-visibility problem areas such as toxic substances, hazardous waste disposal, and acid rain; other programs have been in decline. The Environmental Protection Agency (EPA), together with the National Oceanic and Atmospheric Administration (NOAA) and DOE, are the major federal agencies involved in environmental R&D. There have been substantial reductions in EPA's research budget since 1979, in both current and constant dollars, as illustrated in Figure 2. Whereas a small upward trend over the past 2 years has at least accounted for inflation, the dollar amount is still below that of 1976 (when viewed in constant dollars), and less than half that of 1979. In addition, funding for EPA's Exploratory Research Grant Program, initiated in 1980 to promote long-term fundamental research in universities, has been cut by more than 60 percent—in actual current-year dollars—from the peak year in 1981 (Table 2); the drop in funding has been even more severe (about 70 percent) in constant dollars. A strong resurgence in 1985 seems to have been only an anomaly in the downward trend.

As indicated in Table 1, environmental research within DOE was funded at a lower level in FY86 than in FY84 (although environmental research did not fare as badly as other components of DOE's research). Detailed data on environmental R&D programs in NOAA are not available, although some indication of the trend may be derived from that fact that NOAA expects to receive a 33 percent budget cut in 1986 R&D funding, including marine and atmospheric research programs (Science, 1985).

The NSF is also a source of federal research funds for environmental engineering. Under the new organization of the Engineering Directorate at the NSF, funding for environmental engineering is provided within the Division for Fundamental Research

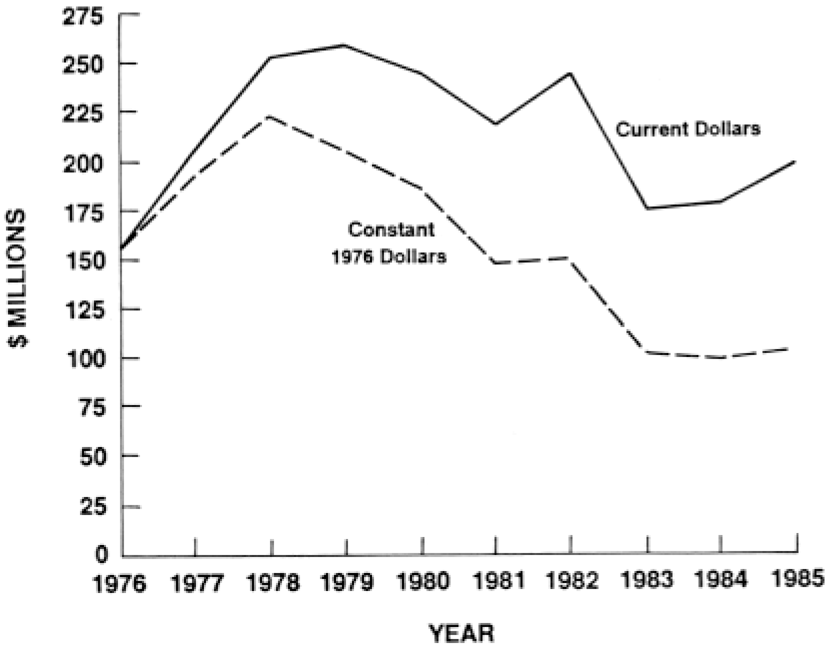


Figure 2
Environmental Protection Agency obligations for research, FY76–FY85.
(SOURCE: National Science Foundation, 1985.)

Table 2 EPA Office of Exploratory Research Grant Program ($millions)

				Environmental		Total		Annual Percent Change	
		Pollution		Chemistry and Physics		Current Year	Constant 1980	Current Year	Constant 1980
	Environmental	Control	Environmental						
Year	Health	Processes	Biology	Air	Water	Dollars	Dollars	Dollars	Dollars
1980	7.28	2.98	3.42	1.83	1.25	16.76			
1981	10.43	4.16	2.88	2.64	1.44	21.55	19.60	29	17
1982	4.49	3.81	1.85	0.83	1.87	12.85	10.90	-40	44
1983	2.57	2.92	1.19	1.88	0.86	9.42	7.69	-27	-29
1984	2.95	1.30	2.09	1.05	1.13	8.52	6.69	-10	-13
1985	3.80	2.00	3.00	1.50	1.50	11.80	8.85	38	24
1986						8.4 (est.)		-29	(n.a.)

NOTE: n.a. = not available.

SOURCE: Environmental Protection Agency (1985).

in Emerging and Critical Engineering Systems. Funding from this source is proposed to be unchanged from FY85 to FY86, at approximately $5.4 million.

Table 3 Federal Obligations for Research in Metallurgy and Materials ($millions)

	Category of Research			Annual	
Year	Basic	Applied	Total	Percent Change[a]	Total[b]
1982	155.9	153.2	309.1		
1983	182.9	149.6	332.5	7.5	3.5
1984	194.9	151.5	346.4	4.2	0.0
1985	207.0	162.2	362.9	4.8	0.0

[a] Current-year dollars.

[b] Constant 1982 dollars.

SOURCE: National Science Foundation (1985).

Mineral Resources

The situation for funding in mineral resources research is mixed. Overall, there has been a gradual increase in federal funding for metallurgy and materials research, sufficient only to compensate for inflation (Table 3). These figures reflect primarily research on the characterization, properties, and behavior of materials, rather than on the extraction and processing of mineral resources. At NSF, with the reorganization of the Engineering Directorate, minerals engineering research has been split into three programs under two of the new divisions. The focus has shifted from mineral extraction and processing per se, to the integration of minerals recovery with the overall cycle of materials processing and product fabrication. With this change, the panel is concerned that needed fundamental engineering research on mineral recovery will not be receiving explicit enough attention in the context of this "big picture" treatment.

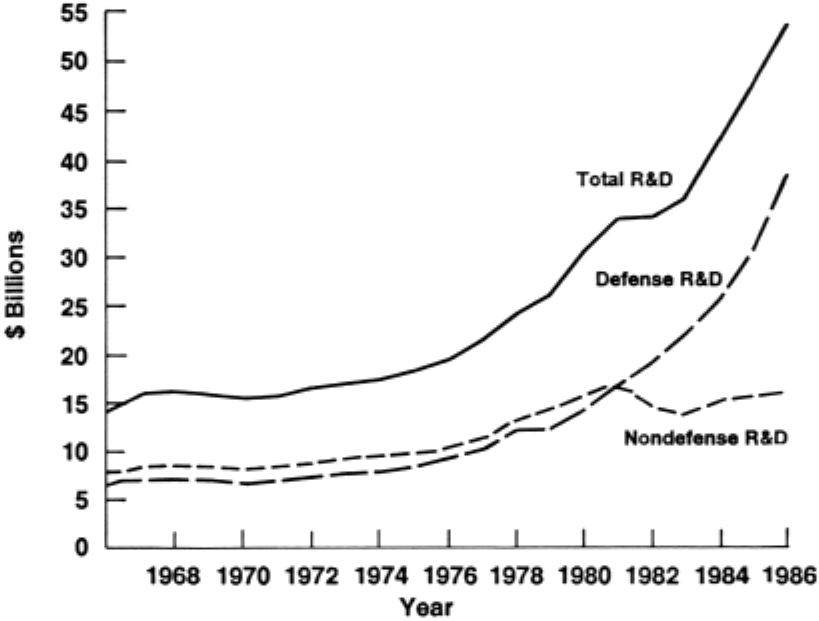
It will take a great deal of coordination and planning on the part of the NSF to ensure that the "components" (e.g., minerals) are not neglected. This is particularly important in light of the fact that the Bureau of Mines' support of engineering research on mineral resources has declined sharply, from $71 million in FY82 to $53.7 million in FY84.

Overview

As Figure 3 illustrates, overall nondefense R&D support by the federal government has remained static in current dollars (declining in constant dollars) since 1980 (National Science Foundation, 1982). Federally funded engineering research overall has fared about the same as total research, as indicated in Figure 4. Engineering research in general is on the rise in NSF, as evidenced by the NSF's budget proposal to increase engineering support from $150 million in FY85 to $170 million in FY86. However, as we have seen, *research in most areas of energy, mineral, and environmental systems is experiencing constant or decreasing dollar support from year to year*. It can be expected that only a small part of the decrement in federal sponsorship of energy, mineral, and environmental research will be made up by private industry and foundation support. Privately supported engineering research projects tend to be highly focused and applications-oriented, rather than basic or exploratory in nature.

In this climate of greatly reduced real funding, there is increased competition for research funds, particularly between universities and national laboratories. Survey responses from individuals and organizations throughout the country have pointed to the shortage of funds as a particular problem affecting the universities' mission to teach and train new students, as well as to carry out needed research. (See the Appendix.)

Thus, as measured by recent trends in public spending, the outlook for the health of research on energy, minerals, and the environment is not promising. Given its already limited support, fundamental research is often cut to very low absolute levels when funding is reduced. Yet this is precisely the type of research that is necessary to build the knowledge base needed to address future problems in energy, minerals, and the environment. Although the pressure is off these areas now, they are sure to become critical again in the future. The time to devote resources to fundamental research is now, when the needs are not so immediately pressing. When the needs become critical, it is too late for research.



Source: Office of Management and Budget.

Figure 3
Federal support of non-defense R&D has been roughly level since 1980 (in current-year dollars). It has been slipping in constant dollars. Defense R&D accounted for 48 percent of R&D spending in 1966, 70 percent in 1986.

Human Resources

Evidence of decline is seen in statistics on the number of new people entering the field—particularly new Ph.D.s, who represent the future of fundamental and applied engineering research. Relative to the situation a decade ago, today's "glamor areas" are in computer science, electronics, and biotechnology, and it is toward these fields that younger engineering talent is strongly gravitating. Engineering in general is healthy, based on enrollments, but the energy, mineral, and environmental fields generally do not share that strong student interest. For example, whereas overall undergraduate enrollment in engineering increased by 16 percent from 1979 to 1984, civil engineering enrollment (a common training ground for entrants into environmental engineering) decreased 14 percent over the same period; environmental engineering majors dropped by 19 percent, and nuclear engineering enrollments fell

14 percent. Metallurgical and materials engineering enrollments increased 3 percent, but this was slight in comparison to overall undergraduate engineering enrollment increases (Engineering Manpower Commission, 1985).

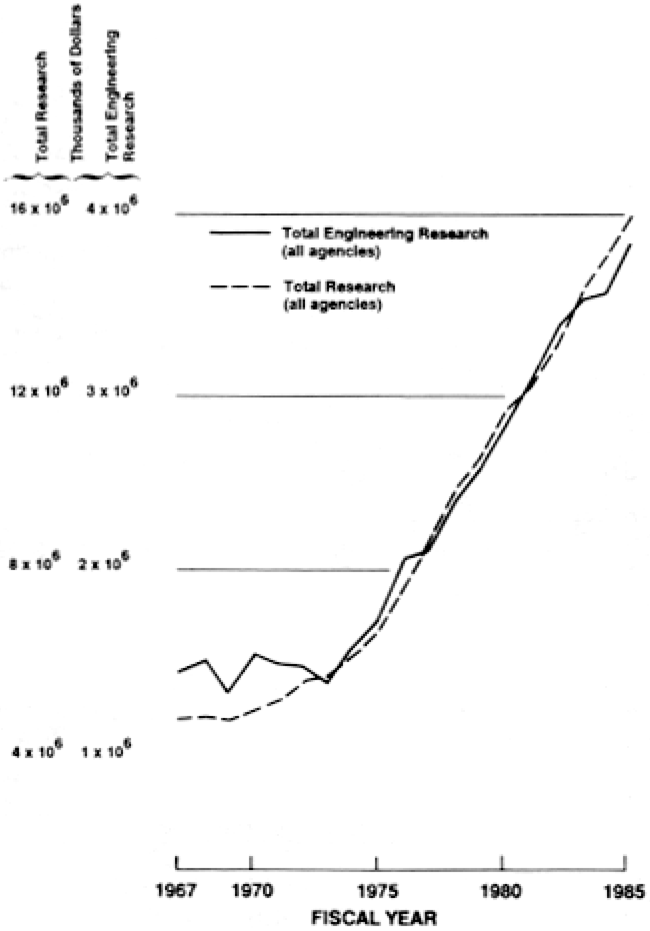


Figure 4
Federal funding for engineering research is keeping pace with total federal funding for research. (SOURCE: National Science Foundation, undated).

Similar trends are seen in graduate study for most of the subject fields. During the 1979–1984 period, overall graduate engineering enrollment increased 38 percent, but graduate enrollment in environmental engineering decreased 8 percent and in nuclear engineering by 7 percent. Given the lag in graduate enrollments compared to same-year undergraduate enrollments, the greater decrements in 1984 undergraduates by now are likely affecting graduate enrollments (although current data are not available as of this writing).

Whereas the challenges to understanding and discovering means of solving new and complex issues in these areas remain, it is clear that reductions in research funding affect the trends for student enrollments and student quality in engineering; these effects are now being seen in the energy, mineral, and environmental systems areas. Student interest and research opportunities have already dwindled sharply in some areas, such as environmental and nuclear engineering. Diversity of training in technical fields, even in areas not currently accorded great importance by the market, is necessary in order to maintain our capacity to meet rapidly emerging needs. The nation will continue to need a cadre of highly qualified researchers in the fields of energy, minerals, and the environment.

The decline in student interest underscores the importance of long-term stable research funding to counteract the negative signals given by the market. *A stable research environment is essential to attract and train the best minds and talent in a given field, and to ensure its continued well-being*. As a nation we must constantly ask whether we are making sufficient intellectual investments in key areas of energy, mineral, and environmental research to ensure the long-term integrity of these vital areas. It is in our best interest to do so.

The first priority should be research support of the faculty and graduate students in universities with engineering schools and colleges. This is the most cost-effective way to develop the needed knowledge bases, and is an important source of new ideas. These engineering faculties, given adequate support for research and for their graduate students, generate the highly educated individuals needed to protect and advance the national interest in these fields and to supply the skilled manpower needs of industry.

Finally, the panel observes that the nature of many critical research problems in energy, minerals, and the environment increasingly demands a multidisciplinary perspective and approach to solutions. A problem such as acid rain, for example, may involve specialists from different fields of engineering, chemistry, physics, economics, forestry, and so on, whose joint efforts are needed to piece together an understanding of an environmental problem of this complexity. Similar situations apply in the areas of energy and mineral resource utilization. Advances in materials science and engineering, for example, may permit progress along many fronts in all three areas. Thus, another determinant of the health and vitality of research in these areas is the extent to which funding mechanisms are in place to recognize and encourage multidisciplinary research. Although the importance of this has been generally recognized, *much more remains to be done to overcome organizational impediments to multidisciplinary research within research organizations (including the universities), as well as within (and among) government agencies responsible for identifying and funding critical research*.

RESEARCH OPPORTUNITIES

The panel was charged with identifying "important or emerging research needs" in the energy, mineral, and environmental fields. The research topics we have identified, which are presented in the following sections, represent a combination of these two criteria. All of the topics are important, for the reasons outlined, and all are emerging, in the sense of having not yet reached that plane of activity at which broad solutions to large problems are in view. Indeed, some research is currently being pursued in each of the areas selected; much more research is needed in all of them, however, before the potential they represent can be realized. Given the dramatic declines in funding described in the previous section, our first concern is to safeguard the future of important fledgling research thrusts, rather than to propose entirely new programs.

Where it seemed feasible to do so, we have suggested relative priorities of importance among the various research topics identified within each of the three main areas. There is no point in attempting to establish relative priorities across the three areas themselves, as they are quite disparate. It might be said, however, that the time frames of the national needs associated with them differ. We would characterize energy and minerals research in general as being of longer term importance, whereas environmental research has applications that are of immediate—indeed, urgent—concern. We would, however, emphasize that "longer term" does not mean that any of the research efforts described here can be postponed. The topics themselves are generally long term in nature, and now is the time to begin pursuing them in greater earnest.

Environment

The activities of a highly industrialized society inevitably result in the generation of large quantities of "residual" or waste materials, some of which can be detrimental to the health of humans and/or the environment. In order for society fully to enjoy the rewards of its accomplishments, the adverse impacts of its waste substances (liquids, solids, and gases) must be effectively addressed. These effect are proving to be one of the most difficult and costly problems facing the nation. In order to take better advantage of available technical opportunities and improve our productivity and competitiveness, adequate national resources must be directed toward solutions to current and emerging waste management problems.

The most pressing waste management issue is that involving hazardous materials. Over the past 25–30 years, our awareness of this problem and its pervasiveness has continued to grow, as has our understanding of the widespread impacts of hazardous waste materials on human health and that of the environment. However, the nation is still far from developing adequate solutions, and new cases of serious environmental degradation appear almost daily. Two issues that are currently receiving widespread national and international attention are acid rain and groundwater contamination. Both conditions derive from the introduction of waste materials into the environment, and both have major adverse impacts on critical national resources.

All manufacturing industries, including high-technology as well as the chemical, electrical power, fuel, and mineral processing industries, contribute in a significant way to the widely felt damages from uncontrolled hazardous and toxic waste materials. Therefore, *research directed toward the cost-effective alleviation of these environmental hazards will have a direct impact on the* *health of U.S. industry as well as on society and the environment in general.*

Landfills or perpetual storage are now the most common means used to manage hazardous waste; these practices are unlikely to be continued indefinitely, because constituents will very likely leak and migrate into the groundwater (National Research Council, 1983). No single alternatives technique or method is presently available that can solve all of these problems. One important practice is industrial process modification to reduce or eliminate the volume of specific hazardous wastes or to permit recycling and reuse of residual materials rather than throwing them away. *Conversion techniques are also important. These include a wide variety of physical, chemical, and biological techniques, such as thermal processes, including incineration and pyrolysis, and biological processes, which can convert harmful organic chemicals into inorganic compounds that are harmless constituents of the environment. In addition, the environment itself can assimilate a certain portion of the residuals with little significant harm either to humans or to itself*. Such ultimate methods of disposal are particularly attractive.

Important opportunities in these areas—combustion technology, biological processes, evaluation of the environment's assimilative capacity, and sensors and measurement methods for monitoring contaminants—are discussed in the following subsections.

Combustion

Combustion is an extremely important area for future research, as it has a direct impact on a wide variety of environmental problems as well as on energy conversion. Indeed, combustion offers perhaps the greatest opportunity for eliminating hazardous organic wastes, through conversion to harmless compounds such as carbon dioxide, water, and chlorides. Combustion often results in the formation of other products that escape as residuals into the environment; prominent examples of these products are oxides of nitrogen and sulfur from fuel combustion, and chlorinated dioxins formed from the combustion of solid wastes. Insufficient information is available on the complex transformations of chemicals that occur during combustion of mixtures. *A much better fundamental understanding of the overall physics and chemistry is needed to help in the development of combustion technology for controlling* *environmental pollutants*. Advances in basic catalytic chemistry and in materials science and engineering will likely hold important keys to progress in combustion technology.

Microbial Transformation

A general characteristic of most of the hazardous organic chemicals that are of greatest concern for human and environmental health is their resistance to natural processes of purification by which organic chemicals might be converted to harmless residues. Because they are not readily destroyed in the environment they can persist for years, or perhaps centuries, during which time they can be transported throughout the ecosystem to cause environmental damage on a broad scale. Examples are DDT and PCBs, which are now found in the tissue and fatty materials of most animals, even in remote locations. Others are halogenated solvents, such as 1,1,1-trichloroethane and trichloroethylene, which currently are among the most prevalent groundwater contaminants.

Recombinant DNA methods might be used to develop strains of microoganisms with a broad-scale effectiveness in transforming environmental pollutants. In addition, recent studies have demonstrated that under proper environmental conditions, some species of microoganisms that are natural inhabitants of the soil can become adapted to the biotransformation of a wide range of hazardous chemicals. Such microorganisms might be exploited on a large scale to destroy contaminants in relatively dilute aqueous environments—conditions that are not suitable for destruction through combustion. *More complete basic knowledge is needed about the microoganisms, their physiology, biochemistry, and ecology, in order to more fully develop the biotechnology for transforming dilute hazardous waste.*

Assimilative Capacity of the Global Environment

Although the natural capacity of the environment to assimilate hazardous chemicals is limited and can be easily overwhelmed, *research directed toward understanding the movement, fate, and effects of chemicals in the environment is most important in developing control strategies, identifying those areas of research that are likely to be fruitful, and assessing the ability of the environment itself to deal safely with contaminants*. Indeed, biological and chemical processes used for contaminant control often mimic natural processes; thus, what is learned about natural mechanisms can often be applied to produce the same transformations in a much more rapid, controlled, and cost-effective fashion. This area of research deserves a high level of research support.

Sensors and Measurement Methods

Another research area that could lead to immediate and significant progress in the control of hazardous waste materials is that of sensors for detecting and monitoring chemicals in the environment. Appropriate methods of measurement must be developed as well. Control technology is severely limited by the inability to monitor continuously the efficiency and reliability of a process for removing contaminants. In addition, knowledge of the presence, movement, and fate of contaminants in ground-and surface water, soils, and the atmosphere is greatly hampered by the lack of instruments to measure rapidly the broad range of contaminants of concern. Current and evolving technology is providing us with the potential to develop sensors and analytical methods that could be used in a great variety of applications, from detection and monitoring to the control of environmental contaminants. *The ability to gather better and more comprehensive information using sensors will require development of analytical modeling techniques that can both integrate this information and identify viable control strategies*. Research in these areas could lead to major payoffs in environmental protection.

Energy

Energy R&D supported by the federal government should be directed toward achieving national energy goals and/or supporting such national energy policies as are implicit in legislation, regulation, and national debate. As a minimum, the fundamental objectives of energy research should be to ensure for the nation the following: an adequate and reliable supply of energy to support economic development, a diversity of energy sources to minimize undue dependence on any single source and reduce vulnerability to political embargo, and efficient and economic energy conversion systems that limit environmental impacts to acceptable levels.

Any review of energy research opportunities can touch only a few key areas. The following are viewed as especially important areas for federal support of engineering research.

Alternative Fossil Fuel Sources and Technology

Coal is abundant in the continental United States and is a possible source of fuels and lubricants for transportation (applications) in addition to direct use in furnaces and boilers. *Liquefaction of coal aimed at transportation fuels; coal gasification to produce pipeline-quality methane or methanol as a possible engine fuel, or other chemicals; and coal beneficiation, to remove impurities prior to combustion all are important areas of research on coal utilization.*

Another alternative source of transportation fuels is oil shale, which is present in the western United States in large amounts. *Extraction and processing of oil shale is a promising research area, and one that would benefit from continued research on mining, waste handling, grinding, and chemical processing*.

Finally, *an aggressive and sustained program of basic and applied research related to the direct utilization of coal for process heat and electric power production is clearly needed*. The demonstration of advanced concepts (e.g., pressurized fluid bed combustion) also should be pursued in conjunction with previously sponsored research efforts to ensure that promising options are effectively commercialized.

Solar Energy

Research on solar energy should be pursued vigorously because it is the only potentially large source of nonfossil fuel energy other than nuclear energy. Research on photovoltaic devices aimed at higher efficiences over their useful life and lower manufacturing costs could result in their more extensive use as a replacement for fossil fuels.

Improved Petroleum Production Systems

With the advent in recent years of higher oil prices, more expensive and efficient techniques for increasing recovery of petroleum from known reservoirs are being used. Water flooding; stream flooding of shallow fields; and injection of carbon dioxide, nitrogen, or gaseous combustion products are some of the techniques now in commercial use. *Research on improving these techniques or on finding viable chemical treatments could result in recovery of more oil-in-place, and at lower cost.* Related to this, there also is important research to be undertaken on understanding and mitigating the environmental impacts of oil production systems, including, for example, the assessment of groundwater contamination and the fate of injected water or chemicals.

Improving the Nuclear Option

Nuclear fission currently produces about 15 percent of the electricity generated in the United States; however, no other plants beyond those under construction are expected for the remainder of the century. *Research efforts that could improve the nuclear option include (1) continued efforts to improve our capacity to evaluate accurately the magnitude of potential accidents against which a plant must be designed, (2) development of plant designs that inherently mitigate against the adverse consequences of potential accidents, (3) development of decommissioning methods for nuclear plants, and (4) resolution of the nuclear waste disposal issue*. Nuclear fusion may also become an option in the future, once the current technical barriers to it are explored and overcome.

In the case of nuclear waste, partitioning of the actinides (long-lived heavy isotopes produced by neutron absorption) from the fission products, with subsequent burn-up of the actinides in nuclear power plants or fusion devices, could change the nature of the nuclear waste disposal problem by significantly decreasing the decay time of the residual waste and the nature of the containment process (Croff et al., 1980).

Integrated Environmental Control Systems

Environmental control technology for fossil fuel power plants generally has been treated as an "add-on" to basic energy conversion systems, rather than as an integral part of the process design. As these systems have grown more numerous and complex, the need to consider air pollution, water pollution, and solid waste control systems as key components of energy conversion technology has become increasingly apparent. Development of advanced fossil energy technologies such as two-stage combustion, slagging burners, sorbent injection systems, and fluidized bed boilers (both atmospheric and pressurized), which incorporate environmental controls into the basic design of the plant represent important areas of research, as does the development and integration of other precombustion, combustion, and postcombustion control technology. Research should focus on: (1) basic process mechanisms (e.g., combustion, multiphase transport, etc.); (2) the engineering of process components; and (3) the optimization of overall system designs, to improve overall plant efficiency and reliability and reduce adverse multimedia (i.e., air, water, land) environmental impacts.

Efficient Use of Energy

Energy conservation and the efficient use of energy have received increased attention as energy costs have risen. Energy-intensive industries such as refineries, chemical plants, mines, and smelters have had a compelling incentive to increase the efficiency of their energy usage. Many of the improvements have come from operating changes, some from capital expenditures to install more efficient equipment, and some from the introduction of advanced digital computer-based process control systems. Commercial buildings and houses also have reduced energy usage, primarily for heating and cooling. *Sustained research supporting the development of more energy-efficient utilization systems and devices for electricity* (motors, control systems, heating and cooling systems, etc.), *fossil fuels* (engines, boilers, chemical processes, etc.), *and solar energy is essential*. Successful efforts in these areas will delay the need for constructing additional energy generating and production facilities, and may be more cost effective than constructing new facilities to increase the energy supply.

Fuel Quality

Economic conversion of low-grade or low-quality fuels (e.g., tar sands, refuse, and fossil fuels with high sulphur or metallic content) into electricity or other energy forms is very important for effective utilization of domestic reserves. Thermal energy conversion equipment (boilers, burners, gasifiers, etc.) capable of efficiently utilizing low-grade fuels also must be developed. These are essential components of any program to reduce dependence on foreign energy resources. An improved understanding of process mechanisms is particularly essential to progress in this area.

Extending Plant Lifetimes

An important emerging area of energy-related engineering research is the issue of extending the lifetime and upgrading the performance and reliability of existing energy conversion and power generation facilities. In the face of increasingly higher costs for new facilities, there are strong incentives to seek ways of modifying, repowering, or upgrading existing plants with the goal of extending their useful lifetimes by 20–30 years (i.e., beyond the nominal historical lifetimes of about 35–45 years). Research issues are generally related to materials behavior, methods of boiler repowering, electrical and mechanical equipment reliability, and design of turbine generators and environmental control systems. In nuclear plants, the critical issue is the demonstration of annealing to remove radiation-induced effects in certain key components, such as the pressure vessels, coolant piping, and control systems. A focused program of federal research, in conjunction with industrial support, could be important in providing basic and applied research support for many facets of this problem.

Energy Storage

Finally, improved energy storage systems (for electricity, synthetic gas, etc.) is another area in which innovative research is needed. Load-leveling through the use of storage systems could significantly delay the need to construct new energy facilities by increasing the utilization (load factor) of existing facilities.

Research on all of these subjects must address their technical and economic viability, while bringing full consideration to the need for acceptable ways to handle or satisfactorily mitigate their environmental impacts. In the case of certain technologies, pilot facilities must be built and operated to validate performance and costs, to establish a baseline of environmental impact data, and to demonstrate the successful mitigation of such impacts. There is an appropriate role here for federal participation (direct or indirect) to ensure that promising options are effectively brought to commercialization.

Mineral Resources

Mineral resources have contributed significantly to the development of a strong industrial base in the United States. Common characteristics of the mineral deposits that are now being exploited are that they lie deep in the ground, are low in grade, and are difficult to process. *Challenges that the mineral industry in the United States will face in the future include finding new deep ore bodies and developing technology for the processing of increasingly low-quality, finely dispersed ores*. Mining and processing of these ores will have to use energy efficiently and be accomplished within stringent environmental constraints. Most important, the technology has to be made more efficient than it currently is in order for domestic products to be economically competitive in the international market.

Mineral resource recovery generally involves extracting the material out of the earth, comminuting it to a size such that the mineral grains are liberated from each other, and then separating the valuable mineral particles from the waste rock. With complex, fine-grained ores, very fine particles that resist treatment are often produced. *Better technology is required to improve size-reduction technologies, the processing of fine particles, and the disposal of wastes*. In some cases, chemical treatment of complex ores may afford an opportunity to exploit them. In certain cases, in-situ methods can replace the mining and the physical concentrating step altogether.

Because existing mining and processing techniques are not fully adequate for exploiting many of our low-quality domestic reserves, there is a great need to develop new techniques. Important opportunities in this regard are

- the development of sensors and instrumentation for exploration, remote control mining, and metallurgical operations;
- the continued development of computer-assisted design and systems analysis of the entire mining and extraction process;
- the application of new technology based on photoelectro-chemical, colloidal, and biological processes for developing new concentration and effluent-treatment techniques; and
- the development of a fundamental data base on the behavior of rocks and minerals during fracture (mining or crushing), dissolution (solution mining and hydrometallurgy), and adsorption and flocculation (mineral beneficiation).

Sensors

Research in the development of new geophysical methods—for example, electromagnetic sensing—should be undertaken to provide technology useful in exploring for ore bodies that lie several hundred feet below the surface. *Development of sensors and remote control equipment for automatic mining is important both to enhance productivity and to reduce health and safety hazards.* There is a similar need for sensors in computer control of all mineral processing operations, including grinding, classification, flotation, flocculation, and electrowinning. In addition, we need to develop a better understanding of the physicochemical behavior of particles, aggregates, and dissolved species of minerals as well as the impurities in process streams.

Eventually, the domestic mineral industry may have to go to the ocean and its floor for many minerals; yet the technology for exploring, mining, and processing such deposits is far from established. A better understanding of the origin and localization of deposits should help in identifying the best sites for exploration, whether on the ocean floor or on land.

Systems Analysis and Control

Design and systems analysis of the entire mining and extraction process to increase the overall efficiency of mining is equally important. It will be necessary to formulate quantitative descriptions of the operations used in mining and processing, with particular attention to possible complex interactions between various operations on different scales. A major overall consequence of decreasing ore grade has been an increase in the scale of mining and processing operations. However, scale-up principles are not yet adequately established. It is important to develop the required basis for scale-up as well as scale-down for process equipment. Such developments should help increase the productivity of mines and mills.

In-Situ Leaching and Burning

On a larger scale, new opportunities exist with in-situ operations using leaching and burning techniques. In this regard, information needs to be developed on the geological structures involved, as well as the techniques for preparing the entire body to accept and use the leaching solutions. Methods to better delineate subsurface geological structures or underground fracturing of rock are required. An understanding of the flow of solutions through geological pores also needs to be developed so that possibilities for groundwater contamination can be estimated with sufficient reliability.

Colloidal and Biological Processes

Leaching also holds great promise for the processing of very low-quality ores, particularly using microorganisms genetically engineered for increased efficiency and for higher toxicity tolerance. Interdisciplinary research is needed to derive information on the electrochemical and colloidal behavior of mineral fines and microorganisms in various media. Interaction among microbiologists, physical chemists, and mineral engineers should prove fruitful in this endeavor.

A complete understanding of the surface and colloidal chemical interactions of fine particles in aqueous media containing various electrolytes, surfactants, and polymers is needed in order to utilize fully certain techniques based on selective aggregation that have emerged recently for the treatment of ultrafines. Selective flocculation processes hold tremendous potential when followed by flotation, elutriation, and so on. Currently, however, application is limited to a couple of ore bodies and it has become clear that further development will depend on our understanding of all combinations of particle/particle/water/oil/gas interactions in the submicron size range.

Size Reduction Methods

Similar problems exist in developing efficient techniques for comminuting the mineral to the fine size range suitable for the abovementioned processes. The notoriously poor efficiency of communition processes in terms of energy consumption and indiscriminate intragranular fracture continues to be the most serious hindrance for the effective processing of mineral raw materials. Here again, *what is required is the development of an understanding of the microprocesses involved in the fracture of mineral grains and transport of the particles in the grinding and classification streams*, along with an understanding of the manner in which these processes are influenced by changes in the hydrodynamic and chemical properties of the environment around the particles.

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APPENDIX RESPONSES TO THE ENGINEERING RESEARCH BOARD'S REQUEST FOR ASSISTANCE FROM UNIVERSITIES, PROFESSIONAL SOCIETIES, AND FEDERAL AGENCIES AND LABORATORIES

Requests for assistance were sent by the Engineering Research Board to a number of universities, recipients of Presidential Young Investigator Awards, professional societies, federal agencies, and federal and national laboratories in order to obtain a broader view of engineering research opportunities, research policy needs, and the health of the research community. Some of the responses included comments on engineering research aspects of energy, resources, and the environment; these were reviewed by this panel to aid in its decision-making process. The panel found the responses most helpful and wishes that it were possible to individually thank all those who took the time to make their views known. Because that is not practical, we hope nevertheless that this small acknowledgment might convey our gratitude.

Responses on aspects of energy, resources, and the environment were received from individuals representing 44 different organizations (Table A-1): 21 universities (including 4 represented by recipients of NSF Presidential Young Investigator Awards), 9 professional organizations, and 14 federal agencies or federal and national laboratories. Some comments covered specific aspects of the panel's scope of activities, whereas others provided input on a variety of subjects.

Research Needs

Research needs that were recommended as being of high priority are summarized in Table A-2. In the energy field, the single most frequently cited priority was for research on coal. Coal was recognized as a major energy resource within the United States, and one that is in need of much greater development. Almost all recommendations on this topic dealt with the environmental problems associated with sulfur and nitrogen in coal as it relates to acid rain, and emphasized the need for better ways to clean coal or to remove oxides of nitrogen and sulfur after coal combustion.

Table A-1 Organizations Responding to Information Requests Relevant to Energy, Minerals, and Environmental Systems Research

UNIVERSITIES AGENCIES
Cornell University
Duke University
Northwestern University
Princeton University
Rensselaer Polytechnic Institute
Syracuse University
Texas A&M University
University of Arizona
University of California, Davis
University of California, Los Angeles
University of Florida
University of Georgia
University of Hawaii
University of Illinois-Urbana/Champaign
University of Michigan
University of Minnesota
University of Missouri, Columbia
University of Pennsylvania
University of Texas at Austin
University of Utah
University of Wisconsin

PROFESSIONAL ORGANIZATIONS
American Academy of Environmental Engineers
American Chemical Society
American Institute of Chemical Engineers
American Society of Civil Engineers
American Society of Mechanical Engineers
Council for Chemical Research
Institute of Industrial Engineers
Society of Engineering Science, Inc.
The Institute of Electrical and Electronics Engineers, Inc.

AGENCIES AND LABORATORIES
Air Force Office of Scientific Research
Argonne National Laboratory
Brookhaven National Laboratory
Jet Propulsion Laboratory
E.O. Lawrence Livermore National Laboratory
NASA Ames Research Center
NASA Goddard Space Flight Center
NASA Langley Research Center
NASA Lewis Research Center
National Center for Atmospheric Research
Oak Ridge National Laboratory
Office of Naval Research
Pittsburgh Energy Technology Center (DOE)
Sandia National Laboratories

Table A-2 Energy, Minerals, and Environmental Research Areas Most Frequently Cited by Responding Organizations

Research Area	Number of Organizations Citing
Energy	
Coal	12
Fission and fusion	10
Alternative sources	16
Storage, transmission, efficiency	14
Environmental tradeoffs	13
Environment	
Contaminant movement, fate, effects	27
Hazardous waste control and management	9
Acid rain	11
Groundwater contamination	12
Water reuse, conservation	13
Monitoring and sensors	11
Combustion processes	15

Many comments concerned the need for more research on combustion technology, air cleaning processes, and the movement, fate, and effects of contaminants.

Research on nuclear fission and fusion was recommended by several respondents as a major alternative to fossil fuels, considering both the environmental problems associated with fossil fuels and the long-term problem of limited energy resources. Many respondents were concerned with the dependency of the United States on other nations for fuel, the impact it could have on industrial development, and related strategic problems. The need for a diverse energy supply was noted by many, with several recommending the development of alternative sources, including solar energy. For similar reasons, energy conservation, efficiency in energy conversion, and better methods for storing and transporting energy were frequently mentioned.

The majority of respondents noted that environmental concerns were associated with the development of most major forms of energy, and that energy development had to go hand-in-hand with safeguards against environmental deterioration. Consequently, research in environmental areas associated with energy development was frequently given high priority.

Regarding engineering research on environmental questions, there was a general need expressed for more knowledge about the movement, fate, and effects of chemicals in the environment, including the air, land, and both surface and groundwater. Respondents believed that this is an important avenue for research that will have significant impacts in all technological areas of development. Associated with this need was the need for research on hazardous waste control and management, and chemicals associated with acid rain and groundwater contamination. A frequent recommendation was for research on combustion processes associated with burning coal, hazardous wastes, and other materials, as well as in vehicle transportation. The limited water resources in many areas of the country resulted in several recommendations for research on water reuse and conservation. In addition, many respondents expressed a need for better monitoring tools to track pollutants in the environment, and also for sensors that could be used to discover and track contaminants through treatment processes and in the environment.

Policy and Health Issues

Whereas most of the responses addressed priority research needs, several respondents did reflect on policy issues. Concerns were frequently expressed about the recent decreases in funding for basic and long-term engineering research in both the energy and the environmental fields. In this regard, several respondents noted that the national laboratories are obtaining a greater share of the remaining funds, leaving the university research programs vulnerable and in a state of declining health. The significant adverse impact this would have on the important role of universities in educating research engineers needed for the future was pointed out a number of times. Some also believed the recently established NSF engineering research centers would lead to less funding being available to researchers at universities or in programs not linked to the centers. Also of concern were the fluctuations in research funding that make continuity of research programs difficult to sustain.

Many of the research needs and issues of policy and health addressed by the respondents were similar to those noted by panel members. The broadened perspective provided by the survey responses was most beneficial in the panel's deliberations.

Information, Communication, Computation, and Control Systems Research in the United States: An Overview

EXECUTIVE SUMMARY

In today's world, information is the key to a successful technology-based society. The speed and effectiveness of information communication, processing, and use are critical to the success—or failure—of our national economic enterprise. Entire industries, such as banking, insurance, and law, have become dependent on computer data bases. Airline ticketing offices and the stock exchanges can handle an enormous volume of transactions with the use of modern information processing technology. Information, communications, computation, and control (IC^3) technologies are also crucial elements of our national defense, both in its management and its weapons systems. Because it is so pervasive, this area of engineering can be considered basic to all others as the world moves into the information age.

That pervasiveness means that IC^3 technologies are now the focus of intense competition among nations for technological leadership and the domination of the enormous commercial markets. Indeed, no battlefield in the struggle for international competitive superiority in technology is as strategic as this one. It is an area in which the United States has led the world, but in which we are being severely challenged by foreign competition. Our economic survival as the leading technological nation depends on our performance in key areas of IC^3 technologies.

At the center of these information-based technologies is the concept of an "information system." An information system consists of an input, at which information is gathered; a processing section, at which computation takes place; and communication of the information from input to processor and from processor to a point of control, an output, or both. Such systems are basic to all intelligent processes. The engineering research problems they represent have grown enormously as our ability to transmit and process greater quantities of information per unit time has increased, making possible orders-of-magnitude greater complexity and productivity in every area of information processing and control.

Information systems consist of hardware (i.e., sensors, actuators, operating devices, and other subsystems in their physical embodiments) and software (i.e., the set of instructions that govern system or subsystem operation). System design requires that the functioning and interactions of both the hardware and the software be understood, and that these elements be efficiently integrated and implemented. In general, the trend has been for information processing hardware to decrease in size, weight, and cost while becoming faster and more powerful. At the same time, software has tended to become more complex. Although there has been a rapid increase in the complexity and performance of hardware over roughly the past two decades, our ability to design and produce software has improved less rapidly. Greater productivity in software development needs to be emphasized in our national engineering research priorities, along with strong support for continued increases in hardware performance.

Recommendations

1. The speed of electronic devices has increased, whereas size, power dissipation, and cost have decreased dramatically over the past two decades. We recommend that research into materials, processing, circuits, and interconnection technologies for IC^3 devices and components be given high priority so that the nation can maintain and strengthen its competence and leadership in these areas.

2. The complexity of computer-based systems has increased because of advances in technology and escalating end-user requirements. Increasingly sophisticated architectures and operating systems are necessary to make such computer systems operate effectively. We recommend that increasing the power of complex systems and the productivity of software development for them be set as national engineering priorities. This will require substantial progress in architectures (especially distributed and parallel processing), in the integration of software, in data base management, and in large-scale communication networks.
3. The versatility and end-user friendliness of IC^3 systems can be greatly improved. We recommend increased attention to both input devices (e.g., sensors, text and speech recognition transducers) and output devices (e.g., graphics displays, speech synthesizers, robotics manipulators) so that the true potential of advances in IC^3 can be realized by humans as well as by technological systems.
4. Government, industry, and universities should address the inadequacies of present engineering research facilities and equipment in universities, so that future practitioners and faculty in IC^3 technologies can be educated to fulfill a critical national need.
5. Universities should evaluate their organizational structures and their reward systems so that more cross-disciplinary work can flourish in both engineering research and teaching.

INTRODUCTION

No battlefield in the struggle for international competitive superiority is as strategic as that of information, communications, computation, and control (IC^3). These disciplines are at the root of our national strength and health in most of the key components of industry and national defense. Furthermore, they are areas in which U.S. leadership is seriously challenged by international competition, mainly from the Far East. We must exert our best efforts to maintain high-technology leadership where we have it, and to regain whatever leadership we have lost. This is a matter of national urgency; the outcome of the competition is by no means decided. If the United States is to remain economically, militarily, and politically strong, information has to be more quickly communicated to the point of decision and more quickly assimilated. To accomplish that, the country must place a high priority on key parts of these areas of engineering.

Each of the disciplines of IC^3 contains both a hardware technology component and a systems and software component. We must have vital engineering research programs in each of these areas. It is a fallacy to believe that we can emphasize either sector and leave the other to the competition. Software and hardware most strongly interact precisely at the leading edge of technology development. That is where advances in one sector can be capitalized on most rapidly by the other in the creation of new capabilities and new systems.

The United States has been a world leader in integrated circuits and other aspects of hardware technology, as well as in software systems design. *It is of critical importance not only that we continue to lead in these two areas, but also that we more effectively integrate progress here*. The panel notes that academia, which is funded for research primarily by government agencies in these areas, is not always able to find equipment and funds for some types of research that are important to industry. For example, bipolar integrated circuits are the foundation of most high-speed, large-scale computers; however, metal oxide semiconductor-type integrated circuits predominate in university research and teaching. Matching that portion of university research that is directed toward future national needs with a more clearly defined perception of what those needs are likely to be is a continuing challenge.

Finally, advances in IC^3 technology are crucial to several other important areas of engineering. Manufacturing now depends on robotics and automation, which in turn require a host of advances in effective and reliable computation, communication, and control. Engineering design of a vast range of key products and systems—such as computer chips, automobiles, buildings, and industrial processing—depends critically on IC^3. Therefore, it is essential that we consider the measures that can be taken to strengthen engineering research in these areas.

Given the importance and impact of these technologies on defense, in industry and commerce, and in the lives of individual citizens, coupled with the strong research base that their advancement requires, it is not surprising that government support and policies play an important role in determining the pace and vigor of research in these fields. Thus, in addition to identifying key research needs, the panel also examines federal policy toward research in IC^3 technologies as well as those policies that affect the conduct of engineering research in general.

In a concluding section, special topics relating to the overall health (both present and future) of research in these areas are addressed. Particular attention is paid to the working environment for university faculty in IC^3 and to the adequacy, both quantitative and qualitative, of graduating engineers who form the talent pool for research.

RESEARCH NEEDS—THE MOST IMPORTANT AREAS OF INFORMATION, COMMUNICATIONS, COMPUTATION, AND CONTROL SYSTEMS RESEARCH

In view of the critical importance of research advances in this field to our national competitiveness and security, this section of the report is the primary focus of the panel's work. The panel considered more than 70 distinct research topics in the broad categories of research encompassed by its scope of concern. In the course of its evaluation, the panel took into account the opinions expressed by engineering deans and faculty, selected researchers, and various professional engineering organizations in response to a survey conducted by the Engineering Research Board (see the Appendix).

The results of this assessment are discussed in the following pages. The discussion is organized in terms of (1) the hardware and (2) the software and systems research needs associated with IC^3 technologies. It is important to bear in mind the point made in the introduction: that hardware and software/algorithms in IC^3 are two indispensable sides of the same coin and must be supported equally. Across the spectrum of engineering research needs identified here, there is an urgency with respect to national interests that must not be ignored.

A number of the identified research topics reflect the panel's perception that the use of information processing systems is often limited by the input of real-world (i.e., analog) information into the systems, and by the output of information in a form that is usable either for control or for human interpretation. Thus, input and output devices and their associated software and algorithms are a bottleneck in the full application of systems whose processing capabilities have, at present, exceeded their general utility.

Hardware Elements

IC^3 systems are constructed from hardware elements. Rapid improvements in the function, density, and performance of these hardware elements have been the driving force behind the revolution in electronics. Continued progress requires ever-increasing sophistication in our ability to control the materials and processes required for component fabrication. This is extraordinarily fertile ground for the expansion of engineering research to solve problems relating to international technological competitiveness.

The issue of how to achieve vastly improved manufacturing techniques for the production of IC^3 hardware, especially computer and communications devices, is very important. A detailed discussion of IC^3-related manufacturing research needs is outside the scope of this report. These issues are addressed in general in the report of the Panel on Manufacturing Systems Research of the Engineering Research Board. Some of the issues specific to the fabrication of computer devices are addressed in the report of the board's Panel on Materials Systems Research. (Both reports are in this volume.)

Computer Devices

Several categories of devices underlie progress in computation; these are integrated circuits, interconnection structures (so-called "packaging"), and magnetic and optical storage. In addition, the need for improved methodologies for testing these devices is becoming very pressing. Continued progress in theoretical and experimental research in each of these areas is vital to the future health of the U.S. computer industry. (See also the discussion of "Semiconducting and Magnetic Materials" in the report of the Panel on Materials Systems Research.)

Integrated Circuits

Progress in integrated circuits for computers takes place in two principal directions: logic circuits and memory circuits. In the case of memory, the principal thrust (as is well known) is increasing density, as measured by the number of bits stored per chip. In the case of logic chips, there are again two principal thrusts: high performance, as measured by circuit switching speed, and high density, as measured by total number of logic circuits per chip. Very large-scale integration (VSLI) has been applied to memory and to logic circuitry. However, none of the very large-scale, general-purpose computers, and few engineering-scientific supercomputers, rely on metal oxide semiconductor VLSI for their critical circuitry. Instead, the hundreds of thousands of logic circuits in these economically and scientifically important computers are almost exclusively bipolar silicon logic circuits. These circuits are much faster than microprocessors, although they are of substantially lower circuit density, and are usually the best means for producing the desired high performance. *The continued improvement and enhancement of bipolar silicon logic is of great significance to the future well-being of U.S. computer technology. In the minds of some, its significance will be as great as the continued improvement in VLSI memory and logic density, size, and performance* . In both VLSI and bipolar technologies there is substantial engineering research to be done; however, the panel notes the remarkably small amount of attention given in universities to the technology that is fundamental to the present and future success of large-scale, general-purpose and scientific computing, namely, silicon bipolar devices, processes, and chips. International competition in this area is very keen, with great strides being made in Japan.

High-Density Structures and Fabrication

Two advances in semiconductor devices are critical to continued performance improvements of integrated circuit-based systems: *(1) advances in submicrometer device structures and fabrication methods and (2) advances in three-dimensional (3-D) devices and circuits*. Both of these research efforts seek to continue the growth in complexity at the integrated-circuit chip level, while improving speed and function. Both tasks pose substantial challenges for industrial and university laboratories alike. The fabrication of submicrometer structures requires special process equipment whose registration and resolution are capable of dimensions less than the wavelength of visible light. Similarly, successful 3-D structure processing* requires sophisticated equipment capable of depositing defect-free materials.

Packaging and Interconnection Technology

Because large-scale, general-purpose and scientific computers require so many high-performance, relatively low-density chips, the "package" that interconnects their hundreds of signal and power connections is of crucial importance to achieving desired system performance. This desired performance involves fast signal propagation, rapid heat removal, and highly reliable mechanical and electrical interconnection with the next level of the packaging hierarchy.

Improvements in signal delay, power dissipation, and parasitic coupling all require more sophisticated technology than is now available. Although these have not traditionally been viewed as technically interesting problems, their solution is essential to achieving the highest possible system performance. Ceramic and polymer engineering, thin-film metallurgy, the mechanical properties of composite structures, and optical interconnection are all relevant disciplines here, and all are areas in which university research can and should be vigorous. Novel approaches to package design are of potentially high leverage, and much more innovation is needed.

Magnetic and Optical Storage

With regard to information storage media and devices, there are a number of promising approaches that can increase storage density by several orders of magnitude. For magnetic storage, both *vertical recording and signal encoding techniques* offer significant improvements over the current state of the art. Vertical recording offers a means for packing magnetic transitions more densely; signal encoding techniques offer a way of using those transitions to store information more efficiently. Run-length coding is one form of signal encoding in use today that achieves about two to three times the storage capacity of unencoded data. Yet this gain is small compared to what is theoretically achievable.

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* 3-D devices have circuit elements stacked vertically to conserve space on a chip.

Optical storage technology has developed to the point that it could potentially supplement magnetic storage for auxiliary memory. However, *erasing and retrieval of stored information* is not possible for some optical storage devices, and for others it is far slower and more costly today than for magnetic storage systems. Research into optical storage may be able to eliminate these and other drawbacks of the technology so as to create storage systems superior to magnetic storage.

As in many engineering research fields, the application of computer-aided design (CAD) technology is crucial in improving the productivity and quality of all forms of computer logic and memory circuit design. Engineering research on new design tools and methodologies has played and will continue to play a vital role in this aspect of the technology.

Hardware and Subsystem Testing

CAD is also of great potential value for testing devices and subsystem assemblies. The chips and packages of present and future computer systems are so complex that their testing and simulation now consumes a large amount of time and adds substantially to their cost. Even the methodology for economical testing of large-scale logic chips and systems is poorly understood and needs further work. Testing should be able to be accomplished on three levels: (1) the design phase (i.e., is it a good design?), (2) the chip-manufacturing phase (i.e., does each chip work?), and (3) the in-operation phase (i.e., self-testing by the chip to ensure that it is functioning correctly). New types of testing methods, such as "contactless" E-beam and laser-assisted testing, are needed. These methods will require considerable research.

Communications

The devices and components that support communication also offer substantial challenges to engineering research. Device research for *optical communication* is a very important area, one in which the Japanese are very strong and in which there is enormous commercial potential. Devices that permit the *switching* of light signals from one transmission channel to another are emerging as an important area of research; this switching technology currently requires more attention than does the technology of optical transmission, which is being well researched. Research on devices that efficiently *transform* electrical signals into optical signals, and vice versa (i.e., lasers and detectors), is of great technological importance in communications. Particularly promising materials include both the III-V and II-VI compound semiconductors. Important and novel structures include superlattices and quantum wells, in which a strong interaction between optical and electrical signals can be obtained.

In order to build the all-optical communications systems of the future, a number of devices familiar in radio will need to be invented in the optical domain. Such optoelectronic devices include amplifiers, filters, isolators, multiplexers, and switches. These optical "plumbing" building blocks will enable the implementation of multichannel communication systems similar to those found in radio transmission, except that the capacities of the optical systems will be enormously greater than their radio counterparts. After these optical devices have evolved, still another generation of development and research will be required to integrate them onto microchips in the manner in which electronic circuitry is now produced.

Sensors for Control of Systems

A great deal of progress has been made in general computation; however, not enough progress has been made in getting information to the computer. The hardware elements that gather data are *sensors*. These input devices translate temperature, movement, thickness, flow, and many other physical parameters into electrical signals that may be used in communication, computation, and control applications—for example, in automation of the total manufacturing process. *There is a great need for improved sensitivity, linearity, resolution, wavelength response, and degree of miniaturization and integration of sensors, as well as for sensors for parameters not now amenable to sensing* (e.g., gas-phase chemical composition or the fidelity of a manufactured part to desired specifications). We note, moreover, that real-time control is dependent on the ability to rapidly and accurately sense process and control variables, so that in certain applications it is the sensor and/or actuator technology that is limiting further progress. The development of sensors is important for improving control of large systems in which the placement, location, and number of sensors is a critical matter. Thus, research on sensors and actuators is greatly needed.

System Architecture, Algorithms, and Software

The foregoing discussion dealt with hardware-oriented research needs in IC^3. To make those devices and components functional—that is, to give functionality to the systems in which they are embedded—requires corresponding advances in systems architecture, algorithms, and software. Indeed, many of the most difficult problems today in the IC^3 field are in designing special computer architectures, in producing enormously large and complex software programs, and in evolving efficient computational algorithms for problems of overwhelming size.

Communications

Many U.S. researchers are now working on problems in the communication field. However, certain problems still require high-priority attention. For example, one of the largest software efforts in coming decades will be the development of a system underlying the national communication network. This network will eventually overlay the current telephone network with a fiber-optics-based network combining voice, data, and video transmission. The majority of transmissions will be in a digital format. Switching will be accomplished by the time-interchange of bits within transmitted data streams, and by the routing and storage of packets of information containing address headers. The need for traffic and flow analysis of these streams of packets, and for efficient and reliable protocols to manage the interchange of data, has grown in recent years.

As long-haul communications have become more and more efficient, much of the research interest has turned toward the bottlenecks in local distribution and collection. Networks of users (generally computer terminals) within the area of a building, a campus, or an industrial complex require methodologies for sharing a broadband medium such as a coaxial cable or fiber. Such a network is known as a local area network. On the next higher level, the breakup of the Bell System and the evolution of a range of choices for communications access have led to increased interest in metropolitan area networks to link large buildings and multiple users in an efficient manner to the long-distance network. In all of these problems, computer software and algorithms play a key role. Some of the largest computer programs, requiring hundreds of man-years of development effort, are required to direct the switching activity of a modern telecommunications node.

Computer Software

Large software systems (for example, the operating system of a large multiprocessor) are some of the most complex creations ever devised. Our ability to design, code, test, and modify large software systems has improved somewhat in the past decade. Yet the need for such systems is outstripping our current capabilities. A sustained basic research program on *methodologies for the efficient development of large software systems* is sorely needed. One thing that would enhance software productivity would be the ability to integrate software subsystems. Methodologies are needed by which software can be made readily integrable with other software. Also important is research on the compatibility, reuse, and standardization of key software modules (e.g., floating point modules, encryption algorithms, and communication protocols).

An issue that becomes increasingly important with the growth in the size of software systems is the general issue of *reliability, testing, and verification*. Continuing attention needs to be paid to this issue in order to combat the combinatorial explosion of testing sequences that occurs as systems get larger and more complex.

There has been much discussion about *distributed computer systems* in recent years. Yet many of the basic issues have not been properly addressed. These issues would benefit from increased attention by academic researchers; this would complement the intense interest in distributed processing in industry. Communications networks currently permit computers in different places of the same or different architectures and software environments to "talk" to each other at a relatively low level. What is needed is a way of linking such computers and their associated data bases with each other and with large, central data bases so that a wide array of services is performed in a manner that is both efficient and transparent to the user.

Some types of sensors (e.g., those mounted on satellites) generate enormous amounts of data for input to processing systems.

Additional work is needed on the development of techniques, such as data bases and signal processing algorithms, that will permit *real-time processing of data* generated in such large volumes.

Parallel Computation

Within this century, improvements in the technology underlying computing can be expected to yield increases in speed of one or two orders of magnitude. In certain applications, the demands for computing power are far greater than are likely to be met by speed improvements in components and by evolutionary improvements of the basic (von Neumann) computer architecture. Certain approaches that have the potential for at least a thousand-fold improvement in speed should be pursued. The most promising areas for research are in architectures for parallel computation.

Special Purpose Parallel Architectures

This area of *real-time processing* often lends itself to very specialized architectures (e.g., systolic arrays) that promise great speed at relatively low cost. Similarly, there are great advantages to specialized parallel architectures for processing *visual inputs*. The complex memory structures used in *artificial intelligence* lend themselves to computer organizations containing a million or more nodes that interact in parallel.

Parallel Architectures for Numerical Computation

Current supercomputers are usually based on machines that can manipulate vectors in parallel. Such designs can yield one or two orders of magnitude of additional speed, especially by increasing the number of vectors that can be manipulated simultaneously.

The NSF's program on supercomputing will give the academic community access to current and next-generation machines. This opportunity should improve our ability to design algorithms that effectively use the available parallelism. In particular, increased attention needs to be paid to redesigning the *numerical algorithms for parallel architectures*.

Attention is beginning to be paid to *non-vector-oriented* parallel architectures for numerical computation. Such architectures appear to be needed for solving physical problems (e.g., weather prediction, fusion machine analysis, and viscous fluid flows) in their full 3-D form and within a reasonable length of time.

General Purpose Parallel Architectures

The greatest risk, and possibly the biggest payoff, involves parallel architectures that are intended for relatively general application (e.g., data flow computers). It is not clear whether there are many problems that lend themselves to such massive parallelism. It is also not clear whether the architectural designs can deliver the promised speeds.

One of the major applications for these computers is rule-based expert systems (see the following section). The development of such a system is one of the goals of the Japanese Fifth Generation Project. Such architectures are also being investigated in Europe and the United States (viz., the Strategic Computing effort of the Defense Advanced Research Project Agency—DARPA). Clearly, much could be learned from the continued analysis and experimental construction of such machines.

Man-Machine Interactions and Artificial Intelligence

The interface between the analog and digital worlds is seen most distinctly in man-machine interactions. This interface is often not very "friendly," and it is here that some of the most notable bottlenecks exist in our attempt to bring computing power into our everyday lives for practical uses. We discuss three areas of needed research next.

Rule-Based Expert Systems

Recently, there has been much interest in applying the technology of rule-based expert systems to the solution of a wide range of problems. We are somewhat concerned that the approach has been oversold. Nevertheless, this concept has broadened, in a very real way, the general idea of what computers can do. Namely, there is a growing realization is being widely realized that computers, like people, can be quite useful in areas in which a complete algorithm does not exist.

Increasing the number of rules often degrades the speed of rule-based systems. This problem is being addressed by designing *massively parallel* computers, as noted previously. Increasing the number of rules can also degrade the effectiveness of rule-based systems because of the complex interactions of new rules with existing ones. This problem is being addressed by *restructuring rules* in a variety of ways so that the complexity of interactions is reduced.

Knowledge-Based Systems

A rule-based expert system is a specific type of "knowledge-based" system. The first knowledge-based systems were large systems such as MACSYMA, for mathematical formula manipulations, and Dendral, for analyzing molecular spectra. Such large knowledge-based systems can be characterized as requiring long-term basic research into a particular problem area (e.g., medical diagnosis) in which a key difficulty is the representation of the large amount of knowledge that humans have about the given area. Additional basic research on the particular input problem of *representation* is clearly needed, as are further examples of large, knowledge-based systems.

Natural Language Understanding

If computers had the capability to understand typed queries in natural language, it would greatly facilitate and improve the interaction between man and machine—computers would be far more "user friendly." Natural language understanding presents very challenging long-term research problems. Much progress has already been made on the syntax of natural language. Most of the challenge now lies in the area of *semantics and discourse*.

Robotics and Automation

Robotics and automation present exciting opportunities and major challenges for IC^3 systems research. This is a prime example of the need to improve both the input and output of data. Coordination of multiple flexible manipulators, coordinated control of multiple robots, and automation in manufacturing require complex decision making and real-time control involving enormous communications and computations. Integrated research in computer vision, artificial intelligence, decision and control theory, large-scale system theory, algorithms for decentralized computation, and data communication should lead to major advances in robotics and automation. *The emphasis here is on the integration and merger of the separate research areas into a unified approach to problems in robotics and automation*.

For further discussion relating to the applicability of robtics and automation to the manufacturing process, see the report of the Panel on Manufacturing Systems Research.

Control Systems

The development, planning, design, and operation of systems for communication, computation, defense, transportation, etc., necessarily involve the issue of real-time control. Control is concerned with the determination of decision variables on the basis of feedback signals from sensors and other data-gathering instruments in such a way as to meet system objectives. Real-time computation and data processing are necessary to produce decision variables from sensor data. Complex, large-scale systems involve a large number of decision variables and a great many system variables whose status is monitored by sensors at a high data rate. It is not always possible or desirable to centralize information gathering and the real-time computation of decision variables.

One of the most critical needs in control systems research is the investigation of decentralized control strategies in which local decision variables are determined on the basis of local sensor signals . As mentioned earlier, with large-scale systems the determination of what system variables should be measured and the proper number and placement of sensors to achieve optimum overall effectiveness are both extremely important. Because local control based on local sensor data ignores data gathered elsewhere, local perceptions of system behavior may differ. Furthermore, local decision variables generally affect system variables elsewhere. Thus, each set of local inputs and outputs leads to a different model of the same system.

There is a need for a theory underlying the behavior of large-scale systems. Research is needed to determine what computational algorithms are necessary for decentralized control of large-scale systems. Multimodeling, multicriteria optimization, stabilization, and coordination are some fundamental issues in decentralized control.

POLICY ISSUES ON FEDERAL SUPPORT OF RESEARCH

Scope

This section of the report focuses on the federal government's role in, and influence on, fundamental engineering research in IC^3. Such research is performed in universities, national and federal laboratories, industry, and cooperative or collaborative institutions. Federal policies have a great impact on the vitality, effectiveness, and potential usefulness of such research.

The federal government's role includes

- Direct support by the NSF, whose unique and specific responsibility is support of fundamental research in universities and related research centers, as well as support of graduate education.
- Direct support by mission agencies such as the Department of Defense (DOD), NASA, etc.
- Encouragement, through tax incentives or other means, of research performed by industry (both by individual companies and consortia).
- Encouragement, via tax incentives and other means, for industry-university cooperative research and education.

Issues

The NSF'S Role in Support of Academic Research in IC3 Systems Engineering

IC^3 system technologies have made phenomenal progress over the last two decades. Much of this advance has been based on outstanding research performed in industrial laboratories, where the necessary state-of-the-art experimental facilities and computational capabilities are available. Nevertheless, academic engineering research is absolutely essential for the future health of these cutting-edge technologies.

Electronic and information engineering research is closely tied to advances in the physical, chemical, and mathematical sciences. One need only review the remarkable progress made in advancing these technologies in organizations such as the Bell Telephone

Laboratories, the IBM Research Laboratories, and several outstanding university research centers to appreciate the value of coupling fundamental science and engineering research. Synergism between science and technology benefits both, and is one of the great strengths of the American science and engineering enterprise.

Although industry performs about 75 percent of the R&D in the United States, it performs a much smaller fraction (15 to 20 percent) of the basic research (National Science Foundation, 1983). The structure of American universities permits the interaction of electronic and information engineering research with its scientific and mathematical base. Therefore, it is important to continue to enhance academic research in these forefront engineering fields and to encourage interdisciplinary research programs that combine the appropriate science and engineering disciplines. It must also be recognized that increasing competitive pressures in communications and computation tend to force industry into a search for near-term results; we must look to the universities for much of the research that will result in the long-term, unforeseen advances in these fields. The NSF, which is responsible for supporting much of the science and engineering carried out in universities, is therefore a key agency for maintaining and strengthening IC^3 engineering research. (See, for example, Farber, 1985.)

The success of the engineering research enterprise is also highly dependent on the availability of a pool of doctoral-level professionals. Responsibility for graduate education rests with those universities that have strong academic research programs. The interaction of research faculty and graduate students is essential to the research enterprise, and provides the best environment for training graduate students for careers in research. The NSF and the federal mission agencies are major supporters of both academic research and graduate training through fellowships and research assistantships and through grants and contracts. They must continue to fill this important role.

The new NSF initiative to establish multidisciplinary Engineering Research Centers (ERCs) on university campuses can become a very effective supplement to its basic individual-investigator grant system. In the first year of the program, NSF established six ERCs involving eight universities. These six facilities are to receive $94.5 million over the next 5 years and are likely to receive significant industrial support as well.

Three of the six initial centers are in areas relevant to IC^3 systems research. The Center for Robotic Systems in Microelectronics at the University of California at Santa Barbara will investigate new automation technologies for the fabrication of semiconductor devices. The Engineering Research Center for Telecommunications at Columbia University will focus on communications networks to integrate various communications transmissions. The Center on Systems Research at the University of Maryland, in collaboration with Harvard, will conduct research on the application of artificial intelligence and VLSI to automatic control and communications systems.

With the addition of new ERCs, the total yearly budget of the Engineering Research Center Program is planned to reach $100 million in federal funds and is expected to attract substantial additional sums from private industry. The program will undoubtedly have a significant impact on the health and vitality of engineering research in the university environment.

The DOD's Role

Since World War II, the DOD has been a dominant source of support for basic and applied engineering research. Today more than ever, IC^3 systems provide the innovative and advanced technologies that are at the heart of our modern military systems. Command and control of our military forces, revolutionary advances in weapons system, worldwide secure communication systems, and surveillance systems are all critically dependent on modern electronics, computers, sensors, and control systems. It is therefore not surprising that the DOD has been the mission agency with the greatest need for (and with the largest support for) basic and applied research in the technical areas of interest to this panel.

The DOD's budget for R&D is divided into five accounts, two of which support fundamental engineering research. Basic and applied research, the 6.1 account in DOD parlance, sustains the science and technology base, whereas 6.2, the exploratory development account, also contains some elements of fairly fundamental research such as the architecture of a new generation of super-speed computers.

In FY84, universities received 50 percent of the 6.1 basic research funding. Federal Contract Research Centers received 20 percent, and government laboratories received 30 percent. DOD basic research funds are managed by a number of organizations, including the Office of Naval Research, the Army Research Office, the Air Force Office of Scientific Research, DARPA, and the Office of the Deputy Undersecretary of Defense for Research and Advanced Technology, as well as the various DOD laboratories. About one-fourth of the 1984 6.1 funding for electronics research was managed by DARPA, along with some 45 percent of the funding for mathematics and computer research. DARPA's program in support of computer research, computer communications, and artificial intelligence has been critically important for progress in these fields. Similarly, the Joint Services Electronics Program has been a major factor in the establishment of outstanding research centers at many major universities. Although this program has expanded somewhat, over the past decade, it has become proportionally less significant in the overall engineering research scene.

However, the FY86 DOD budget indicates a renewed realization of the need to enhance engineering research as an essential element in our national security. The FY86 defense budget contains a $25 million program to support high-risk basic research, multi-disciplinary centers, research equipment, and research fellowships. There are other initiatives as well. An example is the Software Engineering Institute, established by the DOD at Carnegie-Mellon University for research on the production of software for defense applications. The DOD plans to provide $103 million for this institute over the next 5 years. The research performed by the institute is expected to be unclassified and in the public domain.

This recognition by the DOD that the vitality and magnitude of fundamental engineering research and education are essential to the national welfare and defense is welcome. The DOD initiatives will sustain the needed growth of university research; they are indicative of a renewal of emphasis, on the part of DOD, on the long-term health of university research rather than on short-term defense requirements. The panel hopes to see this enlightened outlook and support continue.

There is some concern that continuing attempts by certain responsible and dedicated elements within DOD to restrict the dissemination of unclassified research at open meetings or in journal publications in the United States might impede the essential flow of internal communications, thus hampering the overall health of engineering research as well as DOD's harmonious relationship with the universities. Clearly understood and implementable guidelines must be established and adhered to by all concerned if we are to ensure that research results benefit the technical community while satisfying the legitimate needs of national security.

Recommendation

We recommend continuing the current system of support of fundamental engineering research by the various organizational elements of the DOD. We welcome DOD's new initiatives that enhance the vitality and funding of fundamental engineering research at the universities. We also want to emphasize that the effectiveness of university research lies in the investigation of longer term fundamental issues rather than in meeting the shorter term defense requirements that are the proper province of private industry. We also believe that any restriction on the dissemination of unclassified university research results may be detrimental to the research enterprise, and thus to the longer term advancement of defense programs.

The Federal Role in Encouraging Research in Industry

Although universities have the intellectual resources, the environment, and the incentives to lead in fundamental research, a great many of the engineering research breakthroughs in IC^3 have actually occurred in industrial laboratories. It is essential for the health of innovation in electronics and computers that government policies encourage research in the industrial sector. A number of very significant industrial research activities can be attributed to favorable government policies.

For example, most of the major DOD contractors maintain research laboratories that conduct competent research of great value to DOD agencies. A very significant and perhaps dominant portion of this research is initiated by the industrial research team and is partially paid for as an allowable overhead cost. It is designated Independent Research and Development (IRAD). Because IRAD efforts are funded on a cost-sharing basis, high-risk, high-payoff areas can be chosen for investigation. Furthermore, IRAD may be essential for maintaining the continuity of R&D funding through the vagaries of DOD budgetary fluctuations.

Congressional tax acts have provided a number of tax incentives to encourage industry to conduct R&D. A substantial portion (perhaps 10 percent) of the increased industrial R&D funds are used to conduct fundamental engineering research.

Recognizing the effectiveness of industrial consortia developed in foreign countries, the federal government has permitted industry, within the constraints of the antitrust laws, to form research organizations supported by companies in the same industry. The Microelectronics and Computer Technology Corporation (MCC), established in 1983 in Austin, Texas, is a prime example of such a consortium.

As development intensifies in the IC^3 field, a vast array of new products and services will continue to emerge. Innovations in areas such as software are already giving rise to major alterations in the concept of intellectual property. The extension of existing laws to cover new types of property, and questions about the enforcement of those laws in an environment in which there are often few obstacles to misappropriation, are drawing serious attention in Congress and elsewhere. The lack of an adequate policy in these areas will retard industrial research in certain areas of IC^3.

Recommendation

Fundamental research performed by industry is an essential ingredient in the progress of electronics and computer R&D. The federal government should continue to encourage industrial research through direct contractual support, through DOD IRAD programs, by means of tax incentives, and by encouraging research consortia to be organized where appropriate.

Industry-University Cooperation

Industry and universities have had a long and fruitful relationship. Academia has helped to maintain the scientific and engineering momentum that is the basis of our high-technology industry, both by providing trained manpower and by conducting the research that is part of the training process. Yet, today, industrial research actually leads the universities in some areas because the facilities and staff required are beyond the ability of most universities to afford, even with federal support. It has thus become necessary to develop, with the help of government, new approaches to industry-university research collaboration.

The NSF has established 20 industry-university cooperative research centers. The center at the University of Arizona, for example, was established to study the electro-optical properties of materials and structures for high-speed computers and communications. During its first year, it received joint funding from the NSF and a number of private companies. Currently, the NSF is establishing the Engineering Research Center Program, described earlier, for which industry funding is anticipated.

The Semiconductor Research Corporation (SRC), founded in 1982, has already organized centers of excellence, with major, long-term thrusts at three universities. The SRC has contracts at 32 universities and supports 300 students and faculty members. Its member companies include AT&T, Burroughs, CDC, DEC, DuPont, GE, HP, Honeywell, IBM, Intel, Motorola, RCA, TI, Union Carbide, Westinghouse, and Xerox.

In addition, a number of states have initiated successful programs involving joint state, university, and industrial participation. The California MICRO Program to support industry-university collaboration in microelectronics research is one example. The Microelectronics Center of North Carolina, MCC in Texas, and the Center for Industrial Innovation at Rensselaer Polytechnic Institute in New York are further examples of state, university, and industry cooperation to foster the establishment of technology centers of excellence.

All of these cooperative initiatives have the same underlying objectives (Office of Technology Assessment, 1985):

- improved research and new knowledge—mutual benefits are derived from the sharing of knowledge, funds, equipment, personnel, and technology;
- education and manpower—greater research activity and better facilities result in a strengthened academic program generally, as well as beneficial contacts with industry from the point of view of students and faculty; and
- economic growth—improvements in the research base and the technical manpower base lead to greater vigor in a region's overall economy.

Properly guided, and with sufficient sustained funding and a commitment to these goals, these initiatives have great potential for advancing the nation's strength in the research areas upon which they focus.

Recommendations

The essential character of university research in maintaining our leadership in high-technology industry, coupled with the high cost of adequate facilities and staff, requires cooperative initiatives to upgrade the facilities, capital equipment, and salaries of university faculty and research staff. These initiatives, if they are to be sustained, must provide direct benefits to industrial sponsors while retaining freedom of research and of publication by the universities.

We commend the federal and state initiatives for establishing research centers. These research centers should complement, and not replace, investigator-initiated research.

THE HEALTH OF THE FIELD: AN ASSESSMENT

The Health of the Educational System

In this section we examine the health of the educational system in the IC^{3}d. A primary focus is on the quality of life of university faculty in this field. Other important topics discussed are the status of equipment and facilities, and the cross-disciplinary research approach.

We also examine the adequacy of new talent, in terms of both quality and quantity at all levels (B.S., M.S., and Ph.D.). In particular, we comment on the continuing need to retain foreign Ph.D.s both in industry and academia.

Faculty

For a variety of reasons, the working environment for university faculty in virtually every engineering discipline has declined sharply in recent years. In those disciplines most closely associated with the IC^3 field, key issues include

- high student-to-faculty ratios;
- diminished ability to attract new faculty (especially recent Ph.D.s);
- salary levels;
- adequacy of research and teaching equipment;

- limited funds for new facilities; and
- the difficulty of attracting and keeping research staff.

Despite the enormous growth in enrollments, there has been no comparable increase in the number of the faculty members in these departments. Neither is the imbalance likely to correct itself. The growing rate at which high-technology products are propagated into our society argues for a continuing demand for electrical engineering and computer science graduates in the next decade, so that student demand for these courses of study is likely to remain high. Partly because electrical engineering and computer science departments have been unable to fill empty faculty slots, and partly to ensure a reasonable balance in enrollments within the engineering school, many universities have had to limit enrollments in those departments (to about one-third of total engineering enrollment in most cases). As a result, many qualified students wishing to major in this vital field must be turned away. This pressure of students wishing to major in computer science may be lessening, but it is offset by students in different fields who wish to acquire a deeper knowledge about computing.

At the same time, the attractiveness of opportunities in industry has reduced the number of students seeking the doctorate, as well as the number of new Ph.D.s interested in academic positions. This reduction has made it difficult to recruit the additional faculty needed to lighten the teaching and supervision load. The Institute of Electrical and Electronic Engineers (IEEE [1985]) reports that openings in this field now exceed several hundred, and many more new openings would certainly appear if the existing ones were filled. The situation has tended to be exacerbated further by the departure of mid-career faculty to industry. According to a recent Office of Technology Assessment (1985) study, the rate of outflow of faculty in computer science is twice that of any other engineering field. Clearly, many are leaving to join new entrepreneurial companies bringing the latest research advances to market in the form of high-tech products. (Recent downturns in the computer industry are probably stemming the outflow, however.)

The panel believes that TV- and computer-based instructional resources, used imaginatively, can reduce some of the pressure on faculty by providing an alternative way to present undergraduate engineering students with high-quality instruction. Educational technology could be particularly applicable in beginning courses with large enrollments. With or without the greater application of educational technology, there will still be a need for more teaching assistants to leverage the efforts of faculty in electrical engineering and computer science departments.

Industry/academia salary differentials have been a major contributor to the faculty shortage problem. Recognizing that fact, state legislatures and university administrators have made a determined effort to increase starting salaries for new faculty in these disciplines so that they might compare more favorably with starting salaries for Ph.D.s in industry. At some institutions, this corrective measure has created a serious compression of the salary structure—although this is by no means a universal problem. In general, electrical engineering/computer science faculty salaries are improving and are sometimes very competitive. Indeed, panel members from prestigious companies report that their organizations have, on occasion, found it difficult to match university offers to their high-level research employees. In addition, consulting substantially augments faculty income at all levels—especially in IC^3. Increasingly, the main problem is not industry/academia salary differentials for Ph.D.s, but simply the unattractiveness of pursuing a Ph.D. given the strong appeal to B.S. graduates of jobs in these fast-moving fields in industry.

Recommendations

University administrators should continue granting competitive salaries to faculties in electrical engineering and computer science departments . Attention must be paid to the salary structure of mid-career and senior faculty as well as to that of junior faculty.

A new program of faculty fellowships should be instituted by industry and/or the federal government. Such fellowships should not require the preparation of lengthy proposals or reports. They would give faculty members flexibility in their research that is not presently available. The Presidential Young Investigator Awards program is one example of this kind of support; more such support is needed, especially for more senior faculty members. These programs would be especially useful in the case of well-established researchers who wish to shift the focus of their research, perhaps to new areas of inquiry in which results would not be immediately produced. The number of fellowships available for this purpose would be small (a few percent of the total); but they would offer established researchers an opportunity to "break out of the mold."

With regard to educational technology, the panel supports the recommendation of the Committee on the Education and Utilization of the Engineer (CEUE [National Research Council, 1985])—namely, "These tools should be applied as rapidly and as fully as practicable in all academic programs in such a way as to enhance the quality of engineering education. Engineering schools should be encouraged to create programs for development of educational technology by faculty, with shared institutional, industry, and government funding."

Equipment and Facilities

Much attention has recently been paid by industry and government to the problem of obsolete research and teaching equipment in colleges and schools of engineering (especially in undergraduate labs). The cost to modernize this equipment has been estimated to be $1.2–$2 billion and growing (Haddad, 1983). The most severe problem is in those areas in which technology is advancing the fastest—that is, electrical engineering and computer science. Companies have been generously responsive to this problem. For example, in 1983 IBM and Digital Equipment Corporation together donated $50 million in equipment to MIT. In the same year, Hewlett-Packard donated some $22 million to universities, mostly in the form of equipment. Apple Computer, Inc., has donated more than $21 million in personal computers to schools at all levels. Other contributions have been made by Wang, IBM, NCR Corporation, and Honeywell.

Despite this assistance, however, the problem remains enormous; and it is a moving target. Adding to the situation is the fact that gifts of equipment do not involve funding for maintenance and other operating costs—which can greatly exceed a university's budget for overhead expenses of this type. Advanced electronic equipment, no matter how current and valuable, is useless if it cannot be operated and adequately maintained.

The state of buildings and laboratory space—so-called "bricks and mortar"—is a related problem for engineering departments generally. Because the federal government essentially eliminated support for construction of facilities in the 1960s, physical plants have deteriorated alarmingly. This problem is especially acute in IC^3, because the rapid changes in the field cause facilities to become obsolete very quickly.

Because the state of the art in IC^3 equipment changes so fast, universities face an additional problem relating to their cost accounting practices. For determining indirect costs, they use a different depreciation schedule from the accelerated depreciation currently used by industry. Equipment is depreciated over 16 2/3 years and capital equipment over such long periods, universities encounter a financial problem in the renewal of advanced equipment for research.

Recommendation

A long-term program for support of both equipment and facilities is urgently needed. The panel strongly supports the recommendation of the Committee on the Education and Utilization of the Engineer in this regard—that is, industry, academia, and the professional societies need to join forces in promoting legislation wherever necessary to facilitate gifts of laboratory equipment to colleges of engineering. In the special case of "bricks and mortar," the federal government and industry should be prepared to match those funds raised for this purpose by state governments or from philanthropic sources.

In addition, universities and the government should change their cost accounting practices to reflect the faster real depreciation of equipment in the rapidly changing IC^3 field. Depreciation should be over 5–7 years in the case of equipment, and 15–30 years in the case of buildings.

Cross-Disciplinary Research

The inclusion of research on IC^3 systems within the purview of a single panel suggests the heavily interdisciplinary nature of this field. Like other areas of engineering research that are currently acquiring great economic and technological importance, work in IC^3 systems cuts across traditional disciplinary boundaries. Yet the requirement for cross-disciplinary approaches to research and teaching runs counter to the established structures and practices of most university engineering departments, which have long emphasized specialization. By the same token, cross-disciplinary research is not easily encompassed within the traditional academic department structure or the reward system for university faculty. These problems impede the needed transition to new modes of research and practice in the nation's schools and industries.

Interdepartmental laboratories are a very useful organizational mechanism within universities for dealing with research problems that span several departments or disciplines (e.g., VLSI, robotics, manufacturing). One key to their success is the use of directors who are able to devote a substantial part of their time to management. Another great advantage is the presence of research staff members. The attractiveness of university life to these research staff members has been greatly reduced by many of the problems described earlier, in particular those of salaries and equipment. In addition, the status of these personnel within the university community is a matter for concern. Unlike the faculty members, they do not vote in university councils; nor are they a part of the academic policymaking process. Thus, they are to a great extent isolated from university life. As a result of these problems, they have recently been leaving universities in great numbers.

Recommendation

Universities must evaluate both their organizational and reward structures to permit the cross-disciplinary approach to flourish, in research as well as in teaching. In addition, university administrators must improve the salary structure for interdepartmental laboratory research staff and devise other mechanisms for integrating them into university affairs and otherwise improving their overall morale.

Human Resources: Adequacy of New Talent

The B.S. and M.S.

Students entering the disciplines associated with IC^3 include many of the very best students attending universities. Enrollment limitations in force at many institutions have raised the high-school grade point averages and Scholastic Aptitude Test scores of majors in electrical engineering and computer science to the highest levels in memory (Horgan, 1984). The number of graduates in these disciplines at the B.S. and M.S. levels is at an all-time high and apparently still growing, despite signs of a downturn in engineering enrollments generally. There are now some reports of declining demand for engineers—even for those in the information and computer fields, in which growth has been phenomenal for a number of years (e.g., *Inside R&D*, 1985; Office of Technology Assessment, 1985). However, the panel believes that although there may be fluctuations, demand for these graduates will continue to grow for the next 5–10 years. Model-based projections tend to confirm this expectation (see, for example, Vanski, 1984).

The Ph.D.

As we discussed earlier, a major reason for the current faculty shortage in this field is the shortage of new Ph.D.s. Although it has increased slightly in the past 2 years, the number of engineering doctorates is not substantially greater than it was in the late 1960s, and is, in fact, considerably below the level of the early 1970s. The number of Ph.D.s earned in computer science has remained level, at roughly 200–250 per year over the past decade, whereas the number in computer-related areas of electrical engineering, for example, has been no higher than that.

The quality of these doctorates is high; but their number is clearly insufficient—especially in "hot" areas such as artificial intelligence, CAD, robotics, VLSI, computer architecture, graphics, and computer systems. The need for doctorates in these fields will not abate in the next decade. As noted earlier, computers and electronics are permeating all aspects of life and work; IC^3 will continue to be research-intensive. In addition, whether the supply of B.S. and M.S. graduates comes into balance with demand or not, many more Ph.D.s will be needed to staff university faculties.

The number of Ph.D.s could be increased greatly if more women sought doctorates in engineering. More to the point, the overall *quality* of engineers and engineering education could be raised if more women participated, bringing a new source of highly talented people into practice and teaching. IC^3 does not seem to attract a large share of the women who do enter engineering, however, and most of those who are in IC^3 are involved in software. The difficulty seems to be traceable to the early grades, in which a difference can be seen between boys and girls in the relative appeal of mathematics, laboratory exercises, and even the use of computers. It is difficult to say what could be done to entice more women into advanced study in IC^3. Women involved in research in both academia and industry can help by actively communicating its excitement to their qualified female students and younger colleagues and by encouraging them to follow this path.

The panel notes that, among Ph.D. students currently studying at American universities, the proportion of foreign-born students on temporary visas to American-born students has risen sharply in recent years, to more than 40 percent (National Research Council, 1985). It must be said, however, that these foreign-born students have provided many of the new young faculty members who are in such short supply; some 25 percent of all junior faculty in engineering are reported to have taken the B.S. at foreign schools (Office of Technology Assessment, 1985).

Foreign-born graduates are also extremely valuable in U.S. industry—a point that is often overlooked. It is frequently found that a large proportion of the engineering employees in the most advanced areas of R&D are foreign born. Training them in this country is thus a good investment. This access to some of the rest of the world's best talent gives the United States an edge in international competition—an edge the Japanese, for example, do not have. To discourage these people from staying (or, even more so, from coming) would reverse that advantage.

The long-term health of IC^3 in the United States requires a substantial increase in the number of Ph.D.s who can stay in the country to enter academia and industry. Attitudes toward doctoral study *must* change. The leading B.S. and M.S. graduates must be able to weigh the advantages of a Ph.D. against the alternatives and decide that it is worthwhile to pursue a doctorate.

Recommendation

More students must be induced to pursue the Ph.D. *To that end, the panel recommends that more substantial fellowships be offered to American doctoral candidates, with a stipend equivalent to one-half the starting salary of an entry-level B.S. engineer in industry*. In particular, the panel commends initiatives by Hewlett-Packard and the American Electronic Association to award fellowships containing loans that are forgiven if the recipient remains in academia as a professor. Some quid pro quo in these fellowships might be useful; that is, requiring periods of work, reporting, or some other form of accountability in order to build a sense of responsibility in the recipients.

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APPENDIX RESPONSES TO THE ENGINEERING RESEARCH BOARD'S REQUEST FOR ASSISTANCE FROM UNIVERSITIES, PROFESSIONAL SOCIETIES, AND FEDERAL AGENCIES AND LABORATORIES

Requests for assistance were sent by the Engineering Research Board to a number of universities, recipients of Presidential Young Investigator awards, professional societies, and federal agencies and laboratories in order to obtain a broader view of engineering research opportunities, research policy needs, and the health of the research community. Some of the responses included comments on engineering research aspects of IC^3 systems research; these were reviewed by this panel to aid in its decision-making process. The panel found the responses to be most helpful and wishes that it were possible to individually thank all those who took the time to make their views known. Because that is not practical, we hope nevertheless that this small acknowledgment might convey our gratitude.

Responses on aspects of IC^3 systems research were received from individuals representing 53 different organizations, listed in Table A-1: 29 universities (including 11 represented by recipients of NSF Presidential Young Investigator Awards), 9 professional organizations, and 15 federal agencies or laboratories. Some comments covered specific aspects of the panel's scope of activities whereas others provided input on a variety of subjects.

Although most of the responses addressed priority research needs, several respondents did reflect on policy issues. Many of the research needs and issues of policy and health addressed by the respondents were similar to those noted by panel members. The broadened perspective provided by the responses to the survey was most beneficial in the panel's deliberations.

Table A-1 Organizations Responding to Information Requests Relevant to Information, Communication, Computation, and Control Systems Research

UNIVERSITIES
California Institute of Technology
Carnegie-Mellon University
Clarkson University
Lehigh University
Massachusetts Institute of Technology
North Carolina State University
Oregon State University
Oregon Graduate Center
Princeton University
Rensselaer Polytechnic Institute
Texas A&M University
University of Arizona
University of California, Berkeley
University of California, Davis
University of California, Los Angeles
University of Colorado
University of Georgia
University of Hawaii University of
Illinois-Urbana/Champaign
University of Iowa
University of Kansas
University of Maryland
University of Michigan
University of Oklahoma
University of Pennsylvania
University of Rochester
University of Texas at Austin
University of Utah
Washington University

PROFESSIONAL ORGANIZATIONS
Association for Computing Machinery
American Institute of Aeronautics and Astronautics
American Institute of Chemical Engineers
American Society of Civil Engineers
American Society of Mechanical Engineers
Industrial Research Institute
Institute of Electrical and Electronic Engineers, Inc.
Institute of Industrial Engineers
Society of Engineering Science, Inc.

AGENCIES AND LABORATORIES

Air Force Institute of Technology
Air Force Office of Scientific Research
Argonne National Laboratory
Army Research Office
Lawrence Livermore National Laboratory
NASA Ames Research Center
NASA Goddard Space Flight Center
NASA Jet Propulsion Laboratory
NASA Lewis Research Center
NASA Langley Research Center
National Center for Atmospheric Research
Naval Research Laboratory
Office of Naval Research
Oak Ridge National Laboratory
Sandia National Laboratory

Manufacturing Systems Research in the United States: An Overview

EXECUTIVE SUMMARY

Widespread agreement denotes that most manufacturing industries in the United States are in poor condition compared to their strongest international competitors. Many U.S. manufacturers suffer from growing cost and quality disadvantages in national and international markets. To a certain extent, these problems derive from international economic conditions outside the scope of this report. Nevertheless, this panel believes that large improvements in U.S. manufacturing competitiveness can be achieved through an increased awareness of the full scope and growing importance of manufacturing in the modern era, along with larger investments in related research and advanced education.

Major elements of modern production systems are people, machines, computers, and the communication links among them. An integrated systems engineering approach is essential to maximize product quality, production efficiency, and flexibility in future factories. Research is needed to provide better integration and compatibility of hardware, software, and people operating in the system. Related areas in which research is needed to support the goal of computer-integrated manufacturing (CIM) include modeling, simulation, control systems, and communication networks. In addition, more research is needed on certain unit processes in manufacturing, particularly various materials-forming processes and closed-loop process control.

In the past, government-supported research on manufacturing systems has been only a minor contributor to progress in this field. Recent federal initiatives via the National Science Foundation (NSF), including the establishment of two major Engineering Research Centers on aspects of manufacturing, are highly positive steps. We recommend that research on manufacturing systems be further strengthened through the establishment of one or more multilateral research consortia by industry, perhaps within the range defined by the Semiconductor Research Corporation (SCR) and the Microelectronics and Computer Technology Corporation (MCC). We expect to see research participation by universities, but do not anticipate major federal support for any such consortium.

The flow of new talent out of the U.S. educational system and into manufacturing is inadequate both quantitatively and qualitatively. Although changes are under way at U.S. universities and within U.S. companies that could remedy this situation, a real gap in orientation and educational quality still exists between typical product development engineers and their counterparts in manufacturing. Furthermore, most manufacturing engineers now in practice, and even many new graduates, are ill-prepared to address effectively the difficult problems to be faced on the path to true CIM systems. These weaknesses are barriers to the goal of achieving engineering integration throughout the manufacturing process from product concept through successful production. We recommend several specific actions to improve U.S. research and education in manufacturing engineering. These recommendations, set forth at the end of the report, address issues of education, technical standards, and professional engineering activity.

INTRODUCTION AND BACKGROUND

Discrete-product manufacturing in the modern sense of the term began with the industrial revolution.* For most of its first

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* This report focuses on discrete-product manufacturing, as distinct from continuous-flow processing, such as petroleum refining.

100 years, such manufacturing was not critically dependent on advanced scientific and engineering knowledge. Fundamental limits on materials and processes were seldom pressed. Often, materials and energy were used wastefully, and there was little concern with environmental effects. Manufacturing commonly comprised a sequence of unit processes through which material passed with very little accompanying flow of data. Engineers and workers understood their objectives largely in empirical terms, and managers usually were qualified more through experience than on the basis of scientific or engineering knowledge.

Over the past 20 years, most of those circumstances have changed. Since the advent of the digital computer as a tool of industry, manufacturing has become more information-intensive. The concept of factories as integrated systems under electronic control has emerged. The possibilities for flexible (programmable) automation and for real-time optimization have become clear. Great potential benefits are evident in product quality and cost, production efficiency, flexible response to changing needs, and improved health and safety conditions.

Many forces are at work to make manufacturing systems more complex in the future. Systems must be adaptable to product evolution. Such systems must also be capable of making the wide variety of products and variations that the customer demands, and of using the variety of raw materials available. Today the goal is integrated manufacturing systems that encompass the full scope of production activities from design through fabrication, assembly, testing, and delivery.

This goal is not being met either rapidly enough or effectively enough by U.S. manufacturing industries. Although the basic circumstances of manufacturing—the technology and the economic environment—have changed worldwide, U.S. manufacturing industries are finding it difficult to adapt to those changes. In many engineering fields (e.g., computers, aerospace design, communications), U.S. industry is noted for effective application of modern scientific and engineering knowledge. Unfortunately, this is not true in manufacturing. Nowhere near the full benefits of modern technology and knowledge have been realized in the U.S. manufacturing environment. In important product areas (e.g., steel, automobiles, consumer electronics), foreign competitors are the leaders in developing and exploiting advanced manufacturing capabilities.* Among the reasons for the United States' deficiency in this area are current policies and practices of industry, government, and universities.

This deficiency cannot be sustained. Manufacturing is highly significant for the nation's welfare. The U.S. manufacturing industry generates approximately 24 percent of the gross national product and about 65 percent of the tangible goods produced. The nation is unlikely to achieve full employment without a strong, internationally competitive manufacturing capability. Steadily improving productivity in the manufacturing sector is essential to our national economic well-being. Furthermore, public expectations for environmental quality can be achieved only by applying sophisticated controls to manufacturing plants.

This panel has evaluated the overall direction and strength of engineering research and educational programs that bear on U.S. capabilities in manufacturing. In this report we suggest improvements for existing programs and propose some new research-related activities involving industry, universities, and government. We also recommend actions intended to upgrade the practice and profession of manufacturing generally. Although there may not be complete agreement with some of the proposals made by the panel to strengthen the nation's industrial competitiveness, we have encountered widespread support for the need for more high-quality research and teaching in manufacturing in order to achieve this goal. Our proposals are intended to produce that result.

Research and education are inherently long-term activities. The panel has not attempted to describe the important shorter range planning, development, and implementation activities that must be undertaken by manufacturers seeking to improve their competitive position.

THE MANUFACTURING RESEARCH AGENDA

The panel believes that there are major deficiencies in research aimed at manufacturing technology. The underlying knowledge

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* See, for example, all of the reports listed in the Bibliography under the National Academy of Engineering, National Research Council, and Office of Technology Assessment.

base is still quite limited and highly empirical. Furthermore, the flow of knowledge from research to application needs strengthening. Reasons for these national deficiencies are described in later sections of this report; most can be traced to limitations in the training and education of personnel. In this section we address some examples of critical areas of manufacturing research. We found it useful to classify manufacturing technology in three categories, each of which needs increased research support.

Systems Integration

Systems integration is the most critical area. Basic understanding in this area falls far short of meeting the need for systematic, generic approaches to the design of CIM systems. Major programs of interdisciplinary research are needed to provide better engineering methods for systems integration.

Manufacturing systems are complex, and enormous volumes of data are required to describe and control them. In traditional factories the "data" used are embedded in the (fixed and variable) configurations of the machines, in drawings and other documents, and in the minds of the human operators. Data translations among these elements of manufacturing systems have been performed by humans, with corresponding limitations on the maximum amount of data and the speed and accuracy of its manipulation. Although some excellent manufacturing systems do not make intensive use of computers, these systems usually depend on long-term continuity and excellence of (human) engineering support. As manufacturing processes and end products become more complex, and as demands for flexibility increase, it is clear that computers will be an increasingly necessary element of integrated manufacturing systems. The term *computer-integrated manufacturing* implies that all relevant data axe available throughout a network of computers, so that they can be used as needed to achieve desired overall results for the complete production system. Improvements in product quality, manufacturing flexibility and efficiency, and human productivity will be achieved through more intensive use of information. Figure 1 illustrates the scope of CIM.

The benefits of CIM have already been amply demonstrated. Portions of this technology have been installed in several factories in this country and abroad, and have produced *reductions in:* tooling costs, parts inventories, control and scheduling problems,

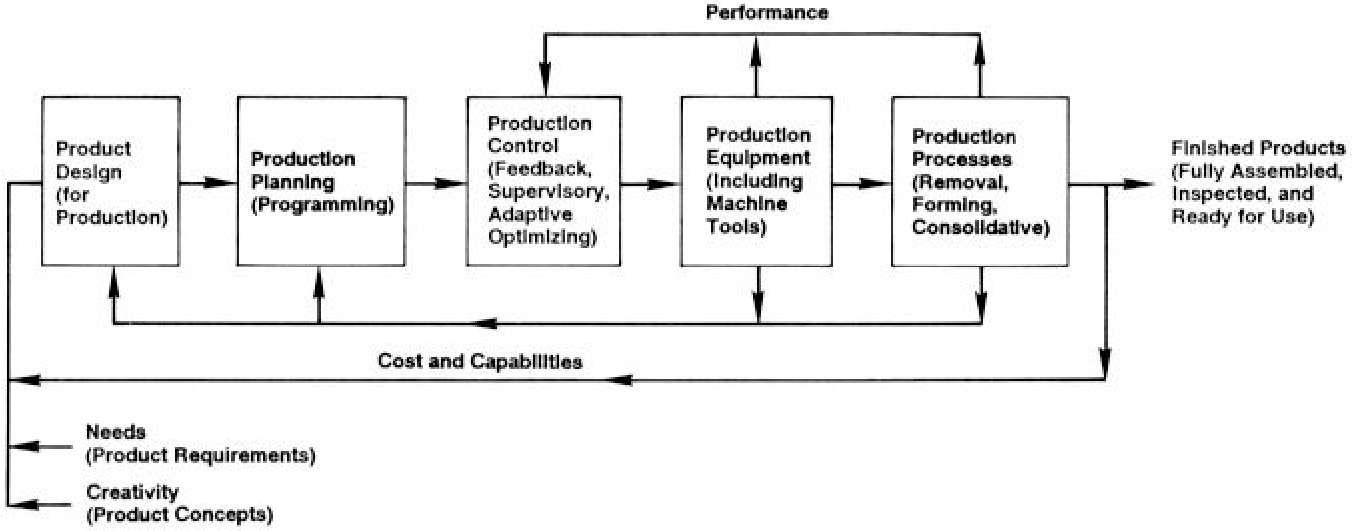

Figure 1
A computer-integrated manufacturing system (Office of Technology Assessment, 1984).

materials handling requirements, start-up and rework, engineering changes, defective work, and unit costs; along with *improvements in:* manufacturability, utilization, throughput, and quality.

CIM has proven difficult to implement. One reason is incompatibilities among data formats, hardware, and software of computers and production equipment made by different vendors. Generally, suppliers have tended to develop unique proprietary data formats, hardware, and software in order to induce customers to purchase all components from one source or to offer selective performance advantages.

Integration of the people and technologies of CIM is one of the most important (yet most difficult to plan and manage) aspects of automation. This problem extends from the factory floor through engineering, design, purchasing, marketing, finance, and management. Needed research in this area must involve a variety of engineering and nonengineering specialists. A system-level approach must be taken in designing elements of CIM. Research should be aimed at providing new hardware and software that is modular, compatible with other systems, adaptable to new requirements, and easily understood by the people who will use it.

The goal of CIM is to improve productivity and quality in production. Even when CIM is achieved, formal optimization (in the mathematical sense) of a complete production process probably will not be practical because of the large number of variables and many nonlinear, dynamic, and stochastic relationships. However, engineering strategies based on breaking down complex systems into hierarchies of simpler components have the potential to be practical and helpful. The hierarchy for manufacturing likely will consist of models and controls at several levels—for example, the individual machine level, the multimachine process cell level, the factory level, and the multiplant level.

A long-term goal is to develop the techniques of artificial intelligence to provide computer-based capabilities such as inference and intuition in ways that can be applied to manufacturing. Such developments would be of great value in achieving full automation and in optimizing the production process, and might also be exploited in training humans for their roles in design and production.

Modeling, Simulation, Control, Networks

Important areas still exist in which available models of materials, physical objects, and manufacturing processes are inadequate for the needs of CIM. Modeling of products, fully describing their geometric and other characteristics and functions, is a prerequisite for manufacturing systems integration. Current models for solid objects do not provide unambiguous geometric information, and they provide almost no information on nongeometric characteristics and functions.

Research is needed to define structured computer data bases for product modeling. Computer-aided design should generate the original data base for a desired product. The goal is to automatically derive, define, and verify the complete data base for planning, controlling, and implementing production and testing of a product directly from this data base. Expert systems techniques may be useful in resolving difficulties encountered in this process. Another long-term goal is to provide computer-aided design systems with automatic feedback of information about the production implications of design decisions. (This link is labeled "Cost and Capabilities" in Figure 1.) Such a capability has a strong potential for improving product producibility and quality and for reducing costs.

Continuing research is needed on *computer models for manufacturing processes*. Although some processes are quite well modeled today, in other cases existing models are inadequate to predict the sensitivity of final results to process variables. Models including dynamics are needed to understand and control such effects as tool chatter in metal cutting and precision pattern alignment in microelectronics.

Computer simulation techniques are still evolving rapidly, driven by advances in hardware, simulation software, and process models. High-quality graphics provided with relatively inexpensive workstations surely will be widely exploited. Such developments are being integrated into engineering curricula; graduates with simulation and graphics experience will be eager to exploit these tools.

Robotics and programmable automation have attracted the attention of many researchers interested in manufacturing automation. Both anthropomorphic robots, which are humanlike in some respects, and nonanthropomorphic robots are important. Physical and mechanical needs include improved sensors and actuators, such as tactile sensors and grippers with closed-loop control of gripping force. Computational and algorithmic problems in robotics, such as image processing, collision avoidance in a changing environment, and response to fault conditions also need intensive research. Product designs and material and information flows in manufacturing will need major changes to optimize the use of robotic and other flexible automation techniques. Research is needed to establish principles or engineering guidelines that can help determine the optimum degree of flexibility for a manufacturing system.

The man-machine interface must be greatly improved so that people involved in manufacturing can work effectively with vast information flows. Better understanding of human information processing is needed so that the essential information flow between people and machines is speeded up. Interactions must be improved by properly exploiting visual displays in color. Rapid access to large data bases is also needed. For example, production scheduling could be enhanced by computationally quick, dynamic network queueing models, with output at any design point provided via graphic animation. Expert systems techniques may well be helpful in support of interactive decision making; these techniques could embody practical knowledge and experience as well as strictly technical data and functions. Such techniques are necessary because future flexible manufacturing systems will be too complex for every eventuality to be deterministically programmed.

Multiprocessor computer system architectures for real-time control require further research. The data complexity, computational loads, and system reliability requirements of future CIM can be met only with multiprocessor systems. Today, most large real-time computer systems (e.g., those for airline reservations and telephone exchange control) are narrowly optimized to a limited set of functions. Future CIM systems will have diverse information flow and real-time response requirements. Computational functions likely will be organized in a hierarchy, and must permit growth to virtually unlimited size, capacity, and reliability. Surely there will be redundancy in both hardware and software.

Communication networks for the production environment are evolving rapidly. Simple, reliable, economical techniques are needed to link diverse systems for design, planning, fabrication, assembly, inspection, and testing. The Manufacturing Automation Protocol adopted by General Motors and many other manufacturers is a worthwhile step toward standardization in this area.

Unit Processes

Research on the individual unit processes that are combined to make up a complete manufacturing sequence has progressed well in many industries. Innovative improvements in materials and unit processes frequently result from the efforts of workers and the engineers who work closely with them.

Examples of fields in which more research is needed include processes for *forming composite materials, net shape forming through precision forging and powder metallurgy, superplastic forming and diffusion bonding, direct forming of rapidly solidifying materials, hot isostatic processing, and evaporative pattern coating*. More work is needed for many materials and products to provide an understanding of and models for the relationships among structure, processing, and final properties. In-line instrumentation and control of many unit processes can also be greatly improved.

Real-time closed-loop process control is becoming more feasible and attractive. The CIM environment makes it possible to adopt processing techniques that are more data-intensive than those commonly used in the past. Thus, in microelectronics manufacturing we see the replacement of open-loop processes such as wet chemical etching and thermal oxidation with closed-loop, electrically controlled processes such as plasma etching, plasma oxidation and anodization, sputtering, and ion milling.

ISSUES THAT DETERMINE THE HEALTH OF MANUFACTURING SYSTEMS RESEARCH

The previous section described some key system and process technologies that must be a part of CIM. Beyond the forces of technological change are new pressures from corporate managements and from society at large. These new demands stem from increased worldwide competition, higher standards for product quality, concern for environmental effects, and increased demand for customized products. Generally, product life cycles are shortening, leading to shorter planning time, higher ratios of fixed to variable costs, and smaller ratios of selling price to cost. For these reasons, manufacturing has become a larger factor in the strategic health of most businesses and of the national economy as a whole.

Manufacturing is changing from the low-risk periphery to the high-risk center stage; from the domain of the unskilled to that of the highly skilled; and from an arena in which skills were learned on the job to one in which advanced formal education is necessary. In the past, many manufacturing processes and products could be understood and evaluated with the unaided human senses; more and more frequently this is impossible. Any assessment of the health of manufacturing research and practice must begin with a recognition of these changes. U.S. manufacturers today are critically short of people who can perform well in the new environment.

Government Support of Manufacturing Systems Research

Government-supported research on manufacturing systems is only a small fraction of overall public expenditures for engineering research; in turn, engineering research allocations are only a small component of total federal research expenditures. Federal funding under the heading of "Programmable Automation" totaled about $82 million in FY84, with more than three-fourths of this money directed toward military programs. The relatively low level of federal research support is one of several serious problems faced by the field of manufacturing systems.

Most programs funded by the Department of Defense (DOD) focus on deliverable space and defense technology and as such have relatively short-term orientations. Although these programs are an important tool for cost and quality control in defense systems, they do not and cannot be expected to address the main problems of high-volume commercial manufacturing.

Civilian agency programs include those of the National Bureau of Standards (NBS), the National Aeronautics and Space Administration (NASA), and the NSF. The research program of the Center for Manufacturing Engineering at the NBS has the aim of developing soundly based standards for integrating heterogeneous hardware and software in manufacturing systems. It also aims to develop means and methods for assuring absolute dimensional reliability in discrete parts production. The work at NBS is of high quality and is directed at two important areas that industry has not effectively addressed.

NASA's automation research concentrates on robotic tools for use in space. The research program is small and focused on technologies that are sophisticated by commercial standards. There are occasional spin-offs to commercial manufacturing.

The NSF supports a Manufacturing Systems Research Program, and a number of other programs within the NSF support automation-related research to some degree. NSF-funded research is conducted at universities and covers both unit processes and systems integration. However, these programs are too small to have a major impact. One very positive development is that two of the new NSF Engineering Research Centers will focus on aspects of manufacturing systems.

Problems in Manufacturing Engineering

Several difficult problems contribute to the slow progress of industry toward CIM systems. Cause-and-effect relationships are unclear; but certainly the problems are strongly interrelated.

A limited professional tradition in manufacturing engineering is at the root of several of the problems we shall enumerate. Many career people in manufacturing lack the scientific and technological knowledge base, as well as the university and professional society contacts outside their own organizations, that are important stimuli to progress in fields such as aeronautics and electronics. Publications and conferences of the Society of Manufacturing Engineers are strong in their trade components and in traditional elements of manufacturing, but have not been highly effective in promoting research, in stimulating a systems approach to manufacturing, or in forging links between theory and practice.

The limited scientific base on which manufacturing engineering is built is a related factor. Other important engineering fields, such as structures, aeronautics, and computers, have generated and then exploited fundamental new knowledge in engineering methods. These fields are guided by growing bodies of scientific principles and engineering techniques that are widely understood, taught, and practiced. The relative absence of such patterns for manufacturing systems reinforces the view that this field is based largely on ad hoc empirical methods rather than on a systematic foundation of scientific and engineering principles. To be sure, the empirical knowledge base is important, as it consists of rules discovered through long interaction with materials, processes, tools, people, and systems. This "rule-based" practical knowledge must not be overlooked as fundamental principles are developed and applied.

The educational level is traditionally low in manufacturing. The field has little status in the engineering community; new college graduates avoid manufacturing positions. Manufacturing managers frequently are craftsmen promoted up from the shop floor. Formal education has been less necessary for advancement in factory management than in other parts of most companies. Managers from such backgrounds must work hard to lead the needed transition to a computer-based systems approach to manufacturing. Many more well-educated process specialists and computer engineers will be needed to design and operate the factories of the future.

Manufacturing engineers have limited opportunity to build on the prior work of others. Product development engineers have the opportunity to study the best competing automobiles or computers. Manufacturing engineers have much more difficulty studying their competitors' best factories. Understandably, managements often restrict the flow of information concerning proprietary manufacturing methods. As a consequence, manufacturing techniques often vary considerably among firms producing similar end products, and there is no arena in which the merits of alternative techniques may be subjected to detailed comparisons.

Manufacturing engineering lacks centers of focused research activity akin to the research laboratories of major firms in the communications, computer, and chemical industries, and the national centers for research in high-energy physics and materials science. Detailed reporting of the best work via professional conferences and high-quality archival publications has only recently begun. In light of all the abovementioned circumstances, it is easy to understand why research on manufacturing is limited in quantity, badly fragmented, and duplicative. Urgently needed generic materials handling and fabrication techniques, flexible manufacturing tools, and information systems standards are appearing much too slowly under the present circumstances.

Costs of labor and capital have for many years been higher for U.S. manufacturers than for their strongest international competitors. Some well-known attempts at factory automation in the United States failed because of inadequate flexibility in accommodating to changes in processes and products. Further, there has been a continuous shortage of engineers qualified to face the challenges implied by computer-integrated factory automation. These facts have influenced many U.S. manufacturers, particularly in fast-changing fields such as electronics, to choose labor-intensive manufacturing in low-wage areas of the world over automated manufacturing in the United States. Such strategies may well maximize the short-run return on investment. However, they deflect management's attention from the R&D efforts required to implement modern automated factories for long-term competitiveness. Implementation of systems such as CIM demands long-term management support. Success in such efforts will require that managers resist the pressure to achieve short-term returns, and that they become personally committed to making the integration process work.

Universities have difficulty mounting interdisciplinary efforts needed in manufacturing systems research. Engineering at universities took a strong turn toward quantitative, science-based instruction and research in the 1960s. Research projects have tended to focus on narrow specialty areas in which significant progress might be achieved with the limited resources available to engineering faculty members. Promotions are awarded on the basis of individual achievement; team research (as is needed for interdisciplinary fields) has been risky for faculty careers. This problem can be overcome if university administrators devote additional effort to the evaluation of individual contributions to large group efforts. Such differential assessments are routinely made with success in good industrial research laboratories. Credit must be given to all involved when "the whole is greater than the sum of its parts."

Manufacturing plants are very expensive to build and very complicated to operate. Small plants cost many millions of dollars and large plants cost many billions. To do meaningful research on the whole manufacturing system requires access to such an operating system, which at this writing can be found only in industry. Thus, university-industry cooperation is vital for the conduct of broad-based, interdisciplinary research relevant to the needs of U.S. manufacturing.

Formation of Research Consortia: A High-Priority Need

The United States needs sharply increased R&D activity to speed progress in manufacturing systems engineering. A major objective should be to accelerate the effective application of computer technology in order to improve the productivity of U.S. manufacturing and the quality of its output.

To achieve this accelerated progress, we believe it is crucial that manufacturers take the initiative in stimulating the formation and strengthening of consortia for research on manufacturing systems. Participants in any such consortium should include numerous industrial firms, and may in addition include universities and federal or state governments. One such consortium is Computer Aided Manufacturing International (CAM-I), based in Arlington, Texas. CAM-I has 150 member companies, but has not been supported at a level adequate to meet all the needs in this field. Examples of strong consortia in other fields are the SRC and the MCC, both of which were formed within the past 3 years. The initiatives for MCC and SRC came from the top management of major corporations in the relevant industries. Actions by Congress and the Justice Department have removed antitrust barriers to the operation of SRC and MCC. SRC supports research and graduate education at universities, with an annual budget of about $20 million. MCC conducts research programs primarily with its own facilities and staff, joined by staff members from participating companies. Its annual budget is about $50 million. The activities of SRC and MCC are complementary, and a number of firms belong to both.

ASSESSMENT OF THE ADEQUACY OF NEW TALENT

The flow of new engineering talent from the U.S. educational system into manufacturing is inadequate. In reaching this conclusion, the Panel on Manufacturing Systems Research has had access to survey questionnaires; information from universities, technical societies, and other sources such as the Presidential Young Investigators; and comments from several major corporations. Each input points to shortages of trained American manpower and, depending on the sector, some particular needs.

Understandably, because no one sector deals with an overall problem, no one respondent offers overall solutions; all of them quite properly address specific inadequacies with respect to their own needs. The panel was able to find no positive opinions to offset the prevailing view that there is a shortage of manufacturing engineering manpower.

The panel believes that the engineering capability of the U.S. manufacturing sector started to slip in the late 1960s. The universities' output of manufacturing engineers was declining and industry continued to fill key positions with personnel "who know machinery," such as skilled tradesman. Graduates of 2-and 4-year institutes of technology provided another attractive option. This led to gaps in position, salary grade, and professional status (obviously coupled to educational level), and eventually to a cultural difference between degreed engineers who work on product-related items and the few who work in the processes of manufacturing. This gap is real and fundamental to the problem of carrying through total engineering programs, spanning the range from product concept through successful manufacturing.

Up-and-down economic cycles and a general focus on short-term corporate financial performance have stifled many attempts at change. There are indications, however, that industrial corporations have now awakened, as evidenced by:

- new company-sponsored ways to upgrade engineering personnel in manufacturing by additional training and rewards;
- attempts by companies to build manufacturing and engineering teams to span the gap between product development and process design; and
- growing interactions between industry and academia to influence curricula, to foster cooperative work-study arrangements as a part of degree programs, and to support and cooperate in teaching and research.

At the same time, universities have been trying to change on their own over the last 12 years, spurred perhaps partly by need, but also by the realization that there is indeed a manufacturing systems discipline. However, there are at present only three programs offering degrees in manufacturing engineering that are accredited by the Accreditation Board for Engineering and Technology. Others are hidden under different names or folded into more traditional engineering areas like mechanical engineering. Our feeling is that such nonspecific efforts will not achieve the gains realized in Japan or working in Europe. We applaud these present efforts, but conclude that they should be more specifically supported.

RECOMMENDATIONS

The panel believes that a strong, internationally competitive manufacturing component is essential to U.S. domestic and international strength. We also believe that there is now a consensus of opinion in industry, government, and universities favoring sharply stepped-up research and education on manufacturing.

Thus, our highest-priority recommendation is that:

- Leaders of large U.S. manufacturers should take the initiative in stimulating the formation and strengthening of consortia for research on manufacturing systems. Participants in any such consortium should include numerous industrial firms, and may in addition include universities and federal or state governments.

We also recommend vigorous actions as follows to improve professional standards in manufacturing engineering:

- More specialist workshops, larger and better supported conferences, more refereed professional-level publications, and strong management support for the participation of its engineers in these activities are needed to place manufacturing engineering more nearly on par with the strongest engineering fields. Academic and industrial leaders must work together through professional societies to achieve these goals.
- The newly established program of Engineering Research Centers, fostered by an initiative of the NSF, provides an excellent vehicle for university-industry collaboration in research and teaching on manufacturing. Sustained personal and corporate commitments will be essential to the success of these centers.

- The 1984 IBM initiative to upgrade university programs on manufacturing engineering is an example of a highly constructive unilateral corporate initiative. We urge other firms to consider similiar initiatives in their fields of interest.
- The dearth of technical standards for mechanical and electrical interfaces and for data transfer has been a severe impediment to progress in manufacturing systems engineering. The General Motors Manufacturing Automation Protocol is a standard now endorsed by many; it must be recognized as an important step forward. For the long term, standards should be established with leadership from professional groups and governments as well as from individual firms.

The following actions should be taken by universities and their industrial supporters to improve the state of manufacturing engineering education:

- Continued support should be developed and planned for university programs that specialize in coupled manufacturing education and research.
- Programs should be defined in which the student can gain a more realistic appreciation of manufacturing applications and the manufacturing systems disciplines. Cooperative work-study programs for engineering students are one good example.
- Centers for Manufacturing Excellence should be established to deal with unit processes such as welding, casting, and flexible machining, and with computer-integration techniques. These centers could be established via mechanisms that encourage the state governments (financially) to engage local university-industry teams, such as in the Industrial Technology Institute program in Michigan.
- Faculty development programs should be established consisting of (reciprocal) personnel exchanges, joint proposals, doctoral fellowships, grants for research initiation, and Ph.D. thesis projects. A good example of an effort that has evolved over a period of 5 or 6 years into a program like this can be seen at the University of Wisconsin, where an M.S. degree in Manufacturing Systems Engineering has been established. (This program was stimulated by a grant from IBM.) The University of Wisconsin's program is a successful model that might well be replicated elsewhere.

- A professional group should be developed that would establish prestigious awards for contributions in manufacturing, provide matching funds for possible faculty winners of Presidential Young Investigator Awards, and provide seed money awards for course and case study development in manufacturing systems.
- Industrial support should be encouraged for university research and graduate education on manufacturing by establishing additional state and federal government programs, such as California's MICRO (Microelectronics and Computer Research Opportunities) program. California's program provides for state matching of private grants to universities in support of research agreed to by faculty and counterpart researchers in industry.
- An effort should be initiated to develop modern, high-quality text material suitable for undergraduate engineering instruction on manufacturing systems integration. Today such texts are in critically short supply. A parallel situation existed in 1960 with respect to transistor electronics. At that time the Ford Foundation and the NSF established and funded the Semiconductor Electronics Education Committee. This group wrote a set of short, high-quality teaching texts that filled this critical need. A similar initiative is needed now to modernize manufacturing engineering curricula.

We believe there are enough forward-thinking members in the American university system that an almost endless list of suggestions like the foregoing could be generated. It would make sense to propose such actions only if the academic community could be certain that someone would listen and act on them. Increased support of university programs by state and federal governments would certainly be applauded, and might well be matched, by the industrial sector.

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APPENDIX RESPONSES TO THE ENGINEERING RESEARCH BOARD'S REQUEST FOR ASSISTANCE FROM UNIVERSITIES, PROFESSIONAL SOCIETIES, AND FEDERAL AGENCIES AND LABORATORIES

Requests for assistance were sent by the Engineering Research Board to a number of universities, recipients of Presidential Young Investigator Awards, professional societies, and federal agencies and laboratories in order to obtain a broader view of engineering research opportunities, research policy needs, and the health of the research community. Some of the responses included comments on engineering research aspects of manufacturing; these were reviewed by this panel to aid in its decision-making process. The panel found the responses to be most helpful and wishes that it were possible to individually thank all those who took the time to make their views known. Because that is not practical, we hope nevertheless that this small acknowledgment might convey our gratitude.

Responses on aspects of manufacturing were received from individuals representing 45 different organizations, listed in Table A: 26 universities (including 9 represented by recipients of NSF Presidential Young Investigator Awards), 10 professional organizations, and 9 federal agencies or laboratories. Some comments covered specific aspects of the panel's scope of activities, whereas others provided input on a variety of subjects.

Table A-1 Organizations Responding to Information Requests Relevant to Manufacturing Systems Research

UNIVERSITIES

- Brigham Young University
- Carnegie-Mellon University
- Clarkson University
- Drexel University
- Duke University
- Lehigh University
- Massachusetts Institute of Technology
- North Carolina State University
- Northwestern University
- Princeton University
- Purdue University Rensselaer Polytechnic Institute
- Texas A&M University
- University of Connecticut
- University of California, Davis
- University of California, Los Angeles
- University of Illinois—Urbana/Champaign
- University of Iowa
- University of Kansas
- University of Maryland
- University of Michigan
- University of Minnesota
- University of Oklahoma
- University of Pennsylvania
- University of Rochester
- University of Utah

PROFESSIONAL ORGANIZATIONS

- American Institute of Aeronautics and Astronautics
- American Institute of Chemical Engineers
- American Society of Civil Engineers
- American Society of Mechanical Engineers
- The Institute of Electrical and Electronics Engineers, Inc.
- Institute of Industrial Engineers
- Industrial Research Institute
- Society of Engineering Science, Inc.
- Society of Manufacturing Engineers
- Society of Naval Architects and Marine Engineers

AGENCIES AND LABORATORIES

- Air Force Institute of Technology
- Air Force Office of Scientific Research
- Army Materials and Mechanical Research Center
- Army Research Office
- Lawrence Livermore National Laboratory
- NASA Goddard Space Flight Center
- NASA Langley Research Center
- Oak Ridge National Laboratory
- Sandia National Laboratories

Materials Systems Research in the United States: An Overview

EXECUTIVE SUMMARY

Leadership in the materials field is essential to compete successfully in areas of both conventional and high technology. Thus, the economic well-being and security of the United States require that actions be taken to ensure world leadership in the development of materials systems that can meet the demands of a technology-intensive future.

This report describes the results of a study conducted by the Engineering Research Board's Panel on Materials Systems Research.* The study encompassed identification of the important classes of materials, emerging areas for future research, and the national climate and infrastructure governing the conduct of materials systems research.

In the United States the science of materials has developed into a recognized discipline with its own group of practitioners, facilities, societies, meetings, and publications. Large amounts of money are spent on basic scientific research in the materials field.

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* We use the term "materials systems" to denote advanced materials requiring highly tailored design and specialized processing. "Materials systems research" refers to the closely interlinked science and engineering research that underlies the development of these complex systems.

The result is a greatly increased understanding of the relationship between the structure and properties of materials. However, we have failed to place a comparable emphasis on research directed at the application of this knowledge. Because of the economic importance of those applications, that shortcoming has placed the nation at considerable risk. Other countries have spent comparatively little on expensive basic scientific research, and have concentrated their effort instead on the relatively inexpensive engineering research needed to bridge the gap between pure science and applications. Thus, they have been able to capitalize economically on the scientific knowledge for which we have paid a high price. By comparison, our ability to put fundamental knowledge to work in order to devise optimal designs for the processing of materials remains seriously inadequate. We are suffering the consequences in terms of declining international competitiveness not only in the mineral, metal, automobile, and textile industries, but now across an even broader spectrum of manufacturing.

If this trend is to be reversed, *we must make at least as much use of our basic materials science as others do*. That will require a focused and interdisciplinary effort on materials systems research comparable to the investments made earlier in materials science. We must attend not only to specific materials with special characteristics important for our economic well-being and for our national defense, but also to activities important to more than one specific material. Thus, among the conclusions reached in the report the following seven are particularly important:

1. We must increase the emphasis on engineering research in:

- advanced ceramics;
- semiconducting materials;
- magnetic materials;
- polymers;
- high-performance composites;
- performance-driven metallic materials; and
- biomaterials.
- 2. There must be a recognition, through special research programs, of the importance of the following subjects that are generic to materials research as a whole:

- processing;
- durability and lifetime prediction;
- tribology (friction, lubrication, and wear);

- computer modeling; and
- materials property data base.
- 3. Research on materials systems—especially research directed at the engineering application of basic knowledge and the processing of materials—is seriously underfunded relative to the opportunities and potential benefits that current basic knowledge presents. Engineering research on problems in this area would produce knowledge of immediate economic importance, but its potential cannot be adequately tapped without substantially greater federal funding for such research.
- 4. Expenditures on the order of $150 million are required for materials processing research facilities of various kinds. This amount could purchase roughly a dozen each of facilities for research on composites processing, semiconductor processing, and molecular beam epitaxy or organometallic chemical vapor deposition.
- 5. Individual-investigator, single-discipline research must continue to be nourished even as we provide a better climate and facilities for multidisciplinary approaches to materials systems research.
- 6. The stability of federal agency funding policies is vital if we are to obtain the maximum benefit from the modest support for fundamental engineering research and if we are to ensure the needed continuity of valuable research programs.

INTRODUCTION AND BACKGROUND

Introduction

The Need

As a result of dramatic technological advancements in many fields over the past two decades, the discovery and development of new materials has become a national imperative—not only for the United States, but also for our major foreign competitors and adversaries. To give just one example, the United States is now engaged in fierce international competition to maintain our supremacy in electronics, the loss of which to the Japanese or others might be a more serious blow to national productivity and security than were the earlier setbacks in the automobile and steel industries. Electronic materials are a key element in this competition. Without the continued development of a leading position in semiconductor materials and fabrication technology, the United States will not even have the opportunity to compete in the information and computer fields (see, for example: General Accounting Office, 1985).

Materials are equally fundamental to many other commercial areas in which our economic fortunes are at stake. They constitute a "gateway" technology; that is, advances in materials open up a cascade of new possibilities in other technologies. A few examples from the past, such as plastics, semiconductors, and the whole range of useful metal alloys, suggest how sweeping the impact of new materials can be.

Scope of the Report

Materials systems research involves the development of new materials and the improvement of existing materials for application in a wide range of advanced engineering systems. The term "materials systems" recognizes the complex interrelationships among structure, properties, processing, performance, and reliability when viewed in the context of an engineering application. It encompasses the activities of materials science and engineering, which are "concerned with the generation and application of knowledge relating the composition, structure, and processing of materials to their properties and uses" (National Research Council, 1974).

In examining the engineering research opportunities and issues associated with materials systems, the panel addressed topics related to the availability, application, and fundamental understanding of present and future engineering materials such as metals, ceramics, polymers, composites, semiconductors, magnetic and biomaterials. In addition, the panel addressed questions important to the overall health of the materials field, including policy, research funding, and human resources issues.

Background

At the outset, it is important to emphasize that the field of materials science and engineering has undergone a fundamental and dramatic change over the past two decades. As characterized by William Baker and Morris Cohen in the 1974 COSMAT report of the National Academy of Sciences, the central element of this change has been the emergence of the science of the solid state as a field of major importance in the latter half of the twentieth century. Also contributing to this change has been the joining of technologies from the ancient fields of metallurgy and ceramics with the more recent fields of synthetic polymers (e.g., rubbers, plastics, and fibers) and modified bioorganic substances. In order to stimulate the exchange of ideas among experts in these fields and to accelerate the development of new materials, the Defense Advanced Research Projects Agency (DARPA) of the Department of Defense (DOD) established several interdisciplinary laboratories in materials sciences and engineering at major American universities over two decades ago. These Materials Research Laboratories, with the continuing support of the National Science Foundation (NSF), have had a significant influence in unifying the subfields of materials science at universities over the past 20 years.

Perhaps the most dramatic change has been a stronger systems orientation in the materials research community at large. Instead of the formerly dominant focus on structure-property relationships (i.e., understanding how structure controls specific material properties), a greater integration of design, performance, and processing requirements with materials properties and microstructures has occurred. As a result, a more rational tailoring of final microstructures to specific applications has been achieved. Materials themselves can now be designed (within limits) to fit the desired use. A dramatic example is seen in large-scale integrated circuits, in which, in addition to the circuit elements, materials structures and transport properties are intricately integrated. The ability to tailor applications to the properties of new materials has also given new degrees of freedom to the product designer —"direct replacement" is not the only option.

In commercializing the new developments in materials, a higher order of functional integration, involving still more disciplinary groupings, has been necessary. New requirements for materials performance and reliability have emerged not only from society's gradual shift from a consumption to a conservation orientation, but also from world competitive forces that increasingly place high market premiums on quality, reliability, and low operating and maintenance costs over the product's expected lifetime.

The additional requirements for greater productivity, manufacturability, and profitability have demanded a broader technology base and detailed attention to a number of new technical considerations. For example, productivity gains and cost advantages have been achieved by some American manufacturers by tailoring their processes to an optimal equipment design rather than by insisting on customized equipment to accommodate an optimal process design. Advances in quantitative nondestructive evaluation have not only facilitated in-process inspection but have also sometimes led to viable rework and recycling strategies for high-value materials and components.

These advances have been further spurred by the advent of computer-aided design and manufacturing, which have presented the design engineer with new flexibilities for patterns of materials use, as well as opportunities for design optimization based on new materials or improvements of existing materials. Artificial intelligence-based systems are emerging to improve the quality of decision modeling and to reduce human error in production, inspection, and testing activities.

New and improved materials have a vital effect on the nation's welfare. The ability to maintain a competitive edge in most engineering technologies depends on the development of appropriate materials. Ceramics and coatings for use in adiabatic diesel engines, advanced gas turbines, Stirling engines, and fuel cells are pacing the rate of development of those systems; international competition is fierce (see, for example: U.S. Department of Commerce, 1984; National Research Council, 1986a). Composite materials are the key to developing lighter, stronger airframes and ground vehicles. Near net-shape processing combined with advanced nondestructive evaluation methods and automated process control are leading to cheaper and more reliable structures. New materials can contribute (along with construction design and codes) to upgrading the nation's infrastructure of roads, bridges, railroads, and buildings through their characteristics of high strength, low weight, and resistance to harsh environments.

Emerging requirements in manufacturing, computation, energy development, and defense are all based on the greater complexity of technologies; these higher requirements will not be realized without new materials. Therefore, it is essential to the nation's general welfare that a strong national materials technology base be maintained. In addition, the bright young talent and scientific discoveries needed to advance this technology base must be nurtured through support of university education and research in materials science and engineering.

ESPECIALLY IMPORTANT OR EMERGING AREAS OF MATERIALS SYSTEMS RESEARCH

Rationale for Selection

Out of the enormous range of possibilities that materials systems research represents, it seems almost presumptuous to identify a few "especially important or emerging" research areas. It is the very pervasiveness of materials throughout a large fraction of engineering that has caused the field of materials research to be fragmented, duplicative, and difficult to codify. Despite these impediments, we believe that the exercise of identifying important areas for future research can actually have a unifying result, as we shall point out next.

Out of the large number of valid candidates for research emphasis, we have selected those that appear particularly promising in terms of three criteria: (1) a present or perceived future market for the ultimate products of that research; (2) a current state of the underlying science such that an advance of appropriate magnitude appears realistic; and (3) a sense of national priority for the product. Given those criteria, we can list the high-priority areas that, in our opinion, are especially well positioned for immediate and intensive engineering research.

Emerging Research Areas

Seven classes of materials that lead to specific end uses need the focused attention of the engineering research community. A failure to focus that attention will prevent the nation from realizing the potential for commercialization of products emanating from a science base in which it has long held a position at or near the forefront.

With no ranking implied, the seven product-specific materials research areas are

1. advanced ceramics;
2. semiconducting materials;
3. magnetic materials;
4. polymers;
5. high-performance composites;
6. performance-driven metallic materials; and
7. biomaterials.

The panel strongly believes that there should be a greater unification of effort in materials systems research. This integration can be accomplished by recognizing that it is wasteful and dangerous to neglect areas of research that transcend specific product or application lines. These five "generic" research areas, ranked in order of importance for progress in materials, are

1. processing (including sensor research);
2. durability and lifetime prediction;
3. tribology;
4. computer modeling; and
5. materials property database.

The remainder of this section is devoted to a discussion of these research areas.

Material for Specific End Uses

Advanced Ceramics

Advanced ceramics are a rapidly evolving class of materials whose usefulness is based on their ability to fill both functional and structural roles. They can be tailored to have combinations of electrical, mechanical, and chemical properties that make them essential and irreplaceable in many new engineering applications. Functional roles include (1) semiconductor device components such as coupling capacitors and substrates, optical communication waveguides, optoelectronic modulators and demodulators, magnetic components, and sensors and transducers; and (2) uses in chemical processes for separations and catalysis. Structural roles include service as load-carrying and wear-resistant components, especially in circumstances requiring corrosion resistance and high-temperature service.

Ceramic materials (like other advanced materials) are a "gateway" technology; rather than being an end in themselves, they open up new potentials in other technologies, thereby achieving high degrees of leverage. The competitive performance of many devices and large systems depends on ceramic components that may make up only a small but vital part of the total. The fact that many ceramics involve nonstrategic or nonimported materials is another factor in their attractiveness.

Electronic ceramics are a large family of crystalline and glassy materials serving as dielectric, semiconducting, magnetic, and optical materials and components. Current limitations in both the performance of devices and the growth of the field as a whole are in many cases due to insufficient research on the actual ceramic materials or components.

The impetus for the development of *structural ceramics* and ceramic composites has been the promise of higher efficiencies through the use of ceramics in heat engines. There are, however, many other applications, both military and civilian, for structural ceramics that take advantage of their good specific strength (strength-to-density ratio), specific stiffness, hardness, and wear and corrosion resistance. Ceramics present special requirements for successful applications under high stress because they characteristically undergo brittle failure. Extreme control of processing to minimize flaw size is required, which presents a special challenge for large parts and complex shapes.

Areas of advanced ceramics more closely allied to process technology than to ceramics per se may play additional functional roles. For example, improved control over pore structure has opened up new routes for selective catalysis, as well as novel separations based on selective adsorption. New classes of zeolites offer promise as shape-selective catalysts for reactions heretofore limited to a small scale with homogeneous catalysts or biological systems. One example is a new catalyst used for selective isomerization of xylenes. In addition, the combination of ceramic materials with organometallic complexes is leading to a new class of catalysts with the selectivity of homogeneous catalysts and the robustness of heterogeneous catalysts.

Despite the great diversity of applications for ceramics, a few common technical themes stand out. In almost all cases one is dealing with the behavior of materials near interfaces. Thus one may be concerned with thin-film coatings to provide proper wear resistance for ceramics, or with the behavior of coatings and layer structures that in turn determine the behavior of ceramics for functional applications (e.g., electronics or catalysis) and other aspects of surface modification. Whenever one is faced with joining ceramics, the preparation and properties of surface layers are, of course, critical. These questions of interface behavior may apply either at a near-atomic level (e.g., in assemblies of components for microcircuitry) or on the bulk scale (e.g., in the manufacture of large monoliths).

There is an urgent need to learn how to optimize collections of properties simultaneously rather than a single property at a time. Because of the dependence of all of these properties on the fundamental constitution of a given material, there is also the need to develop predictive methods that are rooted in a basic understanding of material behavior.

Ceramic processing is in a state of rapid change. This change is due to improvements in the predominant technology based on powder processing and to new chemical routes that combine or bypass some of the traditional steps. Ceramic processing increasingly needs to be studied as a system extending from the raw material to the finished product, with product design requirements taken into account by the processor and processing limitations taken into account by the designer.

Semiconducting Materials

Over the past three decades, no single field of science or engineering has had a greater impact on the national productivity and quality of life in the United States than has semiconductor microelectronics. Semiconductors have revolutionized the communication, entertainment, and transportation industries, and created the computer industry. Further advances that will have profound consequences are possible through the development of optoelectronics and lightwave technology.

There must be an increased emphasis on materials for use in advanced generations of computers, optical storage elements, and high data rate transmission and processing, because these new materials may be critical for specialized applications involving high-speed processing, optoelectronics, and resistance to radiation.

The following discussion summarizes briefly some of the most important areas of research on semiconducting materials.

Materials for Optoelectronic and Microwave Devices

Materials for these uses, such as the binary compounds gallium arsenide (GaAs) and indium phosphide (InP) must be improved in conducting and semi-insulating forms, and grown in large (at least 3-in. diameter), single-crystal ingots. Specific research areas needing emphasis are

- Improved growth methods need to be developed for semi-insulating GaAs and InP ingots. This effort will require better modeling of the growth techniques.
- A determination of the necessary technology for producing ultra-flat polished GaAs wafers should be pursued. All aspects of wafer preparation should be included. Modern structural/analytical techniques should be extensively used to assess wafer quality.

Compound Semiconductor Heterostructures

This is one of the most promising new materials technologies that has emerged in recent years. It offers a high probability for truly revolutionary discoveries. In the area of optoelectronics, for example, III-V quantum-well heterostructures and superlattices promise to have a profound effect. With the advent of advanced crystal growth techniques such as molecular beam epitaxy and organometallic chemical vapor deposition, radically new structures have become available, with profound device applications. For the first time, crystals can be synthesized layer by layer to yield entirely new compositions of matter. Such structures, typified by superlattices, metastable films, and complex multimaterial/multilayer sandwiches, are frequently called artificially structured materials. In many cases these structures possess unusual properties that offer promise for future electronic and optoelectronic devices. Considerable research in crystal growth, characterization of layers and interfaces, carrier transport, and processing methods will be required.

Silicon

Silicon is still the basic, largest-volume semiconductor material. Despite our considerable experience with it, many problem areas still need research attention. In addition to improving the art of crystalline growth generally, there are also more specialized needs. For example, high-purity, floating-zone silicon serves a segment of the device market in which compositional purity and crystalline perfection are prime requirements. The important research areas include

- *growth of large, uniform crystalline ingots*—to provide the silicon wafers needed in integrated circuit manufacturing;
- *gettering techniques*—for isolating and immobilizing impurities. Most gettering techniques now rely on high-temperature processing for activation of the gettering mechanism. Yet as circuit densities continue to increase, lower temperature processing will be inevitable;
- *deposition and characterization techniques*—for fabrication of thin films and interfaces; and
- *doping techniques*—including diffusion and ion implantation (e.g., through the use of focused ion beams).

Small Geometry Effects

These effects are important in semiconductors, conductors, and insulators. As linewidths shrink to submicron geometries, a host of materials problems emerge. Much of the carrier transport occurs at high fields, and traditional device structures must be modified or abandoned as field gradients increase. Research in fine-line lithography and dry etching, surface and interface effects, hot carrier transport, new conductor and contact materials, and multilayer growth of silicon and GaAs with interspersed insulators will be required.

Magnetic Materials

Magnetic materials are an integral part of our modern industrial society, often rivaling semiconductors for breakthroughs in high-technology capabilities. They play a key role in power distribution; they permit the interconversion of electrical and mechanical energy; they underlie microwave communication; and they provide both the transducers and the active storage material for data storage in computer-based information systems. The properties of magnetic materials are continually being improved, so that many new applications for these materials are now possible. Indeed, magnetic materials seem to offer an endless variety of applications. When one is displaced, another arises. For example, the first generation of computers used magnetic drums for memory. These devices were displaced by matrices of ferrite cores.

As these cores were displaced by semiconductor technology, magnetic materials reappeared in magnetic bubble devices and in disk drives.

Despite the many practical uses for these materials, and despite their critical importance to the nation's industry and defense, it has become increasingly clear in recent years that the role of the United States in the science of magnetic phenomena, in magnetic materials, and in magnetic technology has been declining. Since the mid-1970s, American manufacturers have looked increasingly to foreign sources for newer, better, and cheaper magnetic materials and devices. Nations such as Japan have invested far more than the United States has in the R&D needed to advance the performance of magnetic materials. As a result, U.S. system manufacturers are dependent on systems produced by the foreign component suppliers.

To begin to rectify this situation, research is needed on both the science and technology of magnetism and magnetic materials. The recent report of a Committee on Magnetic Materials (National Research Council, 1985) identifies several new long-term research areas with promise for future payoffs in magnetic technology, such as magnetic superlattices, magnetic phenomena at surfaces and interfaces, and semimagnetic semiconductors. An NSF report (National Science Foundation, 1984) points to the need for further magnetic and related studies of: (1) valence instabilities in magnetic materials; (2) the electronic structures of magnetic systems of restricted dimensionality, including surfaces and bimetal multilayers; (3) the onset of structural instability as heralded by the appearance of strong magnetoelastic effects; (4) the coexistence of stable or unstable magnetic states and superconductivity; (5) magnetic ferroelectrics; and (6) magnetic phenomena in the organic and polymeric solid state.

Research in those magnetic technologies with strong growth potential or strategic value should be increased. At present the greatest emphasis is on compact information storage. Other areas with a critical need for research are new magnetic materials for special purpose magnets for low-power or high-field applications. There are classes of magnetic materials that have more strategic military value than economic potential at present, and in which modest research support would be appropriate. Two decades ago the DOD supported development of microwave ferrites; today, 85 percent of that market is still military. Magnetostrictive sensors for sonar devices are a more recent example. Certain rare earth elements and alloys have extraordinarily large values of magnetostriction. If the resistivity of these materials could be increased significantly without affecting the magnetostriction, the resulting suppression of eddy currents would greatly extend the frequency range of such materials. Magnetic "bubble memories" are another example, in that they are able to compete economically with semiconductor memory in only a few applications; yet their combination of nonvolatility and ruggedness makes them attractive for certain military applications. Bubble devices potentially have much higher storage densities than semiconductor memories. The use of modern lithographic techniques and other procedures for forming magnetic microstructures provides new approaches to recording and storage. The processing of magnetic materials in general has become an important, high-tech field toward which a change in national priorities is in order.

Polymers

Over the past 50 years the introduction of polymers for structural, engineering, packaging, elastomeric, and fiber applications has been one of the spectacular changes in the history of materials technology. Most recently there has been a change in the direction of polymer research and development, away from the synthesis of new commodity materials and toward the formulation of polymer systems engineered for specific end-use requirements.

Examples, in various stages of commercialization, include

- fibers with very high polymer chain orientation, with the strength and stiffness of steel, at a fraction of steel's weight;
- blends and alloys of polymers designed for optimum combinations of stiffness and toughness;
- polymers compounded with inorganic fillers to improve properties such as stiffness, dimensional stability, electrical conductivity, etc.;
- polymer structures (e.g., laminates or films with controlled porosity), to control the diffusion of small molecules, for barrier purposes in packaging, or for separation and purification of materials;
- polymers tailored for application in hostile environments, such as high temperatures or in the presence of solvents or degradants; and

- liquid crystal polymers (e.g., aromatic polyesters) capable of high orientation, leading to strength and stiffness during conventional plastics forming processes, and/or to unique optical properties.

Most of these developments are characterized by a strong interaction between processing and the material itself. Second, many of these developments require the combination of several different materials in order to perform their functions. The emergence of these materials therefore challenges materials science and engineering to understand, control, and optimize on the one hand the interactions of the components and their dependence on size scale, and on the other hand the manufacturing and forming processes.

High-Performance Composites

The development of new composite material systems consisting of high-performance fibers unified by advanced organic and inorganic binders could revolutionize the technologies associated with the aerospace industry, ground transportation, industrial machinery and robotics, and general consumer products. Although raw material costs for high-performance composites exceed those of contemporary materials, their potential for eventual economy lies in the use of processing technologies that allow for near net-shape forming and part consolidation.

The term "composite" embraces any material made up of two or more discrete components that combine to yield enhanced properties. In earlier applications, "composite material" usually referred to arrays of continuous fibers bound together by a polymeric matrix—fiberglass is a familiar example. The broadened application of composite materials encompasses a wider range of reinforcing geometries and binders, commonly including ceramics and metals.

These combinations may yield performance characteristics that cannot be achieved by the various components acting individually. In addition, such material systems offer the advantage of being designed to match the requirements of each application. The newer composites, such as graphite-epoxy composites, have already made great inroads in defense aerospace vehicles (fighter aircraft, missiles, and spacecraft) and are beginning to penetrate the commercial automotive and industrial machinery manufacturing industries (e.g., automobile grill openings and panels, and robot arms and grippers).

Because the microstructure of these materials is determined by the manufacturing process to a greater degree than is the case with contemporary materials, the design process must integrate both the macroscale geometry of the finished product and the material microstructure in order for the full potential of these materials to be exploited. Thus, new design tools that are likely to be based on computer-aided methodologies must be developed so that both the manufacture and behavior of the product can be simulated.

Although composite materials are established in several industries and their use is growing, the realization of their full benefits will require fundamental research on the nature of these materials systems. Predicting the performance of the manufactured product requires an understanding of the failure processes, which initiate in defects and structural inhomogeneities ranging in size from a few nanometers to centimeters. The development and substantiation, through experimental observation, of failure prediction models pose a significant challenge for future research.

Contemporary manufacturing methods are often labor-intensive when high-performance materials are achieved, yet contemporary automated methods do not produce high-performance materials. Thus, another key challenge for research in this important technology is to develop the understanding and tools necessary to produce high-performance properties via automated manufacturing methods.

Performance-Driven Metallic Materials

Performance-driven materials are materials that are selected on the basis of one or more special properties (as opposed to cost). Although many metallic materials may someday be replaced by ceramics, composites, and polymers, modern metals will be an important part of most high-performance structures for the foreseeable future. Moreover, major performance gains can still be made through research on metallic materials. In particular, considerable promise is seen in research in the following areas:

- high-strength/high-density materials;
- lightweight materials;

- high-temperature materials;
- metallic glasses;
- improved efficiency of manufacturing by means of closer process control;
- improved productivity by means of continuous processing;
- chemical assembly and plasma spray build-up of complex and graded alloy compositions;
- materials synthesis using process-structure-property modeling and experimentation;
- improved understanding of nonequilibrium;
- an understanding of the effects of adverse environments on metallic materials performance; and
- improved modeling of structure/fracture properties.

Biomaterials

Biomaterials are used in products that repair, restore, or replace functions (or tissues) of the human body. They are, at present, metallic alloys (e.g., stainless steel, cobalt-chrome, and tantalum and titanium alloys); plastics, such as polyurethane and teflon; some ceramics; and some forms of pyrolytic carbon. As we learn more about controlling acute disease, and thus can extend human life, the problems of degenerative failure of the human body are bound to increase—as indeed they are already doing. The idea of retrofitting the human body may become as commonplace as the preventive and corrective maintenance applied by plant managers to the capital investments for which they are responsible. However, the technical (and human) problems are infinitely more subtle and complex in the case of the human "physical plant."

We need to better understand the interactive processes between living and inanimate materials, so that the reliability, safety, and longevity of materials can be increased. Specific areas needing improvement are corrosion resistance (in metals); biodegradation of polymers; the development of nonthrombogenic surfaces for prosthetic materials to prevent blood clotting; design of device structures to reduce hemolysis (destruction of red blood cells); better understanding of cancer-producing factors for materials; and closer simulation of tissue weight, strength, and modulus characteristics. In addition, specific mechanical properties appropriate for various retrofit functions can be specified, and in many cases, designed.

Areas of Generic Research*

Processing (Including Sensor Research)

Because of the historical emphasis placed by the materials community on materials science and on structure-property relations, there has been far too little coordinated effort on the relation between product performance and the many steps by which that product evolved from some native state or from chemically different raw materials. Perhaps because of the complex chemistry involved (and perhaps also because of its relative newness), the plastics industry provides examples from which some important lessons can be learned. When attempting to manufacture a polymer product to match certain performance specifications, one often seeks leverage from each step in a flowsheet that begins with unreacted monomer. Reaction conditions, types and amounts of additives, chemical distribution of products, and orientation and modification during fabrication all come under scrutiny. From intensive study of the full process, one arrives at various sets of design alternatives and proceeds to find an optimum. The same thought processes need to be applied to materials production in general (see, for example, National Research Council, 1979).

If the approach outlined above is to be successful, new diagnostic tools will need to be more closely integrated into processing steps. We shall have to develop in-line sensors that will inform the manufacturer of changes in processing results long before these changes become apparent as off-specification products. In the ideal case, one would wish to design in-place sensors that can reflect undesirable trends either in processing or, subsequently, in the product during service.

Finally, it should be noted that processing refers not only to steps in the preparation of classic structural materials, but also to the broader range of systems already noted in this report. Hence, we include electronic materials and the manufacture of materials associated with biotechnology, including such processing steps as bioseparations. (See the relevant reports of other panels of the Engineering Research Board, as well as the board's own report.)

We are now in a position to think realistically about processing capabilities undreamed of just a few years ago. An active National Aeronautics and Space Administration (NASA) program

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* These areas are not restricted to specific materials.

of materials processing in space has already shown some of the potential inherent in gravity-free processing of structural, electronic, and biological materials. In space we could study and improve on methods for creating new forms of metals and ceramics. The weightlessness of space also provides a means of separating biologically important materials with methods that, because of the adverse effects of gravity, are impossible on Earth. There are already examples of casting methods in conventional foundries that were improved with knowledge gained from crystal-growth experiments in space.

Durability and Lifetime Prediction

The applications of modern materials are often paced by performance characteristics over their entire lifetime. This is because life-cycle cost is frequently the only factor offsetting the higher initial cost of incorporating modern materials into structures. In order to calculate life-cycle cost accurately, documented models of the lifetime and ultimate failure mechanisms of modern materials are needed. This requirement points to several aspects of time-dependent behavior as promising areas for research:

- models for failure mechanisms;
- corrosion of monolithic metallic materials;
- environmental degradation of composites, polymers, and ceramics;
- models for time-dependent fracture (e.g., creep, fatigue, and stress-corrosion cracking); and
- methods for assessing residual life of materials in use.

Tribology (Friction, Lubrication, and Wear)

Tribology is the science and technology of interacting surfaces in relative motion. It encompasses the fields of friction, lubrication, and wear. Tribological processes involve the interaction of the surfaces in relative motion, the lubricant, and the environment in which they must operate. Tribological processes are highly coupled and require a knowledge of chemistry, mechanics, and transport phenomena occurring under transient conditions and at lengths measured in microns or less. Models are still very tenuous, partly because critical data are hard to obtain.

Tribology is an interdisciplinary field. Many of the research problems in tribology are associated with the need for better sensor development. In particular, we need noninvasive means to monitor chemical and thermal changes over short times and small volumes or surfaces. It is also important to be able to associate research results with predictions of service performance under a variety of conditions.

Materials can fail in response to environmental as well as mechanical attack. There is a need to systematize our approach to understanding pervasive causes of failure, such as erosion and corrosion. These causes are also highly coupled phenomena requiring multidisciplinary approaches.

Computer Modeling

Computer modeling refers to the link between large-scale computation as a modern research discipline (as demonstrated, for example, in the NSF's recent awards to four universities for supercomputer centers) and the need to *predict* how a material will function under given initial and boundary conditions. For example, for semiconductors, the progress in thin-film research that has improved our ability to fabricate artificially structured materials has in some respects outstripped our theoretical models. So many potential materials configurations can, in principle, be prepared that very sophisticated predictive models are needed to select the more promising structures.

Not only is this the essence of materials processing, it is also the final object of engineering research—namely, engineering design. Through sophisticated computational algorithms designed by persons knowledgeable about the physics, chemistry, and theoretical structural mechanics of materials, one can predict how a material, a surface, or a composite will behave under complex state conditions. Such modeling need not utilize the enormous power of supercomputers in order to be valuable. Computer modeling in general has not been adequately developed as a materials systems research tool, and continues to offer great promise for materials process development.

Materials Property Data Base

Existing data bases are inadequate for characterizing both physical and mechanical properties of modern materials. Yet the sophisticated computer modeling we are on the verge of developing can never be more reliable than the reliability and integrity of fundamental property data supplied to the computer's data bank. A generation ago, many laboratories were engaged in measuring physical properties; that activity has long been out of favor. Now, with new materials and advances in computational technology, the need for such data has grown rapidly. We are on the verge of being unable to use computer models of modern material behavior for lack of a sufficiently thorough data base on properties. In some instances empirical relationships, equations of state, and property "maps" from which reliable estimates of needed data can be derived by computer will be more useful and cost effective than tabulations of discrete data. Such data bases should (ideally) be interactive to permit cross-checking, comment, and a degree of correction and update by its users. Communication among researchers in a field would be enhanced by such interaction.

ISSUES DETERMINING THE HEALTH OF MATERIALS SYSTEMS RESEARCH

Need for Priorities

Materials science and engineering issues are pervasive in engineering applications, yet the materials community is fragmented by its very range. Various mission-oriented government agencies support overlapping programs of materials research. As a result, findings resulting from the nation's investment in research are often not efficiently assembled, analyzed, or reduced to practice, and some important areas are inadequately supported.

Because materials science and engineering is such a broad area, mechanisms for setting priorities are not well established. For example, the processes by which materials systems research programs are selected by the federal government are not uniform. They range from peer review through internal agency review to, in the extreme, "pork barrel" techniques. Differences in mission account for some of the differences among agencies in selection criteria and procedures, to be sure; but this fact does not explain or justify the degree of variability that is seen. Although there are some interagency coordinating mechanisms, no true clearinghouse yet exists for setting overall national priorities or directions.

Whether or not national priorities can realistically be set leads to the question of "industrial policy"; this concept, involving centralized planning, is anathema to many in the United States.

In assessing the magnitude of the problem, the Committee on Materials (COMAT) of the Federal Coordinating Council for Science, Engineering, and Technology made an inventory of materials research and technology (R&T) development supported by federal funds for the years 1976, 1980, 1982, and an estimate for 1983. In round figures, about $1 billion was spent in each of these years, with the Department of Energy (DOE) dominating, and with substantial expenditures by the DOD, the Department of the Interior, the NSF, NASA, and, erratically, the Environmental Protection Agency (EPA) (see Table 1). The level of funding since 1983 has been relatively constant, although there was a 5.5 percent decrease in FY86. However, it is estimated that the proposed overall allocation for FY87 will again be about $1 billion (Federation of Materials Suppliers, 1986; Margolin, 1985). Private investments in materials research and development are hard to quantify, but estimates of about $4 billion are heard.

These figures seem to suggest a field that is, if not expanding, at least holding its own. However, they are not otherwise very illuminating, and may actually be misleading because of the lack of consistent principles for aggregating the data. To begin with, the portion spent for research is unknown, as is the relative allocation between materials science and engineering. It is generally believed that the majority of materials engineering work is performed in other engineering fields (e.g., electronics), and particularly at the interfaces of various engineering disciplines. Funding for this work does not appear in the table. The DOD may sponsor a good deal more materials research than is reflected in the table (some say as much as $500 million more). The interdisciplinary nature of materials systems research makes it virtually impossible to gain a clear understanding of how much is spent, where it is spent, by whom, and for what purposes. The same statistical difficulties are found in attempting to assess student enrollments, degrees, and faculty numbers and specializations within the materials field. Better data on a national basis are certainly needed to support policy studies in this crucial field. The federal government should sponsor an effort to determine which specific areas of the field need better definition and data, and how this can best be accomplished.

Table 1 Federal Materials R&T Funding (in $thousands)

	Funding			
Source	1983[a]	1982	1980	1976
DOE	276,207	285,262	514,100	332,897
DOD	162,200	147,000	160,200	131,881
EPA	179,426	146,693	2,400	88,398
Interior	104,609	113,667	119,686	165,350
NSF	102,414	99,721	88,920	68,700
NASA	102,305	101,415	78,582	51,533
Health	34,260	31,920	6,070	16,625
Agriculture	28,551	29,759	64,598	38,254
NRC[b]	18,740	17,710	13,674	7,028
Commerce	13,856	14,201	35,795	21,080
Transportation	6,854	5,682	3,442	6,153
TVA[c]	2,607	2,792	9,650	9,226
Treasury	1,258	1,080	2,516	790
Smithsonian	825	750	1,000	1,000
FEMA[d]	0	107	50	0
HUD[e]	0	0	0	6,669
State	0	0	0	540
Labor	0	0	3,000	4,063
GSA[f]	0	0	0	132
Total	1,034,112	998,329	1,103,683	961,320

[a] Estimated amounts.

[b] Nuclear Regulatory Commission.

[c] Tennessee Valley Authority.

[d] Federal Emergency Management Agency.

[e] Department of Housing and Urban Development.

[f] Government Service Administration.

SOURCE: COMAT, 1984.

Given the uncertainties, the main point to be made with regard to funding in the materials field is not whether there is enough money spent, but whether it is spent in the right places, whether the balance between materials science and engineering research funding (to the extent that they can be clearly differentiated) is appropriate, and whether there is continuity of effort along the entire R&D chain. It should also be noted that the apparent proportion of government-to-industry funding in this field (20 percent government, 80 percent industry) is considerably smaller than the proportion in most other technical fields (an average of about 40 percent government, compared with 60 percent industry). This fact suggests that not enough of the kind of long-range, fundamental engineering research normally supported by government is being pursued. It is likely that the largely developmental work pursued by industry could be leveraged and made more effective through a greater effort directed at understanding *how* materials processes work.

Although definitional inconsistencies make it difficult to sort out the relative expenditures on different aspects of materials science and engineering, two points stand out: (1) government funding in this field is unusually low in comparison to industrial R&D spending, and (2) there is a strong perception that basic scientific research and expensive major facilities account for the majority of federal R&D funds, with only a fraction of the total going into materials systems and processing research. Recent studies (e.g., U.S. Department of Commerce, 1984; Government Accounting Office, 1985) point out that other nations—especially Japan—are placing much greater relative emphasis on the processing and aspects of materials and are poised to reap great benefits as a result. The question of proper balance should be examined and, if necessary, priorities should be altered accordingly.

To address these and other questions, Title II of the National Critical Materials Act of 1984 establishes a National Critical Materials Council under and reporting to the Executive Office of the President. This Council is composed of three members, with one member having "a background in and understanding of environmentally related issues."

The bill spells out the purposes, responsibilities, and authorities of the council. They are, in a word, sweeping, including the establishment of a national Federal program plan for advanced materials R&D. There is concern within the materials science and engineering community that this small group will autonomously (and perhaps unduly) influence the allocation of funding in the materials field. The panel doubts that this mechanism can accurately represent the needs and priorities of a constituency as broad as the materials community. We therefore recommend that another, less centralized mechanism be found for achieving these desirable goals.

Research Facilities

Another set of priorities of importance to materials scientists and engineers involves the division of expenditures between salaries and facilities for carrying out research. Facilities fall into three general categories:

1. Major: large national or regional facilities, available on an open or proprietary basis, with or without payment of operating fees (examples are facilities for synchrotron radiation, neutron scattering, and high-resolution electron microscopy);
2. Intermediate: local facilities shared among users at a single location (e.g., electron microscopes, lasers, molecular-beam epitaxy or chemical vapor deposition apparatus, clean rooms, specialized magnets, mid-size computers, etc.); and
3. Small-scale: individual group equipment to be used ordinarily by one principal worker and that person's colleagues and students.

The allocation of resources among these three classes and within each class is obviously a very subjective and controversial matter. As in other areas, the diversity of backgrounds of people who identify with materials research makes it difficult to achieve a consensus that represents *all* of the needs of the community. Sometimes the needs of the "best-connected" people and institutions therefore prevail. Such needs, if narrowly defined or self-serving, will almost invariably fail to represent the mainstream of the materials community. This is especially clear if the "mainstream" is defined as that part of the community that is central to the competitiveness of U.S. producers and users of performance-driven materials.

An attempt has recently been made (by the Commission on Physical Sciences, Mathematics, and Resources of the National Research Council) to assess priorities within the major-facility category (National Research Council, 1984b). Consensus was reached among members of a panel chosen for diversity of interests, research styles, and institutions. The committee's recommendations for new facilities, and their estimated costs in FY85 dollars, are (in order of priority):

1. a 6-GeV synchrotron radiation facility ($160 million);
2. an advanced steady-state neutron source facility ($260 million);
3. a 1-to 2-GeV synchrotron radiation facility ($70 million); and
4. a high-intensity, pulsed-neutron source facility ($330 million).

For comparison, the panel has estimated the cost of setting up facilities for materials processing research, which is currently a neglected area. These facilities are classed as "intermediate facilities." Estimates of their costs are (with no priority implied by ranking):

- a composite processing laboratory for metals, ceramics, or polymers ($5 million each);
- a semiconductor processing research facility ($7–$8 million each); and
- a molecular-beam epitaxy or metalorganic chemical vapor deposition research facility ($2 million each).

There is an urgent need for facilities such as these in universities across the country. We believe that policymakers have commonly failed to recognize research needs that relate directly to the competitiveness of the materials sector of U.S. industry. To be sure, the new concept of funding major cross-disciplinary research centers is intended to be a step in this direction. The Center for Advanced Materials at the University of California at Berkeley and the NSF Engineering Research Centers at the University of Delaware and the University of California at Santa Barbara have the potential to make strong contributions in the materials processing area. But these as-yet unproven centers are only a beginning. More processing-oriented research facilities are needed for many types of advanced material. The cost of these greatly needed facilities, which will produce fundamental knowledge that will rapidly translate into economic gains, is quite low in comparison to the costs of major facilities used to conduct basic research in materials science. Indeed, much of the approximately $1 billion in federal funding depicted earlier in Table 1 supports the physical science aspects of materials research, which often requires costly equipment and facilities. In 1986, for example, 24 percent of the budget of DOE's Materials Sciences basic research subprogram (by far the largest unit for materials R&D in DOE) is devoted to facilities and equipment; in 1985 the figure was 31 percent (Margolin, 1985). *The facilities needed to fill a significant gap in our current materials science and engineering research capabilities and produce knowledge of key economic importance would have a total cost in the range of $150 million.*

University-Government Relations

Although support from industry, state governments, and various other sources has become increasingly important to the materials science and engineering research enterprise, federal agencies continue to be major factors in supporting this research. The government agencies that are concerned primarily with basic research (particularly NSF, DOE, NASA, DARPA, the Air Force Office of Scientific Research, the Army Research Office, and the Office of Naval Research) have generally understood the nature of university research and its importance to national goals.

University research should address the fundamental aspects of materials, devices, and processes. It should be recognized that the accompanying education of Ph.D.s in these areas is of substantial importance to the country's economic health and defense.

To the extent that university-government interactions stimulate, support, and facilitate that type of fundamental investigation and excellence of education, such interactions can be extremely healthy. On the other hand, restrictions and impediments to such research and education can result in an eventual degradation of the economic and defense enterprises which these agencies are charged with supporting.

Mission Orientation and Overmanagement

Federal funding of university research in the postwar period has transformed engineering colleges into major research institutions. However, this support from federal agencies has carried with it a potential for conflict between the free, creative exploration of new ideas on the one hand and these agencies' needs for program development on the other. New materials and new processing techniques important to the country's needs have frequently emerged through these funding programs. Yet if such programs are too narrowly focused, the hoped-for objectives of creative innovation and discovery may be lost.

In materials research, a case in point is the current technological need for a metal-insulator-semiconductor structure in compounds such as GaAs or InP to provide a metal-oxide-semiconductor (MOS) technology similar to that which has been so productive in silicon. Recognizing the importance of this technology to future electronic devices, government agencies may fund work at universities to study the properties of interfaces between compound semiconductors and various insulators. On the other hand, a narrowly defined contract on MOS structures between specific compounds and specific insulators, with deliverables at the end of the contract period, would probably fail because the basic work on interface properties has not been done in detail. Researchers should instead be encouraged to look carefully at the basic materials properties before trying to develop devices.

The problem of overmanagement from Washington need not be associated only with applied research having short-term payoffs. Even in pursuit of basic research, in which a far-sighted approach is being taken by the sponsoring agency, research management that is too detailed can stifle the investigator's ability to capitalize on emerging results.

Overmanagement of a project by the sponsor, whether in basic or applied research, reduces the chance of success and should be avoided. The investigators themselves should be permitted to define and follow their own research strategy. If reviews of a specific research effort after a reasonable period of time (often 3 or more years) indicate a lack of progress or loss of focus, such research should be terminated with the provision of adequate funding to allow any graduate students to finish their thesis work.

Dilution of Basic Research

Because the program needs of DOD and other agencies of the government often bias these programs toward specific problems, there is a tendency to dilute the pool of basic research funding with mission-oriented projects. Although many faculty members are able to extract good fundamental research from a program with a rather applied mission focus, the risk remains that such well-defined programs may stifle the creative enterprise so important to university work. The increase in theme-oriented basic research is an unwelcome trend. If "suggested areas of research" predominate, with no increase in total resources, then truly unsolicited research becomes harder to find. *Agencies must carefully protect the rather modest amount of money going to fundamental research and avoid the temptation of putting highly mission-oriented projects on the list of basic research programs.*

New Thrusts

One of the major responsibilities of support agencies is to remain alert to new developments in the fields they are supporting; the best way is by involvement with productive researchers. These agencies can and should alter their programs to accommodate new areas that emerge from ongoing research. On the other hand, it is important that research funding remain relatively stable at universities, at least on the timescale required for Ph.D. thesis research. In many cases, considerable time is required to bring projects to completion. Particularly in the areas of fundamental materials research and development of new processes, stable research funding over long periods of time (e.g., 5 years) is an almost universal requirement. As a result, funding agencies should be careful not to abandon productive areas of work to chase every new idea.

Given the inevitable restrictions on a particular agency's funding, balancing stable funding for ongoing productive research against the need to support new thrusts and ideas is a delicate matter. *Each agency should have some portion of its budget available for pursuing unexpected developments without detracting from support for productive ongoing projects*. As is often done, reasonable deviations from the originally proposed approaches should be encouraged. At the same time—and perhaps partly as an offset—less productive programs of research should be phased out and terminated. In every case the importance of creativity, innovation, and productivity should be emphasized over a strict adherence to preconceived notions about the direction and methodologies of the research.

Use of Government Research Facilities

Universities can greatly benefit from the use of specialized materials research facilities at government and government-supported laboratories, such as Oak Ridge and Sandia National Laboratories. This is particularly true in the case of large or expensive equipment and facilities. In addition, the expertise of a laboratory's staff can complement the experience of university faculty in materials research. These facilities can also be very effective as sites for the conduct of graduate student thesis research. Such arrangements must, however, be organized to maintain academic control over thesis topics.

There has been good experience at many laboratories in sponsoring such thesis research visits from universities. *In general, universities who are able to use these facilities should take full advantage of the equipment and expertise resident within them.*

Restricted Data

On rare occasions, a new material or process emerging from a university research project warrants protection in the national interest. It is extremely important, however, that funding agencies restrict data reluctantly.

Generally, a free exchange of research data and the publication of results enhances the entire research and development enterprise. This is particularly true in the case of universities, which depend on publications and technical meetings as their research output, rather than providing products for the marketplace. Given the fundamental nature of most materials research at universities, it is rare that results and data are of such immediate use to defense objectives as to require restriction from publication.

There are developments that permit optimism in this regard. In October 1984, then Under Secretary of Defense Richard DeLauer issued a memorandum specifying that no restrictions should be placed on the publication of unclassified fundamental research sponsored by DOD. The new rule thus applies to all DOD-supported research on university campuses; definitions generally cover all 6.1 research and any unclassified research performed on-campus from 6.2 funds. Consideration is being given at the White House to establishing these policies in all other federal agencies. Thus, for the time being, the classification prior to the granting of a DOD research contract is the deciding factor, and most universities will not undertake classified work. *The assumption should be that research results are freely publishable and that restriction of these results is an exception requiring careful consideration by the agency and discussion with the principal investigator.*

ASSESSMENT OF PERSONNEL RESOURCES

Introduction

The success of research in materials systems clearly depends on having the talent available to conceive and carry out that research. This section deals with the availability of new research talent, primarily from graduate programs in materials science and engineering in the United States. These disciplines and research areas include metallurgy, ceramics, polymers, electronic materials, and portions of solid state physics and the chemistry of materials. The industries that will benefit most from the availability of capable graduates in those fields are those producers and users with R&D programs looking ahead to new materials and processing methods for the next generation of materials systems.

Creating the environment, identifying the appropriate problems, and providing the tools for inventive and imaginative research by Ph.D. students is one of the strongest challenges facing the universities with programs in these fields. It is also one of the most crucial issues determining the future of materials systems development in this country.

Assessment of Students and Graduates

It is extremely difficult to measure the quality of students by any objective criteria, particularly in research areas requiring not only academic skills but also the ability to understand and creatively use complex fabrication and measurement facilities. Data on total enrollments are also difficult to pin down, because the materials field is highly interdisciplinary. Therefore, this section largely summarizes the panel's general impressions on the availability and characteristics of students in materials research fields, as compared with engineering and science students generally.

Ability of Graduate Students

Most candidates for graduate degrees in materials programs come from undergraduate engineering curricula, with a smaller number coming from science disciplines such as physics and chemistry. As in other fields of engineering, attractive salaries in industry draw many of the most talented students into the workforce at the B.S. level, so that relatively few continue on toward a Ph.D. and research. Materials researchers must have strong backgrounds in both science and engineering. Thus, one disincentive for many top students is the substantial time and effort involved in pursuing a doctoral program in this field.

Because of the wide range of knowledge and skills required for success in materials research, materials science and engineering students tend to be very broad in their background and interests. Indeed, some enter the materials field through double-major programs in conjunction with mechanical, aerospace, or other engineering majors. Students who pursue advanced study tend to proceed entirely through the Ph.D. program and to do productive work. However, it is generally apparent to faculty members that the best students are not choosing to enter materials programs, with the exception of certain well-publicized fields such as electronic materials. It is difficult to compete with fields such as computer science and bioengineering, which receive more favorable press than does materials science and engineering. There is an image problem to be overcome in attracting the brightest students at both the undergraduate and graduate levels.

Certainly one positive note is the influx of women into the materials field in recent years. Indeed, the rise in the percentage of female students is the largest in any engineering discipline. Women now make up about 14 percent of all graduate students in materials and metallurgy (the closest relevant category) (Engineering Manpower Commission, 1985a). At some schools the percentage of women is even higher—17 percent at MIT, for example (Engineering Manpower Commission, 1985b, 1986). These women seem to perform well in materials graduate programs and as researchers.

A continuing dilemma in materials research is the high percentage of non-U.S. citizens who are enrolled. Often these students are superbly qualified from a technical standpoint, but are not able to provide the level of interaction with faculty, graduate students, and undergraduate students that leads to a healthy discipline. After receiving an advanced degree, these foreign students often, for economic or political reasons, return to their homeland. As a result, we frequently train personnel who in turn are at the leading edge of technology in countries with which we are, or will be, in direct economic competition.

Issues of Supply and Demand

General Concerns

The output of materials science and engineering graduates is adequate on average but inadequate at times of peak industrial expansion. A central problem is the cyclical nature of the demand and the long lead time in the university's response to changes in the demand for engineers from a particular discipline or specialty area. In particular, the longevity of tenured faculty appointments leads to inertia in the ability to change emphasis within a discipline. (This is also true in other rapidly developing fields besides materials.) As a result, university administrators are cautious when responding to the call from industry to increase the degree-granting capacity of or shift the emphasis of a program relative to a current "hot" discipline or area such as electronic materials. This cautious attitude has been legitimized in recent years by a growing tendency of employers to treat engineers (in research as well as applications) more like a commodity than a resource.

If there is to be a better match of supply and demand, positive action must be taken. First, an improved method for forecasting the need for engineering graduates at all degree levels must be developed. Second, longer term commitments by employers are needed. Third, some methods for protecting universities against the "downside" are required. The resources to implement this protection should come from industry, but probably will have to be provided by government before any real prospect of achieving a better balance between supply and demand is to be achieved.

Graduate Degrees

The supply of M.S. and Ph.D. graduates in the materials field is, as is suggested earlier, quite low. Even in the burgeoning field of ceramics, for example, annual degree production nationally in 1984 was 87 at the M.S. level and 18 at the Ph.D. level (Engineering Manpower Commission, 1985). Ceramics is the only specialty of materials for which data on degree production are readily available. In the broader field of materials and metallurgical engineering, in the same year there were 578 M.S. and 241 Ph.D. graduates nationwide. It is not possible to determine how many of these students were specialized in the advanced materials systems areas addressed in this report.

The balance between supply and demand for those with advanced degrees in materials research appears to vary considerably across specialties. Whereas the demand for Ph.D.s in electronic materials and related fields is very strong, the hiring needs in some other fields are static or declining. For example, virtually all M.S. and Ph.D. graduates in ceramics are hired to fill positions related to electronic and structural ceramics, rather than those related to conventional ceramics.

The panel believes that the growing dependence of the nation's economy on new materials ensures a long-term increase in demand for doctoral-level researchers in every area of the materials field, for both industry and academe. *Therefore, it is necessary to take steps that will encourage more of the brightest undergraduate students to enter graduate programs in materials science*. Students should be counseled on the advantages of graduate education in materials, particularly regarding the Ph.D. as an entrée to a research career. Undergraduate students in relevant disciplines should be exposed to materials engineering research. At universities with strong graduate programs this could be done by offering undergraduates (especially entering freshmen) opportunities for part-time and/or summer employment in such programs, and by organizing undergraduate thesis programs.

A large pool of potential talent also exists at smaller schools unable to offer such opportunities. Undergraduate seminars should be given at these schools by academic and industrial researchers to acquaint students with materials systems research fields. In addition, summer jobs in industrial and academic research centers should be made available to interested students. Federal and industrial funding ought to be provided to support these recruitment activities.

Because a lack of awareness of the materials field is a large factor in restricting the supply of graduates, the panel believes that efforts to increase students' interest should be made as early as possible in the educational experience. Therefore, the existence, importance, and challenge of materials systems research as a discipline should also be communicated to high school students. Steps should be taken to ensure that the materials field catches the interest of young women as well as young men. Possible mechanisms to do this include industry sponsorship of engineering fairs (analogous to science fairs); production of suitable video or film programs by professional societies; and tours of academic, government, and industrial laboratories engaged in materials research.

Adequacy of Graduate Programs

This section deals with two factors perceived to be at the heart of quality graduate programs in materials, namely, new programs and the adequacy of the faculty.

Development of New Programs

There are relatively few established formal graduate programs in materials. In ceramics, for example, only 10 schools produce any advanced degrees at all, and only one program is accredited at the master's level (Engineering Manpower Commission, 1985a). In composites there are five formal graduate programs; in polymers there are even fewer programs. New programs are needed in the materials field to meet the emerging demand.

For new graduate programs to be most effective, they should generally be specialized. Such programs should expect to build on a student's strong grounding in fundamentals, and should focus their resources on achieving quality education in a single area such as composites or ceramics. In any field of engineering the development of a high-quality, broad-based program takes considerable time and requires extensive resources. It seems unlikely that many new broad-based programs will be able to compete with established programs for full-time graduate students, especially in coming years, when there will likely be fewer students. Without an adequate supply of full-time graduate students, strong research programs of the type that characterize the great research universities in the United States cannot develop.

There is probably adequate overall capacity in universities to conduct materials research, at least in a time-averaged sense, but sheer research capacity is not the sole motivation for developing new programs. In fact, the development of new programs in engineering is motivated as much by the need for graduate education—especially part-time programs—as it is by research needs. That driver seems to be closely tied to the shifting geographic locations of the "market." This is especially true in the case of part-time graduate study, in which case the university program must be near the workplace to enable part-time students to commute from school to work. In many cases, the new locations of industry are not near well-established universities; thus, there is a tendency for new programs to develop at regional universities and colleges. The deceptive aspect of this is the time required for these new programs to achieve adequate academic stature. If the employers in the new location recognize the need for new technology, then this "quality" issue becomes a major barrier to acceptance of the developing programs. That is, employers who recruit bachelor-level engineers from the best schools in the country find that these people have little interest in continuing their education on a part-time basis at institutions of distinctly lower stature than those at which they studied as undergraduates. Thus, whereas the development of some new programs is inevitable, it may be more productive for the existing established institutions to work harder at delivering part-time graduate programs off-site using such means as visiting faculty, teleconferencing, or videotaped instruction. There are a number of prototype off-site programs that could be mentioned in this context. The degree of success that these programs have achieved is as variable as their formats.

Therefore, the need for programs offering part-time graduate study in materials in areas where high-tech industries are locating can best be served by established, first-rank institutions offering such programs off-site.

Adequacy of the Faculty

In the major existing programs the supply of faculty is generally adequate, with the possible exception of new directions within these programs. Two examples of such directions are advanced ceramics and electronic materials. Even in the few institutions in which these relatively new activities began and are now expanding, there are not enough faculty. Moreover, those new faculty that are available come from only a few schools, so that a degree of inbreeding inevitably occurs. (Here, programs of postdoctoral research abroad offer some opportunity for "cross-fertilization"; France and West Germany in particular have attractive university research programs.) Similar problems also occur in other emerging areas of materials research. There are probably fewer than

40 full-time-equivalent faculty involved in composites research, for example.

Table 2 Materials Processing Faculty in the U.S., by Speciality

	1978		1984	
Speciality	No.	Percent	No.	Percent
Casting/solidification	22	32	16	17
Deformation processing	15	22	18	20
Design/systems	4	6	10	11
Powder metallurgy	13	19	14	15
Welding	14	21	16	17
Ceramic processing[a]	0	0	10	11
Polymer processing	0	0	8	9
Total	68		92	

[a] A different and probably more reliable census yielded numbers of 28 and 36 in this category for 1978 and 1984, respectively.

Source: American Society for Metals, 1985.

Table 2 shows a recent estimate of the specialization of all U.S. faculty in materials processing (American Society for Metals, 1985). The absolute numbers cannot be assumed to be accurate (see, for example, the notation regarding ceramic processing), but their magnitudes and relative figures are probably reasonable. One sees that even in this currently much-emphasized field, the overall numbers are still quite small, and that the fields showing high relative growth rates are in highly publicized speciality areas within materials science.

There is some concern about the loss of faculty to industry, especially in "hot" areas. This has become less troublesome since universities began to realize that they must offer more competitive salaries. However, improvements have been most marked in the first-rate schools; qualified faculty members are still leaving many institutions for better paying industrial jobs. To help alleviate these problems, industry should be encouraged to support the entry of new graduates into academe. The "forgiveable loan" program for graduate students sponsored by General Electric is an example of such encouragement. In this program, loans of up to $5,000 are made to Ph.D. candidates, and repayment is forgiven if on graduating the student pursues an academic career for at least 4 years.

It is particularly hard for developing institutions to compete for high-quality faculty, both on the basis of the environment they can provide (facilities, colleagues, etc.) and on the basis of salary. Therefore, in general it is probably true that quality is a bigger problem than quantity in these programs. In the most competitive areas of materials specialization, it may be difficult for emerging programs to compete for any of the limited number of qualified faculty candidates. In these areas, both quality and quantity are a problem.

New Ph.D.s are especially hard for universities to recruit. There are many disincentives that a recent Ph.D. graduate must face as a new faculty member. Some candidates are uncertain about widely publicized issues such as "publish or perish," obtaining research support, and the promotion and tenure decision processes. The NSF's Presidential Young Investigator Awards help somewhat to ease the question of research support, even though the required industrial matching money has been difficult to obtain in many instances. In addition, it appears that university administrators are beginning to recognize the need to provide a transition period for young faculty to adjust to the spectrum of activities they must learn to deal with simultaneously. The bottom line is that outstanding young faculty can adjust to the university climate and thrive. The ones who have succeeded serve as role models for others and are a source of encouragement. Programs that have no such role models often find it harder to present a convincing case for their environment being "penetrable." These programs may have more difficulty recruiting young faculty.

Continuing efforts should be made by universities to improve the attractiveness of academic life for entry-level faculty, especially in emerging or high-priority areas such as electronic materials, advanced ceramics, and processing. The Presidential Young Investigator Awards or comparable long-term research grants represent one effective mechanism for doing this. Government and industry should both be willing to fund this type of grant in targeted specialized areas.

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APPENDIX RESPONSES TO THE ENGINEERING RESEARCH BOARD'S REQUEST FOR ASSISTANCE FROM UNIVERSITIES PROFESSIONAL SOCIETIES, AND FEDERAL AGENCIES AND LABORATORIES

Requests for assistance were sent by the Engineering Research Board to a number of universities, recipients of Presidential Young Investigator Awards, professional societies, and federal agencies and laboratories in order to obtain a broader view of engineering research opportunities, research policy needs, and the health of the research community. Some of the responses included comments on engineering research aspects of materials systems research; these were reviewed by this panel to aid in its decision-making process. The panel found the responses most helpful and wishes that it were possible to individually thank all those who took the time to make their views known. Because that is not practical, we hope nevertheless that this small acknowledgment might convey our gratitude.

Responses on aspects of materials systems research were received from individuals representing 46 different organizations, listed in Table A: 22 universities (including 9 represented by recipients of NSF Presidential Young Investigator Awards), 11 professional organizations, and 13 federal agencies or laboratories. Some comments covered specific aspects of the panel's scope of activities, whereas others provided input on a variety of subjects.

Although most of the responses addressed priority research needs, several respondents did reflect on policy issues. Many of the research needs and issues of policy and health addressed by the respondents were similar to those noted by panel members. The broadened perspective provided by the responses to the survey was most beneficial in the panel's deliberations.

Table A-1 Organizations Responding to Information Requests Relevant to Materials Systems Research

UNIVERSITIES
Clarkson University
Lehigh University
Massachusetts Institute of Technology
North Carolina State University
Northwestern University
Ohio State University
Oregon State University
Rensselaer Polytechnic Institute
State University of New York, Buffalo
Texas A&M University
University of Connecticut
University of California, Davis
University of California, Los Angeles
University of Florida
University of Illinois-Urbana/Champaign
University of Kansas
University of Michigan
University of Oklahoma
University of Pennsylvania
University of Texas at Austin
University of Utah
Washington University at St. Louis
PROFESSIONAL ORGANIZATIONS
American Chemical Society
American Institute of Aeronautics and Astronautics
American Institute of Chemical Engineers
American Society of Civil Engineers
American Society of Mechanical Engineers
Council for Chemical Research
Institute of Electrical and Electronic Engineers
Institute of Industrial Engineers
Industrial Research Institute
Society of Automotive Engineers
Society of Engineering Science, Inc.
AGENCIES AND LABORATORIES
Air Force Institute of Technology
Air Force Office of Scientific Research
Army Materials and Mechanical Research Center
Army Research Office
Brookhaven National Laboratory
Lawrence Livermore National Laboratory
NASA Jet Propulsion Laboratory
NASA Langley Research Center
NASA Lewis Research Center
Naval Research Laboratory
Office of Naval Research
Oak Ridge National Laboratory
Sandia National Laboratory

Transportation Systems Research in the United States: An Overview

EXECUTIVE SUMMARY

Transportation systems and services (on land and water, in the air, and in space) account for some 20–25 percent of the U.S. gross national product (GNP)—a striking measure of their critical importance to the nation. Transportation is such a ubiquitous feature of modern life that we tend to take it for granted. Yet the efficient and productive design, manufacture, operation, and control of transportation systems is key to the ease or difficulty of day-to-day life and our ability to respond effectively to a national emergency. Because it has a major impact on the cost of goods and their delivery, transportation is also an important key to the competitiveness of companies in the domestic economy and of U.S. industries in the world economy.

Previous engineering research has made possible today's highly developed and complex transportation systems. Continuing research is essential to meet the ever-increasing transportation demands of individuals, the industrial and commercial sectors, and the military. Individuals, organizations, and nations prosper and develop in concert with progress in transportation. If the United States does not contribute substantially to that progress, it will have to defer to other nations in both the economic and defense realms for the advances they devise.

The field of transportation research encompasses almost all of the disciplines and interdisciplines of engineering. It has strong hardware and software components that include vehicles, the guideways along which they travel, and the "intermodal" interfaces or facilities connecting two or more modes of transportation. Important methodology questions of network analysis and design, communications and control, logistics, and system planning and management complement the concern with equipment and facilities.

Important issues for future transportation systems pertain to the need to: increase productivity, increase defense effectiveness, limit undesirable congestion, improve reliability, reduce life-cycle costs, protect the environment, manage safety hazards and risks, and conserve scarce energy and material resources. From these demands derive a wide range of engineering research requirements and opportunities. The Panel on Transportation Systems Research has selected a few research needs from among the many that deserve special emphasis and attention. The research needs identified here are grouped into four categories. Those needs in the first category (needs that are applicable to all modes of transportation) are the most critical, and the panel wishes to emphasize that they present the greatest opportunities for benefiting the nation.

The reader is cautioned that the topics chosen are those judged to be of prime importance in meeting transportation needs relevant to the civilian sector, including those common to both the civilian and defense sectors. Therefore, many areas of critical importance to the U.S. Air Force, Army, Navy, Marines, and Coast Guard that are strictly military in nature do not appear. Among those areas are the technological research subjects essential to an adequate global combat readiness—the rapid and effective transportation of materiel and personnel to and within possible combat areas.

1. Engineering research needs applicable to all modes of transportation (i.e., cross-modal research):

- systems engineering (integration of all or major segments of the entire spectrum of vehicle design, vehicle-guideway interactions, traffic control, intermodal interfaces, and the planning and logistics of transportation networks for moving people and goods);
- the mechanics of slowly deteriorating systems;
- energy conversion and pollution control;
- fluid dynamics of separated flows; and

- nonlinear collapse of structures.
- 2. Fundamental engineering research areas of special relevance to transportation systems:

- tribology;
- computational fluid and solid mechanics; and
- the man-machine interface.
- 3. Broad, fundamental research areas within the purview of other panels of the Engineering Research Board:

- computers and control technology;
- manufacturing sciences;
- composite and other advanced materials; and
- structures.
- 4. Specific aspects of (civilian) modes of transportation requiring research:

- *aerospace*—gas turbine engines, laminar flow control or turbulence suppression, novel configurations and the influence of structural design on aerodynamics, interactions among components, and transatmospheric propulsion;
- *Maritime*—ports, terminals, and waterways; cargo handling systems; hazardous cargos; new ship forms; and shipbuilding;
- *Automotive/highway*—urban/suburban street and highway maintenance, safety, and highway productivity;
- *Railroads*—lighter-weight rolling stock, safety, signaling and communications, maintenance of track structure, evaluation and inspection; and
- *Pipelines*—in-situ inspection and refurbishment, pneumatic transport of fluidized solids, and capsule transport.

The panel found that federal support for engineering research in transportation is very uneven across the different modes. Support for aerospace research is substantial; support for the automotive/highway and maritime modes is moderate, but inadequate in many areas; and federal research support for the rail and pipeline modes is virtually nonexistent.

Many components of all transportation modes, including most of the guideways (e.g., highways and aircraft flight paths) and many of the intermodal interfaces (e.g., ports and terminals), are in the public sector; research on these components is clearly in the government's domain. Research on other components (e.g., most familiar vehicles) is being addressed more or less satisfactorily by the private sector; nevertheless, there are important areas of fundamental research that the private sector cannot adequately pursue and that should be supported by government. In addition, the Department of Transportation (DOT) and other federal agencies should increase the research on which their regulation of the transportation industry and equipment/vehicle manufacturers is based, in order to ensure that such regulation meets national objectives of safety and productivity.

The government is also responsible for the national defense, of which transportation systems are a highly significant element. The panel found that, in areas in which there is a clear and direct relationship between defense and an element of civilian transportation (e.g., air traffic control), federal support for research is adequate. In areas in which there is a less direct relationship, inadequate research may lead to a situation in which civilian transportation systems either do not exist or could not be mobilized quickly enough to meet national defense needs. Fundamental engineering research is needed to define the transportation problems that would be posed by various national emergencies and to provide the knowledge required to solve these problems.

The federal government should assist the maritime and railroad industries in moving toward substantially increasing their research activities in order to meet worldwide competition and to develop a readiness for defense mobilization. In addition, the potential for more fully exploiting pipeline systems should be carefully examined from an engineering and economic standpoint. Research should provide the knowledge base for new pipeline inspection and repair methods.

When a component of a transportation system is in the public sector (and in the aerospace mode generally), government funding should be directed at both fundamental and applied engineering research. For all other civilian components, it should be directed primarily at fundamental research. The National Science Foundation (NSF) should mount a significant program of fundamental research in transportation (including its methodological aspects). In addition, the DOT should resume and expand its programs in support of fundamental research across the spectrum of transportation, with universities as the principal performers, so that knowledgeable graduates will be available to both industry and government.

The panel found that an adequate number of university faculty is currently involved in the methodological aspects of transportation; however, this number is decreasing because of a lack of both research funding and graduate students. The hardware-oriented side of the field draws practitioners from all disciplines, but there is a shortage of qualified and interested Ph.D.s and doctoral candidates (especially U.S.-born candidates). If universities receive the majority of the federal government's funding of fundamental engineering research, this will increase both the knowledge base for private sector development of transportation systems and the supply of appropriately educated people. Additional efforts on the part of schools, industry, and the government are needed to attract greater numbers of qualified Ph.D. candidates into transportation research.

INTRODUCTION

Scope of the Panel's Report

The Panel on Transportation Systems Research was charged with examining issues posed for its parent body, the Engineering Research Board, relating to those areas of engineering research critical to the future of transportation systems within the interests of the United States. The panel's charge encompassed civil and military transportation systems alike, operating in every sphere—land, water, air, and space.

Included within this broad scope was the identification of key areas of engineering research, as a basis for achieving a wide range of objectives:

- to improve the efficiency, safety, environmental effects, cost, reliability, and durability of the components of transportation systems;
- to optimize the design, management, and control of transportation systems, including air, ground, and maritime systems;
- to enable better decisions to be made when choosing among existing modes and intermodal systems; and
- to develop new or greatly improved transportation systems, including interfaces among modes of transportation.

Background

The term "transportation systems" refers to the various means by which technology and resources are applied to the movement of people and commodities from place to place in response to economic and societal needs and desires. Transportation systems and services account for some 20–25 percent of the GNP—one striking measure of the critical importance of this field. In an increasingly global economy, the productivity and efficiency of transportation is a key element in the competitiveness of individual companies and U.S. industries alike. It is equally critical to the nation's defense readiness.

Today's transportation systems have drawn heavily from practically every area of engineering research to achieve their present level of effectiveness, safety, reliability, and economy. Tomorrow's systems—including those not yet developed—will continue to depend on the broad spectrum of engineering research for the many basic elements of knowledge they will require to meet the nation's transportation needs in a safe, reliable, and economical manner.

The field of transportation is so broad that it encompasses most engineering disciplines and interdisciplines. Research results. in materials, in fluid mechanics and combustion, in solid mechanics and structures, in electronic devices and controls, in computer and systems science and engineering, in manufacturing and fabrication, in construction, and in aspects of the man-machine interface have been applied to transportation in a highly integrated way. This research has enabled the transformation of primitive predecessors into the present surface vessels, submarines, trains, autos, trucks, off-road vehicles, pipeline systems, aircraft and space vehicles, and other familiar people and goods movers and the visible and invisible paths on which they move.

It might be tempting to believe that so much has been accomplished in the transportation field to date that little dramatic progress can be expected in the future. The literature of past decades and centuries is replete with assertions that "they've gone about as far as they can go." Each time, the expectations for little change have proved to be false—often catastrophically so in economic and/or military terms, for those so complacent. Sometimes improvements simply continued at a steady pace. At other times new modes or new devices were developed to supersede those for which only marginal improvements could be achieved. The lesson should be clear: *Individuals, groups, and nations prosper and develop in concert with progress in transportation. If the United States does not contribute substantially to that progress, it will have to defer to other nations in both the economic and defense realms for the advancements they devise*.

Engineering research directed at transportation can provide the overall framework for significant advances in the future. Although every individual and every organization uses transportation systems, and are therefore informed about (and typically critical of) their shortcomings, few are aware of the complex and pervasive influence transportation systems have on the national welfare. Most people understand that transportation systems facilitate achieving widely accepted societal goals pertaining to the quality of life, to domestic productivity, and to the nation's economic and defense position relative to the rest of the world. It is also widely recognized that limitations in transportation systems hamper achieving those goals.

Less apparent, however, are the far-reaching implications that relaxing or tightening this system of limiting factors may have on seemingly unrelated events. Adjustments in the system bring about a cascade of changes that affect the growth and decline of cities and regions, the expansion or contraction of sectors of the economy, and our ability to function effectively in the world economic system and to defend successfully our national interests in the world political system. The Organization of Petroleum Exporting Countries' oil embargo of 1973 and subsequent price increases are a prime example, leading as they did to a worldwide economic readjustment and major alterations in the direction and fortunes of some of our nation's largest industries. Substantial decreases in oil prices could in the future have equally powerful impacts on the world economy—and not entirely beneficial ones.

Definition of the Field

A transportation system encompasses (1) guideways (or simply "ways"), (2) vehicles (including containers), (3) operations and control, and (4) terminals and other nodes. Often the term "mode" is used to refer to the vehicle (e.g., surface ship, aircraft, or spacecraft) and/or guideway (e.g., highway, railroad, or pipeline). Thus, a transportation system may be modal or intermodal (e.g., truck-rail). Interfaces between modes are included in the system description.

Transportation systems research encompasses not only the tangible components of these systems, but also the software and methodologies governing a system's design, operation, and management to meet the ever-changing needs and desires of our society. Table 1 lists aspects of transportation that are subject to engineering research. Systems engineering, which encompasses both the tangible components and methodological elements of transportation systems, brings public and/or social choices and concerns into play along with the other aspects shown in Table 1.

IMPORTANT OR EMERGING AREAS OF TRANSPORTATION SYSTEMS RESEARCH

Identifying Research Needs in Transportation

Despite the great importance of our transportation system to the nation, as measured by the enormous public investment mentioned earlier, there is little investment in and almost no coordination of the research needed to improve the effectiveness and efficiency of either the overall transportation system or of the individual modes. Indeed, for our highways alone (a $1 trillion system), less than 0.15 percent of the nation's 1982 expenditures on the system went for research. The leverage for benefit from a proportionately small increase in research is very large. One reason that more research is not done, however, is that the payoffs come not to the industry that might pioneer the advance, but to the users of the system. This absence of financial return has limited industrial investments in research.

As a nation, then, we are missing a great opportunity to invest wisely in transportation systems research. What are the best investments we can make in that research? Too often we do not know, because even the studies that would determine the important trade-offs have not been done. A logical starting point, however, is the projection of likely future societal needs with respect to transportation systems. Because these needs will derive

Table 1 Aspects of Transportation Systems Subject to Engineering Research

System	Topic
Vehicles	Propulsion
	Container
	Communications
	Controls
	Safety equipment
	Pollution controls
	Navigational equipment
Guideways	Structures
	Geometrics
	Maintenance equipment
	Safety equipment
	Navigational equipment
Intermodal interfaces	Conveyors
	Parking facilities
	Freight handling equipment
	Terminals (e.g., harbors)
Communication	Vehicle to vehicle
	Vehicle to guideway
	Guideway to vehicle intermode
Network analysis	Equilibrium
	Logistics
	Congestion reduction
System planning and management	Location
	Safety
	Capacity
	Environmental impacts
	Flow relationships
	Mode choices
	Man-machine interface
	Control

primarily from public (including military) expectations and concerns regarding transportation, the panel undertook initially to identify those expectations and concerns shared jointly by the civilian and defense sectors.

Growing Issues in Transportation

Issues that the panel expects to be of major concern in the context of the overall U.S. transportation system in the next 20 years are

- general pressure for more effective transportation (better and more transportation for less money);
- increasing traffic congestion (both ground and air);
- heightened expectations for improved reliability and durability;
- pressure for lower life-cycle costs;
- demands for less negative impacts on the physical environment (air, water, noise, solid-waste, and aesthetic pollution);
- the need for greater public understanding of safety and/or risk management; and
- the reemergence of energy and material resources as a constraint.

Impacts on Research

Viewed another way, these issues highlight public expectations of (1) increased safety and convenience for the available modes of transportation, (2) commercial and industrial needs for lower costs and more reliable and timely delivery of goods, and (3) enormous military logistics needs in times of national emergency. These expectations and needs will not be met satisfactorily in the increasingly crowded future solely on the basis of present knowledge.

Substantial innovations in transportation systems—in the form of both major improvements and new developments—will be required to meet these expectations and concerns effectively. The often-noted unpredictability of major innovations argues strongly for broadly supporting engineering research to provide the storehouse of knowledge on which such breakthroughs can be based. Yet some engineering research areas deserve special emphasis because they are certain to provide part of the background information essential for future transportation systems.

Strategy for Selection of Critical Research Areas

In order to identify a number of important present or emerging areas of research that have a high potential for meeting these needs, the panel examined much of the range of transportation engineering research. In the process, it identified and considered more than 50 specific promising areas of research. To reduce that group to those with the greatest potential impact, the panel applied certain selection criteria.

A broad criterion was that the research be relevant to the civilian sector (i.e., including military-related research with direct civilian applicability). In addition, the panel weighed the candidate areas on the basis of: (1) their perceived importance to the nation, using the list of needs and concerns expressed previously; and (2) the perceived potential for payoff, especially in the medium-to long-term time frame (or at least 10 years away).

Applying these criteria, the panel developed four lists. The first list consists of five cross-modal engineering research areas with transportation-wide applicability. The panel considers these five research areas as being the most critical ones and as having the greatest potential for benefiting the nation. The second list consists of three broad, fundamental research areas of special relevance to transportation but with applications to other fields as well. The third list consists of mode-specific research topics with the potential to provide a breakthrough in various modes of transportation. The fourth list simply notes, by title, research areas of great importance to transportation but that are the primary concern of other panels of the Engineering Research Board.

Beyond the criteria mentioned previously, the panel focused on research needs that are novel, or that represent the possibility of a major, nonmarginal advance. There was a deliberate intent not to include those research opportunities that are obvious extensions of current knowledge, but that are simply not being funded despite their evident potential for near-term payoff. To that end, the panel endorses wholeheartedly the recommendations for near-term research put forth by organizations such as the Transportation Research Board, with respect to highway systems, and the Association of American Railroads, with respect to rail systems.

The results of the research selection process follow.

Selected Research Topics

List 1: Cross-Modal Engineering Research

Transportation Systems Engineering

The term "systems engineering" in the broad sense means an integrated approach to design, manufacturing, operation, and maintenance in the context of societal demand and use. There are great benefits in utility, cost, performance, safety, and efficiency to be gained from such an approach, and research is needed on the integration of those elements in the context of transportation systems.

Systems engineering can also be applied on several less global levels. First, in vehicle engineering, there is the integration of all aspects of on-board performance; the integrated control of the subsystems; and the overall optimization of the vehicle as a system (including its occupants or users), rather than suboptimization of the components. Research at this level must rely particularly on automation of the design process.

A second level involves integrating the interaction of the vehicle with the guideway. (In the case of cars and trucks, this might involve communications, radar-controlled braking, traction control on slippery pavement, navigation aids, guided steering, etc.) Integrated design of guideways, vehicles/containers, and operations/control is the primary research need. Research must include techniques for sensing and processing data on the condition of both the vehicle and guideway, as well as driver acceptance and training requirements.

The interfaces between transportation modes also lie at this second level of integration. Research is needed on planning, designing, and operating more efficient terminals where people and freight are transferred from one mode to another. Not only the terminals but the modes themselves may have to be modified in order to smooth these transfers. However, these are systems engineering problems.

Beyond the focus on the vehicle and the vehicle-guideway and intermodal interfaces, a third level of concern is emerging in the context of transportation systems engineering. Its major focus is on planning, whether in the design of system components such as vehicles or the design of large-scale private or public transportation networks. At this level a myriad of factors come into play: logistics, parts/materials availability and cost, environmental concerns, foreign and domestic competition, potential shifts in demand, the outlook for economic changes, and so on. The research needs in this area are (1) the development of a basis for optimizing planning and decision-making processes; and (2) logistic systems research for operations and maintenance.

In all three levels of systems engineering, the recent tremendous increase in computing power and data storage and advances in electronic communications (all at lower cost) have opened vast new opportunities for research breakthroughs.

Traffic Control

With increasing congestion on guideways in many locations, on the ground and in the air, traffic control is an increasingly critical element of the transportation system from the standpoint of public, commercial, and military use alike.

In the air, the traffic control system is currently being modernized, but the demands keep growing rapidly. Research is needed that will permit strategic control of all aircraft, civil as well as military, in U.S. airspace on a continuous basis. An example is assured military priority depending on emergency need.

On the ground, a primary research need centers on our limited capability to predict and control traffic along urban and suburban arterial street and highway systems. For prediction, research is needed to improve both our understanding and modeling of the travel behavior of users, as well as our ability to model the dynamic network equilibrium of travel behavior versus travel times, costs, and other service characteristics.

Steady-state, multimodal equilibrium models for large-scale networks presently exist, as do simple network design methods. What is needed are operational extensions of these models that are stochastic, dynamic, and applicable to disequilibrium conditions. Advanced, multilevel decision models incorporating traffic signal optimization and operation are also needed. Further research utilizing supercomputer technology is needed in order to solve such models repeatedly for very short time increments in an efficient manner.

One way to control urban and suburban traffic more effectively is to provide more and better information for users. Recent advances in communication technology (cellular telephones are a portent of things to come) promise to provide users, by a variety of means, with accurate and timely information on current and expected traffic conditions. Such information networks could reduce congestion and facilitate the emergency use of roadways. Advanced network modeling could also provide information for use in traffic signal optimization.

Mechanics of Slowly Deteriorating Systems

Transportation systems tend to deteriorate slowly over long lifetimes. Yet our ability to understand how this process occurs, to determine how far it has progressed, and to analyze existing systems (as well as to design new ones) is limited. The Barkhausen Effect and certain modern x-ray techniques can be used to determine the state of stress at or near the surface of components; these techniques are expensive and cumbersome, however. Ultrasonic techniques offer great promise, but are at the beginning stage for interior stress evaluation and the detection of very small flaws. Other methods of experimental mechanics remain to be developed to assess current conditions in the interior of a deteriorated structure or part.

Extensive fundamental engineering research is needed to achieve a better understanding of the mechanics of slowly deteriorating systems and to develop a basis for their nondestructive evaluation. This research should include identifying "almost-failed" systems and the mechanics of that phase of deterioration.

A practical example of potential applications can be seen in the railroad industry, where present methods of inspection lead to the removal of more than 7,000 cracked wheels annually. Nevertheless, about 45 wheels fail every year, out of the 13 million in service. The cost of derailments associated with those 45 failures is about $10 million per year, not considering the threat to human life. Another example of potentially large savings (in this case, to the military) would be the use of appropriate nondestructive evaluation, instead of a conservative estimate of the useful lifetime, for the replacement of aircraft engines. Still another example is the reliable assessment of the safety of older railroad and highway bridges, as well as pipelines, ships, and aircraft. Significant improvements in this area are not likely without research breakthroughs.

Energy Conversion and Pollution Control

The area of energy conversion includes propulsion and, more specifically, combustion. With the exception of the automobile industry, the private sector regards this activity as involving too little return to individual companies to justify private investment. Consequently, there has not been adequate research on energy utilization, conversion, and pollution control processes to ensure a basis for proper future choices regarding fuel conversion options. For example, substantial cost savings are theoretically possible by reverting to the use of coal in locomotives, provided that suitable energy conversion systems with adequate pollution controls are developed.

Combustion of fuel sprays is an area in which a breakthrough would be extremely important to power plant developments in the automotive, aircraft, rail, marine, and space modes of transport. Spray combustion is used in diesel engines and gas turbines; significant advances could not only improve these engines, but might also permit the development of a more efficient gasoline engine (e.g., the stratified-charge engine).

A better understanding of this complex combustion process could reduce pollutant emissions from all liquid-fueled transportation power plants, and might also permit the combustion of less refined fuels at lower cost. New diagnostic techniques (e.g., the laser diagnostic tools developed at the Sandia National Laboratory in Livermore, California) provide a number of new research opportunities in the field of combustion.

Fluid Dynamics of Separated Flows

Computational fluid mechanics from the subsonic to the hypersonic regime, as it can develop with supercomputers advanced enough to permit all body details and flow conditions to be included, will surely bring about appreciable increases in the efficiency of land, water, and air transport. However, at present a major stumbling block in computational fluid dynamics (CFD) is our understanding of the dynamics of separated flows in three dimensions.

Advances in CFD facilitated by breakthroughs in the treatment of flow separation would be applicable to all modes of transportation. In the automotive field, for example, more effective CFD could help reduce aerodynamic drag, reduce wind noise and sensitivity to gusts, and improve visibility (by keeping windows clean). Because it also relates to internal flows in intake manifolds and cylinders, this research could also lead to increases in the specific power and efficiency of engines.

Nonlinear Collapse of Structures

This area of research deals with the energy-absorbing characteristics of structures during crashes. With regard to automobiles, there has been considerable progress in designing structures to absorb energy on impact, thus preserving the integrity of the passenger compartment and "letting the occupants down easy." The aerospace industry is just beginning to address the protection of occupants during an airplane collision or crash of light-to-moderate severity, and thus some of the same sort of technology is now being designed into aircraft.

Research to date has produced good computer models for linear collapse, but considerable experimental inputs to the models are still required with respect to plastic (nonlinear) deformations. There is a clear need for more research in this area; both analytical and experimental effort would result in definite progress.

List 2: Fundamental Engineering Research Areas of Special Relevance to Transportation Systems

Tribology

This area of research offers renewed potential in the context of new materials and technologies. It encompasses friction, lubrication, and wear—crucial factors in the operation of almost all forms of equipment. Friction is a severe constraint on efficiency, yet lubrication to reduce friction and wear is more an art than a science. Tire and wheel traction and braking are two areas in which advances in our understanding could greatly improve efficiency, safety, and comfort. Wear is an important issue with regard to durability, reliability, safety, and (in the aggregate) remarkably high cost—all of which areas are in the public interest and welfare.

Research is urgently needed in wear-life prediction for mechanical components. Effective predictive tools would allow the consideration of wear-life at the design, manufacturing, and the utilization stages of components. Research is also needed in diagnostic techniques appropriate for tribological applications and utilizing advances in electronics and materials.

Computational Fluid and Solid Mechanics

As we mentioned earlier, the advent of supercomputers large and fast enough to include all vehicle/flow parameters in simulations means that computational fluid mechanics/dynamics will be able to greatly reduce drag and increase the efficiency of land, water, and air transport in all regimes of flow. As research establishes the validity of design procedures, it will be feasible (within time and financial constraints) to test and optimize revolutionary new shapes and concepts on the computer. More efficient, effective engines and external vehicle shapes can then be designed and new concepts can be developed. Prototype construction and testing will always be necessary to check and fine-tune the final design, but CFM will eliminate the need to build models to test the conceptual or preliminary external and internal aerodynamic/hydrodynamic design.

Lightweight, safe vehicle and propulsion structural designs will depend equally strongly on computational solid mechanics (CSM), which often requires more computer capability than does fluid mechanics. Adequate representation of material behavior and damage, of material design, and of the treatment and processing of metals, ceramics, polymers, and composites to achieve the desired properties demands substantial research attention. Coupled to more effective methods of nondestructive evaluation, CSM can lead to greatly improved safety, reliability, and economy of vehicles in both the civilian and military sectors. For example, vehicles can be taken out of service for repair prior to an otherwise-unanticipated early failure; alternatively, they can be kept in service when safe despite some observable damage or the expiration of a very conservatively estimated lifetime.

Man-Machine Interface

This branch of bioengineering, sometimes labeled "human factors," is a diverse field that has taken on many new aspects in our computer-oriented world. It includes research on the role of humans in the workplace, job design and task analysis, human performance, and the design of equipment controls for ease of use and safety. Research is needed in this area to increase operational safety and efficiency in all modes of transport, and to obtain higher performance in the operation of ever-more complex systems, while using fewer people with no higher skill levels.

The arrangement of seating and the instrument panels in the cockpits of modern aircraft and spacecraft illustrates our current knowledge and the value of further research. In the railroad industry, physiological measurements of human subjects have yielded sound mathematical models that are being extensively applied in calculating stresses exerted on body joints in the performance of job functions. These data are leading to redesigned workplaces and training programs and, as necessary, the appropriate selection of personnel so as to minimize injuries. In the maritime industry, research on the man-machine interface has led to the introduction of the "automated bridge" and permitted smaller crews to operate larger vessels easily and safely. Continually increasing productivity in the use of more complex and versatile machines and devices in the workplace, whether aboard a transport vehicle or on the factory production line, is required if we are to meet international competition successfully. Research on the man-machine interface can provide some of the needed improvement.

List 3: Mode-specific Research

Aerospace

Areas of research important to aerospace transportation systems include combustion, fluid dynamics of separated flows, computational solid and fluid mechanics, computers and control, manufacturing sciences, composite materials, and structures. Those topics identified below describe several of the specific subareas of research, not explicitly in the purview of other panels, that relate strictly to aerospace systems.

Research needs in the aerospace field encompass both civil and military applications (most of the research is common to both). Any discussion of those needs must include the range from subsonic through supersonic to transatmospheric flight. Transatmospheric flight applies, at present, only to military and government needs, but is likely someday to have significant civil applications. Research needs in "aeronautics" and in "space" increasingly overlap.

Gas Turbine Engines

New materials, advances in microelectronics, and new computational capabilities promise substantial gains in propulsive performance. The application of advances in these technologies to new engine configurations will be essential if we are to maintain our world leadership in gas turbine engines. An example of a future opportunity is the superbypass engine. The best current subsonic aircraft engines have a bypass ratio of about 6. This means that six times as much air flows through the fan and is not heated as flows through the core, burns the fuel, and drives the fan. Major improvements in fuel efficiency (25–35 percent) could be achieved using bypass ratios between 20 and 40. Such machines would have fan blades whose angle is controllable and that have either mechanical or aerodynamic gearing between the compressor and the fan itself. They may have a one-stage or a two-stage fan (with the stages contrarotating in the latter case). There may or may not be a shroud around the fan. Basic research for such configurations leaves much to be desired; and community noise levels may prevent their use altogether unless more research is done.

Laminar Flow Control or Turbulence Suppression

Much of the drag on any aircraft derives from the transition from a stable laminar flow to one that is turbulent. This random, large-scale mixing of the flow near the aircraft's surface greatly increases drag. We are beginning to understand the fundamental mechanisms of these flows, but a great deal more research is necessary for both subsonic and supersonic transport types. Solving problems such as the balance between wing shape and the power required for partial boundary-layer suction would sharply reduce drag and, in the case of supersonic aircraft, would also determine the material used for much of the aircraft's wing structure.

Novel Configurations and the Influence of Structural Design on Aerodynamics

New and novel configurations with a potential for greatly reducing induced drag and for improving structural efficiency need a far better research base. Computational fluid (aero-) dynamics, described previously, will be an essential tool as new materials permit new configurations. As a simple example, the change from metal to fibrous composites in the main-wing torque box structure changes the stiffness radically. Applied to a simple swept-back wing, this allows a greater aspect ratio and thus greater aerodynamic efficiency. However, the effect of a stiffer material on aerodynamic design in the subsonic case will need far more research before we can optimize aerodynamic efficiency.

Interactions Among Components

As we seek higher levels of optimization for both sub-and supersonic aircraft, the interactions among components become more and more important. The body of research on wings, nacelles, bodies, tails, and such considerations as sweep-back, total propulsion efficiency, and total aircraft drag requires much greater attention, particularly in view of new developments in propulsion, structural mechanics, electronics, and aerodynamics.

Transatmospheric Propulsion

The exploitation of space, the improvement of surveillance, and rapid international point-to-point transport will all require a new-generation transatmospheric system. Technology available or partly available today could produce a vehicle that would take off and land from level fields using conventional air-breathing engines, and then make the transition through the atmosphere into space using some other type of propulsion. Operating costs would be a small fraction of those of the Space Shuttle. However, there are fundamental questions as to whether the system can have a single stage, and as to exactly how versatile it can be. These questions all revolve around propulsion and fuels. Fuels such as metastable helium and liquid hydrogen, for example, have been proposed. A great deal more research into fuels and the circumstances of their storage, handling, and burning is needed before we can proceed with any optimized transatmospheric system.

Maritime

Maritime transportation covers several activities that are, in many respects, quite different. They simply use water as a common pathway. Among the most significant of these activities are

- liner express cargo shipping;
- dry-bulk general cargo shipping;
- liquid-bulk cargo shipping;
- naval surface ships;
- submarines;

- port, terminal, and waterway operations; and
- shipbuilding and repair.

Some research for the U.S. Navy and Coast Guard is ongoing, but much more is required for future defense preparedness. In the combined civilian and defense sector on which the panel's deliberation focuses, it is the first three of the abovementioned categories—liner, dry bulk, and liquid bulk shipping in international trade, along with the associated shipbuilding activities—that require the most research at present.

Ports, Terminals, and Domestic Waterways

Maritime transportation is characterized by low cost per ton mile, and by the ability to move very large masses of cargo. The most severe restrictions in the maritime transportation system occur at the interface between land and water transportation. Research is needed to develop more efficient means of transferring millions of tons of cargo though this land/water interface at the least cost and with the least disturbance to the ecology (dredging, for example.)

Cargo Handling Systems

Research on cargo handling and stowage still offers substantial opportunity for improvement through changes in the ship/terminal complex or changes in the form of cargo itself.

Hazardous Cargos

Maritime transportation presents a special problem when moving hazardous cargos (e.g., petroleum products, chemicals, etc.) simply because of the very large volumes that are assembled for shipment. More research is required to assure that this large and growing category of cargo is moved and handled in an efficient and safe manner.

New Ship Forms

The current and conventional ship hull form and propulsion devices are the result of several hundred years of development. There have been efforts to break through conventional limits to design special ships such as submarines, hydrofoils, ground effect machines, and small water-plane area twin-hull craft. There is much room in the cargo spectrum for a vehicle that could go faster than current ships and also carry a sizable cargo. Research is essential to provide the basis for significant advances.

Shipbuilding

Efficient shipbuilding and repair is important to international trade, and is especially important for national defense. Major improvements in productivity have been achieved over the past 20 years through new manufacturing concepts such as zone outfitting, work process lanes, and automation. Significant new improvements are possible through research in the use of robotics and computer-assisted design/manufacturing systems.

Automotive/Highway

In the engineering of automotive highway vehicles (automobiles and trucks, specifically), strong research activities are already ongoing in the private sector; government funding is not generally sought. However, broad fields of research remain unsupported that have great long-range relevance to automotive vehicles (for improvement of product quality, reliability, safety, and industrial competitiveness). These are the cross-modal and fundamental research areas that would benefit transportation generally (discussed under Lists 1 and 2). Specific areas of research that are not adequately supported by industry relate to the guideway for automotive vehicles—that is, streets and highways.

Highway Maintenance

This is a top-priority near-and long-term research area made more pressing as truck sizes and axle loads increase along with the age of the nation's highways. Six specific needs identified in a recent Special Report of the Transportation Research Board* are

1. long-term pavement performance research;
2. asphalt research, including suitable product specifications;
3. research on bridge-life extension (for both old and new bridges);
4. better use of new technologies to support highway maintenance;
5. basic research to improve the use of portland cement in concrete; and

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* Transportation Research Board. *America's Highways: Accelerating the Search for Innovation* (Special Report 202). Report of the Strategic Transportation Research Study: Highways. Washington, DC: National Academy Press, 1984.

6. noncorrosive, nonhazardous alternatives to salt for deicing.

Safety in the Use of Highways

Great increases in tort liability for claims against the highway network mean that the states will examine the safety aspects of highways much more closely than they did in the past. Joint use by vehicles of disparate size, visibility, and maneuverability exacerbates the problem. Topics that require research include

- technological ways to ameliorate the problem of drug and alcohol use by drivers;
- highest safe speed limits, including possible local variations;
- vehicle design/equipment factors such as seat belts, passive/active restraints, and driver resistance to their use;
- the contribution of trucks to the frequency and severity of traffic accidents;
- biomechanics of crash injury as an aid in vehicle design; and
- better techniques for accident investigation and reconstruction.

Highway Productivity

Improvements in transport productivity (i.e., passenger-miles or ton-miles vs. cost) have historically been related to societal progress and development. Research could provide opportunities for even more efficient goods-and people-moving systems and methods than are currently afforded by, for example, restricted-use lanes and triple-bottom trucks.

Railroads

The high capital cost and long lifetime of railroad facilities, rolling stock, and track mitigate against any rapid changes in large segments of the existing system. Yet remarkable changes do occur and must continue if the railroads are to contribute to the needed improvement in the nation's industrial productivity and defense readiness. Many of the essential research areas have already been covered in the cross-modal research list (List 1). Special mention should be made of several specific needs.

Signaling and Communications

The signal communications systems used in railroads in the United States are dictated by local, state, and federal laws and safety regulations. Thus, the technology of 50 years ago is still in service. The result is an inefficient use of the equipment, a loss of flexibility in performing job functions, and higher transportation costs. New technologies suggest the need for research on means of communication, location, and computation affecting rail transportation. An onboard computer in a locomotive, for example, in association with the proper signaling and communication systems and an adequate location system, could (1) monitor the distance between trains, (2) calculate stopping distances, and (3) ensure adequate spacing to permit a safe stop should a train or other obstruction ahead present the threat of a collision. The rail industry is depending on railroad suppliers to provide solutions to this problem, but it is not clear that the necessary engineering research has yet been done to sort out the options and suggest optimum solutions.

Maintenance of Track Structure

A railroad track comprises ballast resting on subgrade, ties resting on ballast, and rails resting on ties. Track maintenance consists of readjusting the ballast to provide a level bed on which the ties can rest; the objective is to avoid substantial geometric discontinuity along the length of each rail and between the two rails. As loads increase, more care has to be given to maintenance; materials and spacings have to be altered, for instance. Yet as the number of loads and trains increases, the amount of time available for effective track maintenance decreases proportionately. Therefore, practices that may be satisfactory under some patterns of traffic density are not acceptable under others. Whereas some research on this problem has been pursued, additional research on track systems and their maintenance is needed.

Evaluation and Inspection

Certain research needs for nondestructive evaluation techniques are specific to railroads, and should be mentioned in this context. First, accumulated longitudinal displacement of rail in response to thermal expansion and the movement of trains over the rail leads to a few instances of "horizontal buckling," that is, significant lateral displacement of a line of track, each year. Second, wheels that go into service with a residual compressive stress in the rim to resist cracking are gradually changed by repeated thermal exposure in braking to a condition of tension. Far too little research has been carried out to permit effective measurement of the state of stress in components such as tracks and wheels.

Pipelines

Pipelines are tubular structures that permit totally enclosed internal transport of bulk solids, fluids, gases, discrete commodities, and (potentially) passengers. As a mode of transport, pipelines are attractive when a stable, high-volume, point-to-point demand exists that provides the economic justification for investing the large amounts of capital required to install the line and its associated propulsion and auxiliary equipment. When the economic conditions are favorable, pipeline transport offers significant advantages, such as: long, low-maintenance operating life; high reliability and safety; low labor costs; and minimal disturbance of the environment.

Pipeline transport is already a substantial mode of freight transportation for liquids and gases, and has a strong potential for expansion in the area of bulk materials and discrete commodities. Slurry pipeline technology (i.e., solids in a liquid medium) is sufficiently mature that industry can be counted on to conduct much of the needed incremental research. The same is true for oil and gas pipelines, whose application is now widespread. There are several areas, however, in which engineering research could bring about major improvements in the productivity of this special and perhaps underutilized mode of transport.

In-Situ Inspection and Refurbishment

The nation has a large investment in existing oil, gas, and water pipelines that (like much of our infrastructure) are aging and deteriorating. In older cities, water and gas lines are often so deteriorated that extensive leakage and frequent ruptures present both safety hazards and large repair costs. The cost of replacing these pipelines is extremely high, especially when other services, structures, and facilities must be disrupted, as in urban areas. Engineering research aimed at determining methods for in-situ evaluation of pipelines, combined with innovative methods of refurbishing or replacing them (e.g., by working inside the pipe) could produce major national savings and reduce both product loss and the hazards associated with pipeline rupture.

Pneumatic Transport of Fluidized Solids

At present, two of the primary technical limitations of bulk solids transport by pipeline are the requirement for a transport liquid such as oil or water, and the energy per ton-mile required to move the mixture. Research aimed at developing feasible concepts for transporting solids in fluidized form using air as the transport fluid could make this approach considerably more attractive, and could have a major economic impact. Specific issues are

- maintaining fluidization along the line without excessive energy loss;
- controlling abrasive wear of the pipe walls;
- trade-offs between particle size and fluidization effectiveness;
- avoidance of dynamic wave phenomena that result in particle condensation and loss of fluidization; and
- total system energy requirement per ton-mile of solid.

Capsule Transport

Freight transport via capsules in pipelines is a new and relatively unexplored means of transportation. Possible applications include feeder systems for bulk materials such as coal, bulk handling systems for offshore loading and unloading over shallow water, and the collection/distribution of goods and bulk materials in cities and urban areas. Although some experimental demonstrations have been mounted, no systematic studies have been conducted that consider alternative configurations, performance in terms of energy and operating costs, initial construction and fabrication, environmental impacts, and the relationship of such a system to other modes. Systems studies that consider pipeline capsule transport as a part of a multimodal feeder, line-haul, and distribution system would help to identify applications, technical characteristics, and areas for subsystem research.

List 4: Broad, Fundamental Research Areas Within the Purview of Other Panels of the Engineering Research Board

Four research areas fall under the domain of other panels:

1. manufacturing sciences;
2. composites and other advanced materials;
3. structures; and
4. computers and control.

POLICY ISSUES ON FEDERAL SUPPORT OF TRANSPORTATION RESEARCH

Transportation in the United States is ubiquitous and diverse, with systems being operated by both public and private entities for both public and private purposes. Ideally, it should be the function of transportation (and the purpose of transportation research) in the civil sector to make substantial contributions to the economic growth of the nation and to the competitiveness of its industries. In the military sector, the function of transportation (and the purpose of transportation research) is to provide substantial support for our defense capability, flexibility, and readiness. *The continuing objectives of federal policy should be (1) to aid in creating the basis for a flexible and adaptable overall transportation system from the standpoint of public and private needs; and (2) to help balance and optimize the numerous and changing elements of that system in terms of whatever efficiencies and benefits can be achieved.*

However, given the diversity of the system and the range of responsibilities that are encountered in transportation, it has proven to be very difficult to establish overriding principles with regard to the role of government in transportation research. Instances of the problem one faces in trying to establish such a role emerge from the most casual inspection of the transportation system.

Modal Differences

Water Transportation of Civilian Commodities, Goods, and People

The water transportation system in the United States is important in view of its low cost and the extensiveness of our navigable waters. In the inland water transportation network, the waterways are construed to be in the public sector. Accordingly, the responsibility for improvement and maintenance has largely been assumed by the public sector. Only recently have serious attempts been made to assign fiscal responsibility to private operators to cover in part the costs of improvement and maintenance.

Research on improvement and maintenance of waterways has been conducted at a relatively low level

The U.S. Army Corps of Engineers and the Bureau of Reclamation have undertaken to do such public sector research as is absolutely necessary to operate the system. Vehicles for use in the inland waterway system have been developed by the private sector with very little application of research and with no point of focus in government for such research, aside from the Coast Guard and some concern with safety.

Coastal and international shipping also involves a mixed set of responsibilities. The vehicles are privately owned. Although they can draw, to a modest extent, on some fundamental research on water resistance, control, corrosion, and fracture problems, there has been only a modest investment in these areas. The development of channels, and in some cases docking and loading facilities, is generally left to the public sector. In addition, the interplay of commercial needs is heavily influenced by local political decisions. *No integrated, national perspective on optimum investments in water transportation has yet been achieved* . We should improve our ability to recognize changes in transportation requirements and to understand the impact of these changes on international waterway systems and port facilities.

Surface Transportation for Civil Purposes

Surface transportation in the United States presents an even more complex picture. The *highway network* is at once the responsibility of the federal and state governments and of local communities. Thus, the development of plans for highway construction, highway maintenance, and highway improvements falls to a wide variety of institutions, with the only degree of coordination being that achieved through federal funding for a part of the infrastructure problem. In the past, this funding has been restricted to new construction. More recently, some federal funds have been applied to maintenance. *Effective research has been limited to those portions of the federal highway budget allocated to the states for planning and research.* Although the funding is small, much of it goes for planning, and the research tends to be very applied. The Transportation Research Board of the National Research Council has played a significant role in establishing priorities for research, and the recently defined Strategic Transportation Research Study (STRS)* holds promise for additional funding for highway programs heretofore not regarded as requiring research.

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* The Strategic Highway Research Program is the highway portion of STRS.

The *vehicles* operating in the highway system are produced by the private sector and are, for the most part, owned by private operators. Research regarding their nature and behavior is strongly influenced by government policy on such matters as emissions, energy or fuel consumption, and safety. For example, half of General Motors Corporation's research activity is in areas covered by government regulation. Restrictions on the sharing of information by industry have in the past made it difficult to achieve an optimum investment of research dollars in areas in which regulation has created the need for innovation. These restrictions have recently been relaxed—a positive policy step. *However, it is now more important than ever for the government to ensure that its regulations are based on valid, accurate technical facts. More research is needed in this area to ensure that regulations meet national objectives of safety and productivity.*

Traffic control for surface transportation lies within the police powers of the states and is shared among the various levels of government—a complicated situation. Amid this decentralization of control, there is only a limited body of research available that would permit movement toward an optimum traffic control system compatible with effective utilization of the traffic lanes and improved safety and fuel efficiency.

In the *railroad freight* network, the right-of-way is owned by the operators of the system. As a result, there is a greater opportunity for developing compatible vehicles and right-of-way systems. That opportunity has not been met as effectively as it could be, and current funding is quite low, although earlier in this decade a larger allocation of industrial and federal research dollars did permit more attention to be given to these interaction processes.

Research on safety issues related to materials performance, track and equipment interaction, dynamic behavior, and other safety matters continues to be a recognized responsibility of government. Yet that funding has been drastically reduced in recent years, and some problems are accorded minimal attention. Technologies developed elsewhere in the economy, including communications and control devices, offer opportunities for technology transfer that are pursued aggressively by the railroad industry. The rail transportation network in the United States does reflect an opportunity for a coordinated research program because of the control of large segments of the system by single operators. *However, it is current public policy to minimize, if not eliminate, all* *governmental research at the federal level on railroad issues not immediately related to safety.*

*Guided moderate-to high-speed rail passenger transportation has been given only sporadic attention in the United States.** *The rail passenger vehicles that move commuters, whether on heavy or light rail services or as a part of conventional railroad lines using their rights-of-way, are not different in essential detail from those used decades ago. There is very little commitment to research on the part of either private operators or the federal government.*

Air Transportation

The history of research on air transportation has been rather different. Aircraft technology is based, to a significant extent, on federally financed research carried out by the National Aeronautics and Space Administration (NASA) (and its predecessor, the National Advisory Committee for Aeronautics), the Air Force, the Navy, and to a lesser extent, the Army. Armed with this publicly financed work on aerodynamics, propulsion systems, navigation and communication systems, and related technical processes, *the airlines recognize that they do not need to support major basic research programs of their own*. All they must do is define, from their commercial perspective, what their technical requirements are.

The airframe and engine manufacturers, who derive substantial income from military procurement programs along with their associated direct research and independent R&D, are readily able to adapt technology from defense and NASA programs to commercial applications. A threat comes, however, from consortia abroad, when direct governmental funding of research and technological development makes Japanese and European manufacturers more competitive than American aerospace companies trying to satisfy a commercial market. Furthermore, the sheer cost of bringing new designs to market has risen to the point that an aircraft manufacturer or engine manufacturer essentially "bets the company" on its ability to command a large enough market share to earn back its applied R&D costs in translating the underlying technology into a competitive airframe or engine. Competitive

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* High-speed ground transportation systems in Japan and Europe continue to attract attention, although none are able to operate without substantial government subsidies. Various U.S. operators are considering the feasibility of adopting these approaches, but they are generally even less economically attractive in the special circumstances of distances and population densities in the United States.

forces have now narrowed the base of commercial suppliers in the United States almost to the vanishing point. Nevertheless, the U.S. aerospace industry is satisfied with the existing arrangement, there appears to be no change in view for government policy on research relevant to commercial needs.

The nation's airways and associated control systems are supported by the federal government, partly for national defense purposes. Major research programs in progress are already looking at next-generation air traffic control systems designed to increase flight density without impairing safety.

Pipelines

Pipelines are a very special form of transportation-and a very successful one. They carry a large percentage of the liquid fuels that are transported, and account for 18 percent of all ton-miles of intercity domestic freight. Pipelines are not receiving significant amounts of research funding. Accordingly, the full range of opportunities for pipelines to compete with other modes of transportation has not been explored (see the section on "Identifying Research Needs in Transportation"). *The use of different transport media, slurries, and capsules for transport of bulk products warrants considerable study to determine whether the role of pipelines in transportation can be expanded.* Serious doubt has been cast on the economic viability of pipelines in some applications, as a result of the high costs of the Alaskan pipeline. Research is needed on materials, construction, and inspection technology to ensure that pipelines can play their proper role in the economy of the future.

Need for a Rethinking of Policies

In summary, it is clear that the government does not allocate equal funding to fundamental or applied research and the development of technology across the various modes of transportation. If a field is obviously and closely related to ongoing, large-scale military procurement needs (e.g., aircraft and spacecraft), it derives great benefits from the associated government-sponsored research and development. If the procurement is sporadic, as it is for naval surface vessels, then government-sponsored research activity is modest, as is any carryover to commercial shipping. If the connection to defense is clear, but critical only under wartime conditions or an imminent threat of war—as it is for the highway, railroad, maritime, and pipeline transportation systems—then research is left primarily to the private sector. Yet the private sector cannot afford to conduct research that addresses defense needs.

As a consequence, the rate of technological advancement differs significantly from one transportation mode to the next, and the United States as a whole does not have the benefit of optimum technology deriving from both private and public investment decisions. Numerous attempts have been made to address the question of a national transportation policy. However, none of these studies has been able to clarify how local governments, states, the federal government, and the private sector can coordinate or combine their research efforts in pursuit of what should be common transportation goals.

The United States surface freight transportation system (truck, rail, and ship) is extremely effective but expensive because of the substantial duplication of services. That duplication must be reexamined in the face of much more aggressive international economic competition on the one hand and our emergency national defense needs on the other.

During a national emergency, the nation's transportation system must be ready to function efficiently in an entirely different way. The tranportation problems that would be posed by such an emergency require research, first to define them, and then to provide the necessary knowledge base for their solution. The delays and inadequacies of previous responses provide ample warning of the need.

With regard to the movement of people, the United States has unparalleled mobility, but at great expense—too great, in the eyes of some. It is not clear, given the growth and redistribution of the population and the increasing cost of energy over time, that the current balance between the private and public transportation of people represents the optimum national investment. Research aimed at exploring options is not adequately supported. Accordingly, present and future decisions will have an ad hoc quality. These decisions will not be systems-oriented, as they must be if efficient and rational choices in the use of resources are to be proposed and then acted on by the public and private sectors.

ISSUES AFFECTING THE HEALTH OF TRANSPORTATION SYSTEMS RESEARCH

Funding

For an activity that accounts for between 20 and 25 percent of the national economy, and that is critical for national defense, transportation systems have remarkably little research backup outside of the aerospace field. Some years ago, a systematic study of the allocation of federal resources to research revealed that about 10 percent of the budget of the national security program was devoted to research. Agencies participating in the national security program include the Department of Defense, that part of the Department of Energy related to nuclear systems, and NASA. The remaining federal government agencies allocated an average of about 1 percent of their budgets to research. Of civilian mission agencies involved in transportation, however, less than 0.5 percent of budgeted funding is for research. According to NSF data, only 2 percent of federal support of engineering research comes from the DOT—about $80 million (or less than half the outlay of the Nuclear Regulatory Commission for engineering research).

Elements of transportation that are predominantly civilian in times of peace have generally been viewed as a private sector concern. As a result, much of the transportation field does not have a healthy research base. The private sector as a whole allocates only about 0.1 percent of its combined budget to research. However, there is a broad range here. Some parts of the private sector allocate 10 percent or more of gross revenues to research, whereas others allocate less than 0.01 percent. This creates disproportionate technical capabilities in various parts of the economy. The same unevenness is reflected in the transportation. Airborne systems, for example, are supported by significant levels of research-both industry-and government-sponsored—on military systems, communications, and other applicable areas. Thus, technology transfer is feasible and can be pursued aggressively by organizations committed to commercial applications. In the very broadest sense, then, research in the aviation and space fields can be considered healthy on the basis of the number of parallel activities being pursued—although, as mentioned earlier, the cost of development is high.

By contrast, there is virtually no federal commitment to guided ground passenger transportation. At the same time, the industry is allocating less than the "standard" 0.1 percent of its total budget to research. Therefore, although there is not a total absence of new ideas in this field, there is insufficient research to permit a fully informed choice among alternative propulsion systems, guidance and control systems, information management and decision-making systems, vehicles, materials, materials fabrications, materials inspection systems, and so on. Indeed, there is too little research funding available to permit either the substantial improvement of existing systems or the development of new ones.

For highway transportation, apart from vehicles, the fraction of dollars allocated to research and development is relatively small. As a consequence, issues of design and construction, fabrication technology, maintenance technology, inspection technology, and control technology are poorly understood. In addition, the technologies are applied by a multitude of government organizations and their contractors. Because of these factors, the practitioners of these arts are not sufficiently exposed to opportunities arising from research to take full advantage even of what is already in place.

This argument applies with equal force to water-borne commercial transportation as distinguished from naval vessels. In maritime transportation there are very few options available to a designer or an operator because of the lack of exploration and aggressive consideration of new ideas and new concepts. The academic community has few opportunities to pursue research in these fields. Thus, the applied R&D community is presented with few ideas for new options. Operators are not able to look at totally different ways of providing necessary services because there are so few applicable research findings.

This situation does not necessarily justify a large increase in federal funding for transportation R&D. *The federal government has not shown itself to be effective in dealing with the interface between R&D in relationship to specific needs of the economy*. There have been failures in both the transit car and bus arenas, in which federal funding for development actually impeded rather than facilitated the delivery of better services by the private sector.

A principal option that should be explored is for changes in tax policy, antitrust controls, and regulatory policies so as to facilitate and encourage private sector investment.

Another major option would be to expand the federal role in supporting fundamental research on which the private sector could build its applied R&D programs. In the 1970s, the DOT had two university research programs, one in the office of the secretary (later in Research and Special Programs Administration) and one in the Urban Mass Transportation Administration. Both programs have since been dismantled. Nevertheless, the DOT is one of the two agencies (along with the NSF) best suited to support fundamental research in transportation, especially at universities.

Changes in tax and other policies should be explored continuously to facilitate and encourage private sector investment in the universities for support of research applicable to transportation systems.

Among federal agency program heads, as well as in Congress, there has been little understanding of the distinctions and interrelations among fundamental engineering research, applied research, development, and the performance and productivity of transportation systems. There has been little attention to medium-and long-range research, and almost no focus on the intermodal questions that have become more crucial as energy costs have risen and foreign competitiveness has intensified. These are precisely the kinds of research that industry is least likely to fund without specific incentives for doing so.

When a component of a transportation system is in the public sector (as it is generally for the aerospace mode), government funding of engineering research should be directed at both fundamental and applied research. For all other components, it should be directed primarily at fundamental research. The NSF should initiate a significant and broad program of fundamental engineering research in transportation. * *The DOT should resume and expand its programs in support of fundamental research across the spectrum of transportation, with universities as the principal performers of this research.*

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* There is already movement within the NSF toward establishing such a program. See, for example, the summary of an NSF Conference on Transportation Research—State of the Art and Research Opportunities; Special Issue, Transportation Research 19A (5/6), 1985, D. E. Boyce, editor.

Role of Universities in Transportation Engineering Research

The total national R&D effort in 1984 for all fields is estimated at $98 billion. Industry, the federal government, universities, and others are estimated to contribute by performance 75, 11, 8, and 6 percent, respectively, to this total. Universities (representing 8 percent of the total R&D effort) are unique in that their primary emphasis is on fundamental research and their primary output, apart from research results, is educated students. (Universities perform about 25 percent of all research and nearly 50 percent of all basic research.*) University research is also generally characterized—in part because of its close coupling with graduate education—as being long term, with an emphasis on originality; it is also relatively inexpensive. In addition, the diverse nature and openness of the university allows a greater cross-fertilization of fundamental research ideas than normally occurs in either industry or government laboratories.

These factors make the universities indispensable in providing much of the basis for advancements in the hardware, software, and methodology of technology-based fields. University research can be uniquely helpful when complex systems such as the transportation network, with its intermodal problems and linkages to other industries, are at issue. However, it is not sufficient to speak only of research. Research results will not lead to improvements in the transportation system unless those results are effectively transmitted to the relevant user organizations. At present, the coupling between universities and operating agencies— whether in the public or private sector—is generally inadequate. There must be accommodations on both sides, in attitudes as well as in practices, if the linkage is to be improved.

Training and Education

An important indicator of the health of research in any engineering field is the production of highly qualified researchers and practicing engineers—specifically, Ph.D. output. The panel attempted to assess Ph.D. output in the transportation field, using published dissertations as a measure. However, definitional inconsistencies in the labeling of degrees and the description of thesis

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* Science Indicators, 1982. National Science Foundation, 1983.

topics made this an impossible task. The difficulty was more pronounced for hardware-oriented Ph.D.s than it was for those concerned with methodological aspects of transportation research. A complicating factor is that almost anyone with a doctoral degree who works in transportation has a degree with another label (e.g., aeronautical, chemical, civil, electrical, and mechanical engineering; computer science; mechanics; or operations research).

On an empirical basis, panel members in industry (e.g., in the automotive industry) report that the demand for recent Ph.D. graduates in every aspect of transportation technology exceeds the supply. Transportation faculty members at universities around the country report that doctoral-degree enrollment in transportation programs—particularly of U.S.-born students—is declining steadily. *It appears likely that there will be a shortfall of suitably trained doctoral researchers and practitioners relative to future demand by universities, government, and industry.*

Declining research support is certainly a major factor here. Current research projects being funded by the federal government (Federal Highway Administration) and by the National Cooperative Highway Research Program are usually so large and require such elaborate proposals that many universities cannot compete successfully for this research. Another important factor is the relatively greater attractiveness to students of other fields at present. The key problem, again, is that for most transportation doctoral students, transportation is an area of specialization in a larger field. These students are presently less inclined to consider transportation as an area in which to specialize. As a result, some "marketing" of the field to graduate students may be needed.

However, with inadequate funding for research, there is little to market except enthusiasm. More than 20 universities have transportation research centers and currently offer graduate degrees (including master's degrees) in transportation.* This is a strong infrastructure for teaching. Yet as enrollment drops, the falling student/faculty ratio is beginning to necessitate a shift of faculty to other areas of engineering. This is not a healthy trend, except to the extent that it demonstrates the flexibility of researchers in this field.

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* From data provided by the Council of University Transportation Centers.

The panel believes that under the current circumstances there is cause for concern about the future of the researcher pool in transportation. An improved outlook for sustained research support could do much to reverse these trends. Thus, *research funding by the government should be "packaged" in sizes suitable for universities with Ph.D. programs in transportation*. Additional NSF funding for fundamental research, recommended earlier, would permit universities to attract larger numbers of highly qualified U.S.-born graduate students.

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APPENDIX A RESPONSES TO THE ENGINEERING RESEARCH BOARD'S REQUEST FOR ASSISTANCE FROM UNIVERSITIES, PROFESSIONAL SOCIETIES, AND FEDERAL AGENCIES AND LABORATORIES

Requests for assistance were sent by the Engineering Research Board to a number of universities, recipients of Presidential Young Investigator Awards, professional societies, and federal agencies and laboratories in order to obtain a broader view of engineering research opportunities, research policy needs, and the health of the research community. Some of the responses included comments on engineering research aspects of transportation systems research; these were reviewed by this panel to aid in its decision-making process. The panel found the responses to be most helpful and wishes that it were possible to individually thank all those who took the time to make their views known. Because that is not practical, we hope nevertheless that this small acknowledgment might convey our gratitude.

Responses on aspects of transportation systems research were received from individuals representing 36 different organizations, listed in Table A-1: 17 universities (including 2 represented by recipients of NSF Presidential Young Investigator Awards), 9 professional organizations, and 10 federal agencies or laboratories. Some comments covered specific aspects of the panel's scope of activities, whereas others provided input on a variety of subjects.

Although most of the responses addressed priority research needs, several respondents did reflect on policy issues. Many of the research needs and issues of policy and health addressed by the respondents were similar to those noted by panel members. The broadened perspective provided by the responses to the survey was most beneficial in the Panel's deliberations.

Table A-1 Organizations Responding to Information Requests Relevant to Transportation Systems Research

UNIVERSITIES
Carnegie-Mellon University
Clarkson University
Georgia Institute of Technology
Northwestern University
Old Dominion University
Oregon State University
Princeton University
Purdue University
State University of New York, Buffalo
University of Hawaii
University of Illinois—Urbana/Champaign
University of Michigan
University of Minnesota
University of Oklahoma
University of Pennsylvania
University of Utah
Washington University at St. Louis
PROFESSIONAL ORGANIZATIONS
American Institute of Aeronautics and Astronautics
American Institute of Chemical Engineers
American Society of Civil Engineers
Society of Mechanical Engineers
Association of American Railroads
American Industrial Research Institute
Institute of Industrial Engineers
Society of Engineering Science, Inc.
Society of Naval Architects and Marine Engineers
AGENCIES AND LABORATORIES
Air Force Institute of Technology
Air Force Office of Scientific Research
Brookhaven National Laboratory
NASA Ames Research Center
NASA Langley Research Center
NASA Lewis Research Center
National Center for Atmospheric Research
Oak Ridge National Laboratory
Sandia National Laboratory

Index