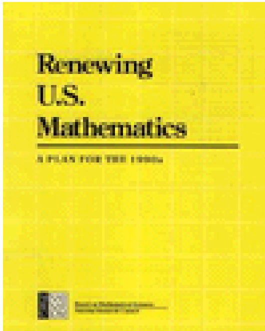
Renewing U.S. Mathematics: A Plan for the 1990s



Committee on the Mathematical Sciences: Status and Future Directions, Board on Mathematical Sciences, National Research Council

ISBN: 0-309-59492-8, 148 pages, 8.5 x 11, (1990)

Renewing U.S. Mathematics

A Plan for the 1990s

Committee on the Mathematical Sciences: Status and Future Directions
Board on Mathematical Sciences
Commission on Physical Sciences, Mathematics, and Applications
National Research Council

NATIONAL ACADEMY PRESS
Washington, D.C. 1990

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.
The National Academy of Sciences is a private, nonprofit, self-perpetuating society of distinguished scholars engaged in scientific and engineering research, dedicated to the furtherance of science and technology and to their use for the general welfare. Upon the authority of the charter granted to it by the Congress in 1863, the Academy has a mandate that requires it to advise the federal government on scientific and technical matters. Dr. Frank Press is president of the National Academy of Sciences.
The National Academy of Engineering was established in 1964, under the charter of the National Academy of Sciences, as a parallel organization of outstanding engineers. It is autonomous in its administration and in the selection of its members, sharing with the National Academy of Sciences the responsibility for advising the federal government. The National Academy of Engineering also sponsors engineering programs aimed at meeting national needs, encourages education and research, and recognizes the superior achievements of engineers. Dr. Robert M. White is president of the National Academy of Engineering.
The Institute of Medicine was established in 1970 by the National Academy of Sciences to secure the services of eminent members of appropriate professions in the examination of policy matters pertaining to the health of the public. The Institute acts under the responsibility given to the National Academy of Sciences by its congressional charter to be an adviser to the federal government and, upon its own initiative, to identify issues of medical care, research, and education. Dr. Samuel O. Thier is president of the Institute of Medicine.
The National Research Council was organized by the National Academy of Sciences in 1916 to associate the broad community of science and technology with the Academy's purposes of furthering knowledge and advising the federal government. Functioning in accordance with general policies determined by the Academy, the Council has become the principal operating agency of both the National Academy of Sciences and the National Academy of Engineering in providing services to the government, the public, and the scientific and engineering communities. The Council is administered jointly by both Academies and the Institute of Medicine. Dr. Frank Press and Dr. Robert M. White are chairman and vice chairman, respectively, of the National Research Council.
Support for this project was provided by the Air Force Office of Scientific Research, the Army Research Office, the Department of Energy, the National Science Foundation, the National Security Agency, and the Office of Naval Research under contract number DMS-8821296.

Library of Congress Catalog Card Number 90-60044
International Standard Book Number 0-309-04228-3
Available from National Academy Press 2101 Constitution Avenue, NW Washington, DC 20418

S109
Printed in the United States of America

Summaries of this report are available from the Board on Mathematical Sciences, National Research Council, 2101 Constitution Avenue, NW, Washington, DC 20418.

NATIONAL RESEARCH COUNCIL

2101 CONSTITUTION AVENUE WASHINGTON, D. C. 20418

OFFICE OF THE CHAIRMAN

This report, Renewing U.S. Mathematics: A Plan for the 1990s, updates the 1984 "David Report," which recommended a national plan to renew and ensure the health of the U.S. mathematical sciences enterprise. The new report, presenting the committee's assessment of progress made since 1984, communicates a sense of promise and achievement, but also the conviction that further corrective action is urgently needed to ensure the vitality of U.S. mathematics.

Substantial progress can be seen in increased federal support for graduate education and postdoctoral researchers, as well as in stronger leadership and improved cohesiveness within the mathematical sciences community. Yet major problems remain: the continuing inadequacies in support for mathematical sciences research, especially for principal investigators; the slow response on the part of many members of the mathematical sciences community to the serious issues of renewal; and the absence of a concerted response by universities to problems clearly described six years ago. The high drop-out rate from mathematical sciences career paths warns that U.S. mathematical research faculties, institutions, and education at all levels must be renewed.

At the same time, this report's presentation of some of the exciting recent achievements in mathematical sciences research, as well as the wealth of opportunities for future research and applications, points to the promise of what is achievable in the mathematical sciences and by extension in U.S. science and technology.

We have an opportunity now to deal with the issues confronting the mathematical sciences community and the nation, especially the challenge of attracting and educating tomorrow's professors and researchers. This report recommends specific actions to address those issues. I commend it to your attention.

Frank Press
Chairman
National Research Council

NATIONAL RESEARCH COUNCIL
COMMISSION ON PHYSICAL SCIENCES, MATHEMATICS, AND APPLICATIONS
2101 Constitution Avenue Washington, D.C. 20418

BOARD ON MATHEMATICAL SCIENCES

April 1990

Dr. Frank Press
Chairman, National Research Council

Dear Frank:

In submitting to you the report of the Committee on the Mathematical Sciences: Status and Future Directions, let me include some comments and observations which are unusual for a letter of transmittal. I do so in hopes that their message will be promulgated and heard, not only in the National Academies but more widely.

Nine years ago you asked me to chair the Ad Hoc Committee on Resources for the Mathematical Sciences, to review the intellectual health of mathematical research in the United States and do an in-depth analysis of federal support for the field. As a communications engineer who could trace his roots back through his thesis advisor Jerry Wiesner to the great mathematician Norbert Wiener, and as a science administrator who had seen first-hand the enormous impact of mathematics and mathematicians, first at Bell Laboratories and later at Exxon Research and Engineering, I was pleased to accept. You gave me a superb committee to do the work.

You are as familiar as I am with what we found: a field brimming with intellectual vitality, preeminent in the world, and poised to make even greater contributions to science and technology, yet a field in which the research infrastructure had eroded, in part because federal support had been allowed to deteriorate. In 1984 the ad hoc committee recommended a coordinated set of actions to be taken by government, universities, and the mathematical community over five to ten years to rebuild the infrastructure and enable mathematics to renew itself.

Halfway through that decade you asked me to chair a different but equally distinguished committee, on behalf of which I am now reporting. We were to assess progress made in implementing the 1984 recommendations. I was happy to accept because I remain vitally concerned with the health of U.S. mathematics. Our report tells what has happened in the five years since we published <u>Renewing U.S. Mathematics: Critical Resource for the Future</u>, and this second report recommends what to do now. Our message is in one sense very simple: balance between support for the mathematical sciences and support for related fields must be restored; stay the course, see it through. We do suggest, however, a modification of the original plan for renewal, tying it more closely to human resource issues and concentrating attention on the pipeline which develops mathematical and scientific talent--issues which loom much larger in the minds of all of us today than they did five years ago. This modification may require some policy changes in federal agencies, drawing their research and educational missions closer together. It will certainly require strong commitment and bold action by the mathematical sciences community, working in conjunction with the research universities.

Let me end with a personal perspective. Overall, I am both pleased and puzzled by progress since 1984--pleased because strong leadership by individuals in government and the mathematical sciences community has brought substantial progress, creating movement toward renewal, yet puzzled by three matters:

1. the general membership of the mathematical sciences community, unlike its leadership, seems only beginning to grasp either the nature or seriousness of its renewal problems, problems that are being compounded by the greatly increased need to attend to revitalization of mathematics education;

2. the lack of concerted action by the research universities or their leaders, either in calling attention to the problems of mathematical research funding or in bolstering their mathematical sciences departments; and

3. the inability of the science policy mechanisms of government to deal decisively with a funding problem as easily soluble and vitally important as the one we pointed out back in 1984.

On the first point, I plan to continue the work I began five years ago, of encouraging the mathematical sciences community to act vigorously, and am happy to see the intense dialogue which is shaping up for 1990 within the community. As former directors of the Office of Science and Technology Policy, we are not very surprised by points 2 and 3. But, for my own part, I remain somewhat distressed by them. Perhaps we can join together in carrying a message to government and the universities. If the mechanisms of science policy cannot solve this critical problem in mathematics, it is doubtful whether they can solve any problem at all.

Sincerely,

Edward E. David, Jr.
Chairman, Committee on the
Mathematical Sciences

COMMITTEE ON THE MATHEMATICAL SCIENCES: STATUS AND FUTURE DIRECTIONS

EDWARD E. DAVID, JR., EED, Inc., *Chair*
CALVIN C. MOORE, University of California at Berkeley, *Vice-Chair*
KENNETH J. ARROW, Stanford University
PETER J. BICKEL, University of California at Berkeley
RICHARD D. DELAUER, Fairchild Space and Defense Corp.
GERALD P. DINNEEN, National Academy of Engineering
CHARLES L. FEFFERMAN, Princeton University
MICHAEL E. FISHER, University of Maryland
KENNETH M. HOFFMAN, Massachusetts Institute of Technology
CATHLEEN S. MORAWETZ, New York University
DAVID A. SANCHEZ, Lehigh University
MORRIS TANENBAUM, AT&T
ROBERT E. TARJAN, Princeton University

Ex-officio Member

PHILLIP A. GRIFFITHS, Duke University
Board on Mathematical Sciences Liaison
GUIDO L. WEISS, Washington University

Staff

LAWRENCE H. COX, Director
DONNA CARTER, Senior Secretary
JO NEVILLE, Administrative Secretary
RUTH E. O'BRIEN, Staff Associate
WILLIAM G. ROSEN, Consultant
JOHN TUCKER, Staff Officer
SCOTT T. WEIDMAN, Senior Staff Officer

BOARD ON MATHEMATICAL SCIENCES

PHILLIP A. GRIFFITHS, Duke University, *Chair*
LAWRENCE D. BROWN, Cornell University
RONALD DOUGLAS, State University of New York-Stony Brook
DAVID EDDY, Duke University
FREDERICK W. GEHRING, University of Michigan
JAMES GLIMM, State University of New York-Stony Brook
WILLIAM JACO, American Mathematical Society
JOSEPH KADANE, Carnegie-Mellon University
GERALD J. LIEBERMAN, Stanford University
ALAN NEWELL, University of Arizona
JEROME SACKS, University of Illinois
GUIDO L. WEISS, Washington University
SHMUEL WINOGRAD, IBM T.J. Watson Research Center

Ex-officio Member

MORRIS DEGROOT, Carnegie-Mellon University (deceased)

Staff

LAWRENCE H. COX, Director
DONNA CARTER, Senior Secretary
JO NEVILLE, Administrative Secretary
RUTH E. O'BRIEN, Staff Associate
WILLIAM G. ROSEN, Consultant
SEYMOUR SELIG, Staff Officer
JOHN TUCKER, Staff Officer
SCOTT T. WEIDMAN, Senior Staff Officer

COMMISSION ON PHYSICAL SCIENCES, MATHEMATICS, AND APPLICATIONS

Acknowledgments

Many individuals in and beyond the mathematical sciences community contributed to this report. The interdisciplinary Committee on the Mathematical Sciences: Status and Future Directions, which developed the ideas that form the writing and core of this report, actively and willingly participated in the creation of it, as is reflected in its finished form. Their names are listed on a previous page. I particularly acknowledge Calvin C. Moore, vice-chair of the committee, for seeking out, selecting among, organizing, and presenting well a diverse collection of research opportunities in the mathematical sciences. For a field that offers so many rich opportunities for the future, this was a challenging task. Thanks are due also to some 40 mathematical scientists (listed in Appendix B) for providing written expert summaries. These summaries provided much of the substance for the section titled "Accomplishments and Opportunities."

The committee had before it a great deal of information that influenced its conclusions. Much of that information is not contained in the report itself since it will appear elsewhere. The committee thanks those who brought it forward, often at considerable effort. Among them are the directors, deputy directors, and staff of the ICEMAP federal agencies, and William G. Rosen and other members of the staff of the Board on Mathematical Sciences. The chairpersons of some 25 university mathematical sciences departments furnished detailed data on their departments as compared to physics, chemistry, biology, and engineering departments. The committee also wishes to thank four chairpersons in particular who, through thoughtful essays, provided valuable glimpses of conditions for fulfilling the research and education goals of the mathematical sciences.

Thanks are due to the National Science Foundation and the other ICEMAP federal agencies for commissioning this study and to the Board on Mathematical Sciences, National Research Council (NRC), for organizing it. The board supplied valuable information and advice concerning both the research opportunities and wider matters, through its chairperson, Phillip A. Griffiths, and participants Guido L. Weiss, Peter J. Bickel, and Cathleen S. Morawetz.

Conscientious and effective support was given to the committee by the board's staff, particularly by its Executive Director, Lawrence H. Cox, and Senior Staff Officer Scott T. Weidman. The attention to detail yet quick response of the NRC offices responsible for the review and production of the report, and of the independent reviewers of the report assigned by the NRC, also deserve mention as constructive influences.

EDWARD E. DAVID, JR.
CHAIRMAN, COMMITTEE ON THE
MATHEMATICAL SCIENCES

Contents

Executive Summary

RENEWING U.S. MATHEMATICS—THE 1984 REPORT

In 1981 a committee of the National Research Council was formed to investigate the health of the mathematical sciences[1] in the United States. Its 1984 report, *Renewing U.S. Mathematics: Critical Resource for the Future* (the "David Report"),[2] found that although the field was thriving intellectually, government support had deteriorated to a dangerously low level. Moreover, the number of young people entering the mathematical sciences had decreased to a level inadequate to replenish the field. In particular, the number of productive mathematical scientists was projected to decline sharply in the 1990s when the current generation of senior researchers retires. This decline was expected to have serious consequences for the nation's scientific and engineering research effort because of the fundamental role of mathematics in the exact sciences. Today a shortage of mathematicians takes on added urgency as we recognize mathematics education as a national priority.

The 1984 Report recommended a plan for renewal, the National Plan for Graduate and Postdoctoral Education in the Mathematical Sciences. That plan's essential feature was a call for funding to bring support for the mathematical sciences into balance with support for the physical sciences and engineering. The interdisciplinary committee that wrote the 1984 Report quickly realized that the low level of research support for U.S. mathematical sciences was so severe that it threatened the vitality of the entire scientific enterprise: the enormous disparity in the number of people supported in the mathematical sci ences vis-à-vis other sciences and engineering meant that support for the overall science and technology base was in poor balance, thus threatening its effectiveness. Solving the problem of inadequate support for the mathematical sciences was therefore necessary as much to restore a healthy balance to the nation's scientific enterprise as to assure a healthy mathematics research capability. Without attempting to define "ideal" balance, the 1984 Report recommended levels of support for research and researcher training in the mathematical sciences that would eliminate at least the gross imbalance documented there. Their actions in response to the 1984 Report make it clear that the mathematical sciences community and the federal funding agencies have accepted the conclusions that an imbalance existed and needed to be countered.

THE CURRENT REPORT—A PLAN FOR THE 1990S

As requested by the National Science Foundation (NSF) and the Inter-agency Committee for Extramural Mathematics Programs (ICEMAP), this report updates the 1984 Report. Specifically, the charge directed the committee to (1) update that report, describing the infrastructure and support for U.S. mathematical sciences research; (2) assess trends and progress over the intervening five years against the recommendations of the 1984 Report; (3) briefly assess the field scientifically and identify significant opportunities for research, including cross-disciplinary collaboration; and (4) make appropriate recommendations designed to ensure that U.S. mathematical sciences research will meet national needs in coming years.

Of the several components of the mathematical sciences community requiring action, its well spring—university research departments—is the primary focus of this report.[3] The progress and promise of research—described in the 1984 Report relative to theoretical development, new applications, and the refining and deepening of old applications—have if anything increased since 1984, making mathematics research ever more valuable to other sciences and technology.

However, although some progress has been made since 1984 in the support for mathematical sciences research, the goals set in the 1984 Report have not been achieved (Table A). Practically all of the increase in funding has gone into building the infrastructure, which had deteriorated badly by 1984. While graduate and postdoctoral research, computer facilities, and new institutes have benefited from increased resources, some of these areas are still undersupported by the standards of other sciences (Table B). And in the area of research support for individual investigators, almost no progress has been made. A critical shortage of qualified mathematical sciences researchers still looms, held at bay for the moment by a large influx of foreign researchers, an uncertain solution in the longer term.

TABLE A Federal Support of Mathematical Sciences Research—Progress Over Five Years, 1984 to 1989

Category of Support	1984 National Plan Goal	1984 level	1989 Level	Percent Change, 16984 to 1989	Percent of 1984 National Plan Goal Reached by 1989
Number of researchers supported					
Senior Investigators	2600	1800	1900	+6	73
Postdoctoral researchers	400[a]	132	188[b]	+42	47
Graduate research assistants	1000	411	661[b]	+61	66
Total dollars (millions)	225[c]	99.6[d]	133	+34	59

[a] The 1984 National Plan calls for awarding 200 two-year postdoctorals annually, resulting in a population of 400 at any given time.
[b] Most recent counts comparable to the 1984 numbers, from fall 1988. (NSF Division of Science Resources Studies, personal communication.)
[c] 1984 National Plan goal adjusted for inflation using Higher Education Price Index; revised National Plan goal is $250 million.
[d] In 1989 dollars, using Higher Education Price Index.

While government has responded substantially to the 1984 Report's recommendations, particularly in the support of infrastructure, the universities generally have not, so that the academic foundations of the mathematical sciences research enterprise are as shaky now as in 1984. The greatest progress has been made in the mathematical sciences community, whose members have shown a growing awareness of the problems confronting their discipline and increased interest in dealing with the problems, particularly in regard to communication with the public and government agencies and involvement in education.

TABLE B Selected Indicators of Imbalance in Research Support

	Chemistry	Physics	Mathematical Sciences
Percent of R&D faculty with federal support, 1987	56	75	37
Number of postdoctorals with federal support, 1988	2587	1280	188
Percent of graduate students with research support, 1987	49	51	18

SOURCES: Faculty percentages from National Research Council, Survey of Doctorate Recipients project (personal communication); other entries from National Science Foundation, Division of Science Resources Studies (personal communication).

In addition to being essential to the continued vitality of U.S. science and technology, addressing the problems of the health and renewal of the U.S. mathematical sciences research enterprise offers the added benefit of contributing directly to solving the critical problems of mathematics education in America. Without the vital nerve center of a healthy and self-renewing research enterprise in university mathematical sciences departments, all the other aspects of our necessary national effort to improve mathematics education will be slowed and inhibited. The need for replenishment of university research faculties is greater and more uncertain in the decade of the 1990s than ever before, and the effects of a deterioration will be felt not only in research production, but also in the educational preparation of scientists and engineers generally, of mathematical scientists and teachers, and of a scientifically and mathematically literate public.

CONCLUSIONS AND RECOMMENDATIONS

I. Implement the 1984 Report's National Plan

Conclusion: Progress has been made in carrying out the 1984 Report's National Plan, but support for the mathematical sciences remains seriously out of balance with that for the other sciences and engineering. The numbers of supported senior investigators, graduate research assistants, and postdoctoral researchers are still seriously out of line with the numbers supported in other sciences of comparable size.

Since 1984 there have been significant increases in support for mathematical sciences graduate student and postdoctoral researchers, but no meaningful increase in the number of senior researchers supported. The numbers of currently supported graduate student and postdoctoral researchers are still far short of the goals set in the 1984 National Plan, and the number of senior researchers supported remains approximately 700 short of the goal.

The National Science Foundation has substantially increased support for the mathematical sciences, as has the Department of Defense, which has established new programs at the Defense Advanced Research Projects Agency and the National Security Agency. The Department of Energy has funded a major effort in computational mathematics. Other government agencies have provided only moral support. The mathematical sciences community has responded actively to the challenges posed in the 1984 National Plan.

Recommendation I: Fully implement the 1984 National Plan while increasing the level of annual federal funding for the mathematical sciences to $250 million from $133 million (in 1989 dollars) over the next three years. The 1984 National Plan's goal of $180 million per year has risen due to inflation to $225 million, to which is added $25 million per year for implementing the second thrust of Recommendation II, below.

II. Improve the Mathematical Sciences Career Path

Conclusion: The rate at which young people enter the mathematical sciences remains inadequate to renew the field.

The attrition rate for students in the mathematical sciences is 50% per year, the highest among all scientific fields. The recent report by W.G. Bowen and J.A. Sosa[4] predicts a severe shortage of mathematical and physical scientists for academic positions during the 1990s. Similar shortages are expected for government and industrial positions. This committee believes that bright young people are discouraged from pursuing careers in the mathematical sciences because they find their career prospects are dim. Mathematical sciences departments in universities provide much less opportunity for research, fewer graduate research assistantships, and far fewer postdoctoral research positions than do other science departments. Students are quick to perceive this.

Corrective action is needed to assure an adequate supply of talented students for the mathematical sciences. Professors must show students that the mathematical sciences offer both intellectual excitement and attractive career prospects. Providing intellectual excitement is the responsibility of the mathematical sciences community. To perceive attractive career prospects, students must see in their own mathematics departments active, successful research enterprises that involve graduate students, young faculty, and senior researchers, all supported at levels competitive with those in the other sciences and engineering.

Recommendation II: Improve the career path in the mathematical sciences. Specifically,

- **The funding called for in Recommendation I should be used to increase the numbers of senior, junior, and postdoctoral researchers, and graduate research assistants supported.** This committee reiterates the 1984 call for annual federal support for 2600 senior investigators, 200 new postdoctoral researchers, 1000 graduate research assistants, and 400 research grants for young investigators.[5] At these levels of support the mathematical sciences would be renewed by an influx of fresh talent, and the nation could realize fully the scientific potential of some 700 first-rate senior researchers who now lack federal support. Finally, funding for the mathematical sciences would be brought into balance with levels of funding for other science and engineering fields. Until the present clear imbalance is countered, students will continue to find the mathematical sciences less attractive than other fields, and renewal of U.S. mathematics will fail.

- **Ten percent of the funding called for in Recommendation I should support coherent programs that directly encourage young people to enter and remain in mathematical sciences careers. Recruitment of women and minorities into the mathematical sciences is a high priority.** The NSF and other government agencies should solicit research proposals for programs that will improve the career path. Such proposals may combine research opportunities for students, postdoctorals, and young faculty with increased support for senior researchers who can act as mentors. Proposals may be submitted by whole departments, faculty groups (possibly with members from different departments), or individuals. Special criteria may be required to judge such proposals.
- **Academic mathematical sciences departments should give increased recognition to faculty who act as mentors for students and junior colleagues, who contribute to education, and who interact with collaborators from other disciplines.** This change would encourage efforts to improve the teaching of mathematics both at the undergraduate and graduate levels.
- **Universities should do more to strengthen their mathematical sciences departments.** Universities should give these departments adequate resources to meet the responsibility of preparing large numbers of science and engineering students while also providing adequate resources for research training in the mathematical sciences.

III. Support a Sufficient Number of Individual Investigators

Conclusion: Mathematical sciences research has been extraordinarily productive over the past five years.

Striking progress has been made along a broad front, from the most abstract branches of core mathematics to computer algorithms for the most practical problems. Appendix B of this report documents some of the main trends. As remarkable as recent mathematical research has been, a substantially greater rate of progress is possible because the mathematical sciences enterprise is running well below its attainable productivity. Most mathematical sciences research is done by some 2600 highly active investigators—a conservative estimate[6] —of whom about 700 are entirely unsupported by federal funds. A substantial fraction of these 700 lack even the two months of uninterrupted research time provided by typical grants. Because these mathematical scientists are highly productive, small investments in support can produce large payoffs in the form of major advances. Since the mathematical sciences have so strong an impact on science and technology, this opportunity should not be wasted.

Recommendation III: Increase to 2600 the number of mathematical sciences senior investigators supported annually. As explained in Conclusion II, this is also a basic step to meeting the problems of renewal. It is the one funding recommendation in the 1984 National Plan on which no meaningful action has been taken. The time has come to take full advantage of the remarkable scientific opportunities offered by the mathematical sciences.

These overarching recommendations suggest particular actions—by federal agencies, universities, department chairs and university administrators, and the mathematical sciences community—that are specified in Chapter 5 of this report.

Underpinning these recommendations, and adding urgency to them, is the belief held by the committee that a vigorous mathematical sciences *research* enterprise is crucially important to the task of upgrading mathematics instruction in our primary and secondary schools and at the collegiate level. As Morris Tanenbaum, Vice-chairman of AT&T and a member of this committee, expressed it:

> Everything you read about our children's education makes you weep because so many of them can't add, subtract, or understand a simple formula. Everything you read says that many of their TEACHERS can't teach them those simple, necessary skills. You ask: "Where do the teachers come from?" The teachers come from undergraduate schools, where they haven't had any significant education in mathematics. Then you ask: "Who teaches the teachers? Who will really teach them from a first-rate point of view?" Their professors must come from good schools that attract and educate people who are truly interested in mathematics. All the information we've seen tells us that the community that produces professors in the mathematical sciences is threatened, and weak, and should be rebuilt.

Mathematics plays an essential role at all levels of the educational process, particularly in science and engineering. We must have excellence at the top to have excellence down the line. Research mathematicians train gradu ate students who go on to teach in the nation's colleges. Mathematics researchers receive their training from active researchers in university research departments. So, too, do most college teachers, whether they become researchers or not. U.S. postsecondary mathematics education and mathematics research are interdependent, and the university department is their nexus. College teachers in turn train the next generation of primary and secondary school mathematics teachers. This process is producing too few teachers who are qualified to teach mathematics. Yet it is at the primary and secondary school levels that students often decide that they can or cannot undertake careers in science or engineering. Today mathematics is too often a barrier that discourages students from making ambitious career choices. This is particularly true for minority and women students. Major initiatives in mathematics of the kind recommended in this report thus play a crucial role in strengthening mathematics education at all levels and hence in assuring that the United States will be internationally competitive. *Indeed, the committee believes that the health and vigor of the mathematical sciences is a vital index in judging the prospects for national attempts to solve the science-based problems of U.S. society.*

NOTES

[1] The discipline known as the mathematical sciences encompasses core (or pure) and applied mathematics, plus statistics and operations research, and extends to highly mathematical areas of other fields such as theoretical computer science. The theoretical branches of many other fields—for instance, biology, ecology, engineering, economics—merge seamlessly with the mathematical sciences.

[2] National Research Council, *Renewing U.S. Mathematics: Critical Resource for the Future* (National Academy Press, Washington, D.C., 1984).

[3] *Additional reports on critical issues in the mathematical sciences are forthcoming from the Mathematical Sciences in the Year 2000 Committee, whose final report late in 1990 will detail many crucial recruitment and education reforms needed into the twenty-first century.*

[4] Bowen, W.G., and Sosa, J.A., *Prospects for Faculty in the Arts & Sciences* (Princeton University Press, Princeton, N.J., 1989).

[5] These goals, developed in the 1984 Report, pp. 57–65, were examined by the committee and found to be still valid today.

[6] National Research Council, *Renewing U.S. Mathematics*, 1984, pp. 61–64.

1

Background and Introduction

THE SITUATION IN 1984

In the early 1980s the Office of Mathematical Sciences of the National Research Council (NRC), chaired by William Browder, presented to the Assembly of Mathematica1 and Physical Sciences[1] of the NRC startling evidence suggesting the deterioration of federal support for mathematical sciences[2] research in the United States. Because of the critical dependence of science and technology on continued generation of new mathematical methods and concepts, the Ad Hoc Committee on Resources for the Mathematical Sciences was established by the NRC to review the health and support of the field. This panel of scientists, engineers, and mathematicians was asked in particular to determine whether federal and/or university support had in fact deteriorated and, if so, how this had come about and what should be done about it to provide for the future health of the discipline.

After three years of investigation and analysis, the ad hoc committee presented its findings and recommendations in *Renewing U.S. Mathematics: Critical Resource for the Future* (the "David Report"; National Academy Press, Washington, D.C., 1984), referred to herein as the 1984 Report. It told a story that was deeply disturbing to both practitioners and policymakers in science:

- *Federal support for mathematical sciences research had come to be markedly out of balance with support for related fields of science and engineering. Discrepancies in support for essential research needs were very large*. The 1984 Report summarized that committee's estimate of the number of funded senior investigators needed in the mathematical sciences as follows (p. 64):

> Apply to the mathematics faculty the lowest percentage for those with federal support in other fields, 54% [from a 1980 National Science Foundation report]. One obtains 2400 as a base figure for the number of mathematicians to support. The mathematical sciences faculty is 1.3 times the size of that in mathematics, suggesting that ... 3100 is about right for the number of mathematical science faculty members on grants. From this, subtract 400 young investigators (Ph.D. age three to five years), to obtain 2700 as an appropriate number of established investigators.... Guido Weiss of our Committee surveyed chairmen of mathematical science departments nationally, asking them to examine their faculties and judge how many researchers without support were doing research of the quality done by those with support. Extrapolation from the responses led to the estimate 260$1$2$3900 for the total of "supported" plus "equally qualified." ... we adopt 2600 as the threshold level for the number of established investigators to support.

Goals given in the 1984 Report for other categories of support were estimates of the numbers of young people needed at each stage in the mathematical sciences pipeline in order to replenish this necessary core of 2600 senior researchers at the rate of some 100 per year.

- *This situation had come about through a combination of (1) abrupt losses of support for mathematics in the five-year period from 1968 to 1973 caused by shifts in federal policy (e.g., the Mansfield Amendment, fellowship cutbacks), and (2) steady deterioration of support over the decade 1973 to 1983*, during which the growth of computer science as a discipline and the practice of lumping this field together with mathematics in aggregate federal research data masked the deterioration of funding for the mathematical sciences.
- *The infrastructure supporting the mathematical research enterprise had been seriously weakened*, especially in university mathematical sciences departments, which contained 90% of the mathematical researchers, with the result that the field was in serious danger of being unable to renew itself.
- *This weakening had also gone largely unnoticed*, for two closely related reasons: (1) the mathematical sciences community did not bring its growing problems to the attention of the broader scientific community until the early 1980s; and (2) the spectacular performance of American mathematics, which had risen to a position of world leadership in the decades immediately following World War II, continued unabated

throughout the 1970s, relying heavily on creative talent developed and incorporated into the field before the deterioration began to take its toll.

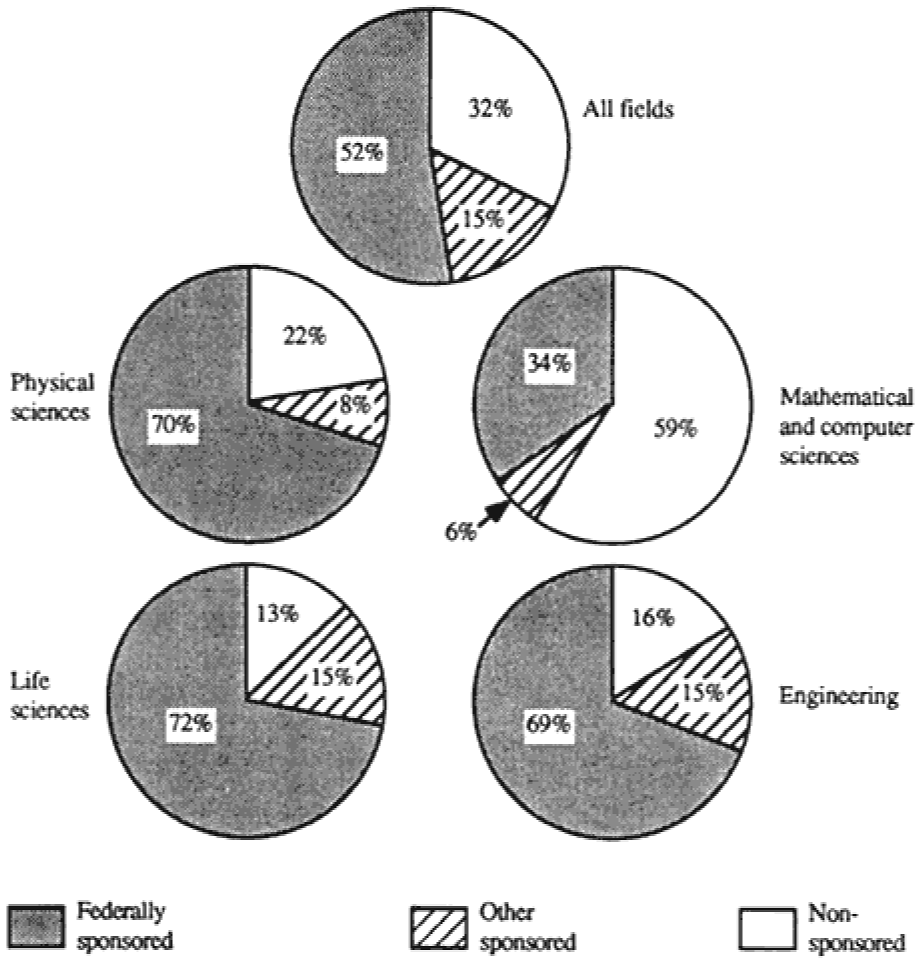


Figure 1.1
Research time in universities, November 1978 to October 1979.
Source: From National Science Foundation Report 81-323, reprinted from National Research Council, *Renewing U.S. Mathematics: Critical Resource for the Future* (National Academy Press, Washington, D.C., 1984), p. 32.

The conclusions of the 1984 Report were supported by data such as that given in Figures 1.1 and 1.2 and in Table 1.1, which are reprinted here from that report. These data document the imbalances in support experienced at that time by the U.S. university mathematics sector.

Then, as now, the academic research communities of mathematics, chemistry, and physics were of comparable size. However, the numbers of active mathematical scientists and trainees who were supported, and thus encouraged to perform or learn to perform research, were strikingly out of balance with the numbers supported in chemistry and in physics. The low ratios of graduate research assistants per mathematical researcher and postdoctoral researchers per mathematical researcher pointed to a great many missed opportunities for better training of young people.

In fact, there were so few research grants in mathematics that many qualified researchers were without support, while the level of support for graduate students and postdoctoral researchers was so low that the mathematics Ph.D. pipeline suffered both in quality and quantity. There was clear evidence that the field was not renewing itself and therefore legitimate concern that research progress would diminish in the future. The prospect of becoming a professional mathematician had begun to look less and less inviting to students.

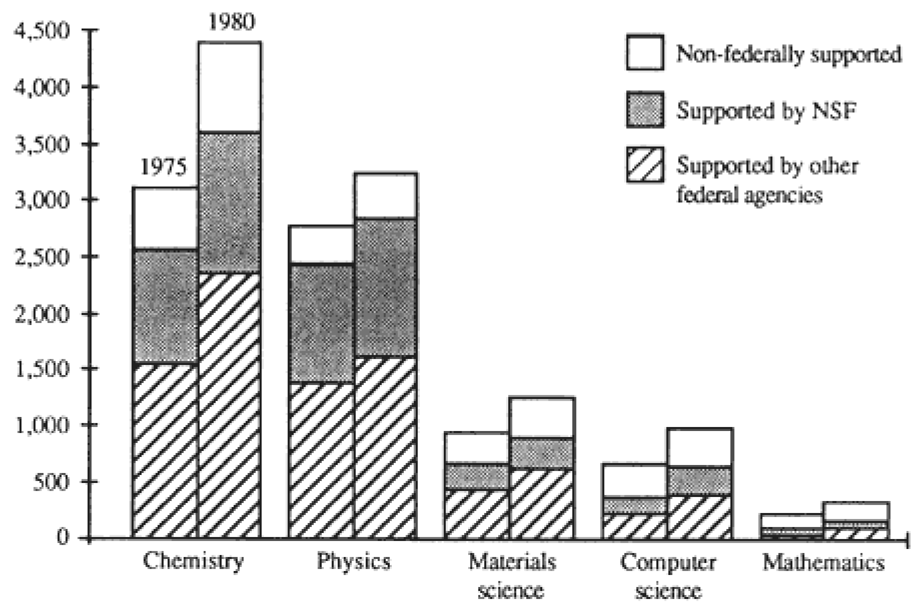


Figure 1.2
Graduate students with research assistantships enrolled full-time in doctorate-granting institutions, mathematical and physical sciences.
Source: From National Science Foundation Report 82–260, reprinted from National Research Council, *Renewing U.S. Mathematics: Critical Resource for the Future* (National Academy Press, Washington, D.C., 1984),p. 33.

TABLE 1.1 Postdoctorals in Graduate Institutions, 1981

	Total	Federally Supported	Non-Federally Supported
Chemistry	2870	2465	405
Physics	1450	1217	233
Mathematical Sciences*	99	56	43

* This number excludes about 75 university-sponsored "research instructors" in mathematics.
SOURCE: National Science Foundation; reprinted from National Research Council, *Renewing U.S. Mathematics. Critical Resource for the Future* (National Academy Press, Washington, D.C., 1984), p. 33.

This situation developed during the 1970s, largely due to causes mentioned above. Rather than arguing for increasing Ph.D. production and hiring, mathematical sciences departments adapted to increased teaching responsibilities by expanding the use of graduate teaching assistants, thus freeing up some faculty time for research, but at the expense of providing quality research training for those entering the field.

The noteworthy productivity of mathematics researchers in 1984, as now, was at least partly due to a trio of singular events: the emergence of mathematics tools from World War II, the post-Sputnik alarms, and the ongoing expansion of the field as computer use widened the demand for mathematics. As the first two of those stimuli fade, the intellectual momentum they sparked is certain to run out. Just as today's mathematicians are prolific due in part to events and support levels of decades ago, tomorrow's mathematics will suffer because of the more recent underfunding and concomitant fading of career opportunities.

IMBALANCE IN SUPPORT FOR RESEARCH

The interdisciplinary committee that wrote the 1984 Report quickly realized that problems resulting from insufficient research support for U.S. mathematical sciences were so severe that they threatened the entire scientific enterprise. Due to the enormous disparity in the number of people supported in the mathematical sciences vis-á-vis other sciences and engineering, the overall science and technology base was in poor balance, thus threatening its effectiveness. Revers ing the underfunding of the mathematical sciences was therefore necessary as much to restore a healthy balance to the overall scientific enterprise as to assure a healthy mathematics research capability. The 1984 committee concluded that the extreme imbalance in the numbers of junior people involved in research, while partly attributable to the differing needs of laboratory and nonlaboratory fields, nonetheless indicated a distinct shortcoming in mathematical sciences researcher training. Without attempting to define "ideal" balance, that group was able to make recommendations for levels of supported research designed to restore balance. The actions of the mathematical sciences community and the federal funding agencies in response to the 1984 Report make it clear that they have accepted the conclusions that an imbalance existed and needed to be countered.

Balance does not necessarily imply funding parity, nor the achievement of equity between fields; rather, it implies supporting each field of science to whatever degree is required to keep it and the totality of science functioning efficiently. The 1984 committee saw the science and engineering disciplines as an ecosystem: while the components have different needs and roles, they must all function in a balanced way for the system as a whole to thrive.

The present committee agrees with this analysis and believes that elimination of the imbalance documented clearly in the 1984 Report—and still present, as demonstrated by Table B (Executive Summary) and Tables 2.3 and 2.4 (Chapter 2) of this report—is still the most pressing need of the mathematical sciences as a field. Since the arguments and underlying premises of the 1984 Report were widely accepted, the present report does not reargue that case but instead refers the reader to the Executive Summary of the 1984 Report, reprinted in this report as Appendix A.

THE 1984 NATIONAL PLAN

The 1984 Report challenged the Administration and the Congress, the universities, and the mathematical sciences community to implement, through a decade or more of sustained effort, a national plan for renewing the mathematical research enterprise in the United States. The seven principal elements of this 1984 National Plan for Graduate and Postdoctoral Education in the Mathematical Sciences—discussed in Section IV of Appendix A —called for the following:

- *Restoring* a reasonable degree of balance between federal support for mathematical sciences research and support for related fields by increasing mathematics support from $79 million to $180 million per year over a five-year period (figures in 1984 dollars);
- *Restructuring* the general pattern of use of resources, once they were made available to the mathematical sciences, by moving away from a pattern of small research grants supporting only principal investigators, which was especially prominent at the National Science Foundation (NSF), and toward a grants model consistently supporting graduate students, postdoctorals, and other components of the research infrastructure as well.[3] Briefly, the 1984 National Plan called for annual summer support for 2600 senior investigators, 24-month research positions for 200 postdoctorals, 15 months plus two summers of research support for 1000 graduate students, and 400 research grants for young investigators. These goals were based on the premise that more young mathematical scientists need thorough training with mentors.
- *Reducing* the unusual dependency of the mathematical sciences on the NSF and the service agencies of the Department of Defense (DOD) by fostering development of new mathematics programs at other agencies, especially programs concerned with research having long-term payoffs;
- *Extending* the lines of contact and support outward from the mathematical sciences departments to business and industry;
- *Initiating* within research universities in-depth reviews of the health of their mathematical sciences departments, focusing on the working circumstances of their faculties, the relationship of federal support to university support, and the widespread university practice of justifying allocations to mathematics departments solely on the basis of the department's instructional role;
- *Developing* within the mathematical sciences community a greater sense of responsibility for its own fate and a greater unity of purpose and action, drawing together professional organizations from across the varied subdisciplines to act in concert in (1) presenting regularly to government and universities the research needs of the field; (2) creating a long-term, coordinated public information effort

aimed at increasing understanding of the roles and the importance of mathematics in science, technology, and culture; and (3) accelerating the efforts of mathematical scientists to attract brilliant young people into their field; and finally

- *Expanding* the mathematical sciences community's commitment to and involvement in the revitalization of mathematics education, with special attention to the precollege level.

The present committee reconsidered the 1984 National Plan and determined that its goals remain valid and necessary today.

Complete implementation of the 1984 National Plan would assure the continued replenishment of the field's personnel base with talented new scientists. This, in turn, would assure continued intellectual production by the discipline. This intellectual output is valuable in itself, contributes substantially to other quantitative fields, provides the environment necessary for training mathematical scientists and educators, and—when explained well—serves as a beacon to draw students into the mathematical sciences.

THE CURRENT REPORT

Purpose and Emphasis

This report was prepared at the request of the NSF and the Interagency Committee for Extramural Mathematics Programs (ICEMAP). Specifically, this committee was charged to (1) update the 1984 Report, describing the infrastructure and support for U.S. mathematical sciences research; (2) assess trends and progress over the intervening five years against the recommendations of the 1984 Report; (3) briefly assess the field scientifically and identify significant opportunities for research, including cross-disciplinary collaboration; and (4) make appropriate recommendations designed to ensure that U.S. mathematical sciences research will meet national needs in coming years.

While recognizing that many critical issues face the mathematical sciences—especially demographic and educational ones—this report focuses on university research departments. These are the intellectual wellsprings of the field and the source of many teachers who set the pace for educational progress. By so focusing, this report comple ments other recent and ongoing efforts within the mathematical sciences community.[4]

Definition of the Mathematical Sciences

The discipline known as the mathematical sciences encompasses core (or pure) and applied mathematics, plus statistics and operations research, and extends to highly mathematical areas of other fields such as theoretical computer science. The theoretical branches of many other fields—for instance, biology, ecology, engineering, economics—merge seamlessly with the mathematical sciences.

This intellectual definition does not correspond exactly to the administrative definitions under which data are collected. Most data included in this report adhere to the NSF definition of mathematical sciences, which is somewhat more restrictive than the intellectual definition given above. By using these data, the committee sought to maintain continuity with the 1984 Report, being confident that trends and conclusions would not be skewed by such a small mismatch in the basis. The research progress reports included in Appendix B were deliberately chosen to span the broader definition of the field.

NOTES

[1] The Assembly of Mathematical and Physical Sciences at the NRC subsequently evolved into the newly constituted Commission on Physical Sciences, Mathematics, and Applications.

[2] See below, section headed "Definition of the Mathematical Sciences."

[3] The specific issue addressed in this part of the 1984 National Plan was not individual investigators versus group research. Rather it was grants that support only the research time of principal investigators versus grants that do that and also support graduate students, postdoctorals, and so on. In the early 1980s the average NSF research grant in mathematics supported two months of summer research time for a principal investigator and little else.

[4] For example, the National Research Council reports *Everybody Counts: A Report to the Nation on the Future of Mathematics Education* (National Academy Press, Washington, D.C., 1989) and *A Challenge of Numbers: People in the Mathematical Sciences* (National Academy Press, Washington, D.C., 1990).

2

Response to the 1984 National Plan

FEDERAL RESPONSE

The responses of science-intensive federal agencies to the recommendations of the 1984 Report have been highly variable, ranging from bold and sustained action in some cases to moral support with virtually no action in others. The two major agencies most directly involved are the NSF, which accounts for 55% of the total federal support for the mathematical sciences and more than 90% of the support for pure mathematics, and the DOD, which provides nearly 40% of the total federal support for the mathematical sciences and 65% of the support for applied mathematics and statistics.

Nearly two years before its 1984 Report was published, the Ad Hoc Committee on Resources for the Mathematical Sciences had gathered comprehensive data comparing federal support of mathematical sciences research with support for related fields. These data were used as part of the research briefing on mathematics given in October 1982 to George A. Keyworth, director of the Office of Science and Technology Policy (OSTP), by a panel of the Committee on Science, Engineering, and Public Policy (COSEPUP) of the National Academy of Sciences. The data had been developed under the aegis of the government's Interagency Committee for Extramural Mathematics Programs (ICEMAP) and were used extensively throughout the years 1982 to 1986 in staff presentations within several of the agencies, particularly NSF. The early response to these data by OSTP and NSF was swift, resulting in a 20% increase in the budget of NSF's Division of Mathematical Sciences[1] from FY 1983 to FY 1984.

Total federal agency academic support for the mathematical sciences over the five years since the 1984 Report's appearance is profiled in constant dollars in Table 2.1. (Some federal agencies also support significant internal research in the mathematical sciences, which is not included in Table 2.1.)

National Science Foundation

By the start of the 1980s, the Division of Mathematical Sciences at the NSF had already begun an effort to better support the mathematical sciences research infrastructure as well as research per se. A new postdoctoral program was initiated after discussions in the National Science Board, led by G. D. Mostow and others. Intensive debate over proposed "research institutes" led eventually to the funding of the Mathematical Sciences Research Institute (MSRI) at the University of California-Berkeley and the Institute for Mathematics and its Applications (IMA) at the University of Minnesota, both now regarded as highly successful. The debate also led to emphasis on "alternative modes," which encompassed several other programs, notably an expanded postdoctoral program.

Following the June 1984 publication of the National Plan in the 1984 Report, a strong NSF response was sustained. In November 1984, the National Science Board, the governing body of the NSF, passed a resolution calling on all federal science agencies to join with NSF in remedying the marked imbalance in support that the report had pointed out. Strong leadership by Erich Bloch, NSF director, his predecessor Edward Knapp, and Richard Nicholson, associate director for the Directorate for Mathematical and Physical Sciences, resulted in continued high priority for mathematics in the allocation of its funds. As a result, NSF support of mathematical sciences research nearly doubled (almost 50% real growth compared to 29% for total NSF R&D) over the six years from FY 1983 to FY 1989. Equally important was the way these funds were used. With strong backing from their advisory panels, three successive directors of the Division of Mathematical Sciences, E. F. Infante, John Polking, and Judith Sunley, led an effort to restructure the support patterns of NSF mathematics programs, balancing support for the research infrastructure (as represented by graduate students, postdoctoral researchers, computing facilities, and so on) with research support for principal investigators.

This ordering of priorities continues to be recommended by the NSF's Mathematical Sciences Advisory Panel. Thus virtually all of the real

TABLE 2.1 Federal Agency Academic Support for the Mathematical Sciences, Constant 1984 Dollars (Millions)

Agency	FY 1984	FY 1986	FY 1988	FY 1989	Percent Increase, FY 1984 to FY 1989
Department of Defense					
AFOSR	10.20	11.94	11.81	13.04	28
ARO	6.80	7.54	8.76	9.11	34
ONR	11.90	11.50	9.50	9.53	-20
DARPA	-	4.94	9.91	7.13	N.A.
NSA	-	-	1.60	2.04	N.A.
Total DOD	28.90	35.92	41.58	40.85	41
Department of Energy	2.90	3.54	5.48	5.43	87
Other agencies*	2.00	1.80	0.83	0.79	-61
Total non-NSF	33.80	41.26	47.89	47.07	39
NSF					
DMS	41.20	46.45	52.67	52.20	27
Other	4.00	4.94	4.54	6.34	59
Total NSF	45.20	51.39	57.21	58.54	30
TOTAL FEDERAL ACADEMIC SUPPORT	79.00	92.65	05.1	105.61 34	

* Estimate, including the National Aeronautics and Space Administration, the National Institutes of Health, and the National Institute of Standards and Technology. Also includes NSA prior to FY 1988.
Acronym key: AFOSR = Air Force Office of Scientific Research, ARO = Army Research Office, ONR = Office of Naval Research, DARPA = Defense Advanced Research Projects Agency, NSA = National Security Agency, NSF = National Science Foundation, DMS = Division of Mathematical Sciences.
N.A. = Not appropriate.
SOURCE: FY 1990 Joint Policy Board for Mathematics submission to the American Association for the Advancement of Science (AAAS). Higher Education Price Index used as dollar deflator.

growth in the division's budget over the last six years has gone into infrastructure support.

These priorities, consistent with the thrust of the 1984 National Plan, have had a significant impact on the Ph.D. pipeline. An undesired side effect is that, because of funding limitations, no progress has been made on another important part of the plan: increasing the number of principal investigators supported. It should also be noted that, although the profile of grants in the Division of Mathematical Sciences has changed for the better because of the consistent application of these priorities, balance within the grants to the extent recommended by the 1984 National Plan has not yet been attained. Thus future priorities will need to emphasize both principal investigators and the infrastructure for support.

Department of Defense

The percentage growth of total DOD support for the mathematical sciences has been nearly the same as that at NSF, 94% over the six-year period from FY 1983 to FY 1989 (about 46% real growth compared to a 23% increase in the total DOD R&D budget). The form of the growth has been quite different, however.

The three service agencies—the Air Force Office of Scientific Research (AFOSR), the Army Research Office (ARO), and the Office of Naval Research (ONR)—have been in a period of flat funding and have not evolved comprehensive plans for strengthening their support of the mathematical sciences. However, when the DOD's University Research Initiative program was launched in 1986, high priority was assigned to mathematics by the directors of the service agencies and the civilian R&D management of DOD, notably Undersecretary Richard DeLauer and Deputy Undersecretary Ronald Kerber. Still, there has been no net increase in funding except in support of computational facilities.

Support at DOD has grown principally because two new mathematical sciences research programs were created, one at the Defense Advanced Research Projects Agency (DARPA) and the other at the National Security Agency (NSA).

The new DARPA program in Applied and Computational Mathematics, currently funded at the $9 million level (7% of total federal sup port), was initiated by Craig Fields, now DARPA's director. It has emphasized relatively large ($500,000 to $2 million) project grants to groups of mathematicians working on broad problems in areas identified by DARPA staff as (1) important to the DOD and/or DARPA mission; and (2) particularly promising scientifically. Originally, the principal areas were dynamical systems, turbulent flow, computational algorithms, data/image compression, and harmonic analysis/clustering algorithms. The contracts have greatly strengthened the research efforts in those areas and have provided exciting models of problem-focused group (or team) research. This program has generated increased appreciation of the importance of mathematics to the DOD mission well beyond the boundaries of DARPA. At the same time the DARPA program has caused some concern within the mathematical community for two reasons: (1) the program has tended to provide more support for a few mathematicians who were already well supported, and therefore has done little to increase the number of people supported in the field; and (2) initially the program has remained independent of the broader mathematical community.

With 600 mathematicians on its staff, the NSA is one of the largest employers of mathematicians in the country. Though still producing large amounts of classified research, mathematicians at NSA—backed by successive directors W.E. Odom and W.O. Studeman and Chief Scientist Kermith Speierman—have broadened their interaction with the larger mathematical sciences community. The extramural Mathematical Sciences Program at the NSA has had a positive impact on federal support for the field far greater than its size ($2.5 million, or 2% of total federal support) might indicate. It has established a program of "small science" research grants that support some 80 mathematicians, most of whom work in areas traditionally labeled as pure mathematics. Awards are made subject to a peer review system organized by the NRC's Board on Mathematical Sciences (BMS). The program has helped to increase somewhat the number of principal investigators supported in the field. The building of both formal and informal bonds with the mathematical sciences community is also a significant benefit.

Department of Energy

The Department of Energy (DOE) doubled its support for the mathematical sciences over the period from FY 1984 to FY 1988, focusing primarily on computational sciences and applied mathematics but also on geometry and mathematical physics. Its budget for supercomputing quadrupled over the same period. A postdoctoral fellowship program begun in 1989 supports 14 computational mathematicians at national laboratories; this program promises to strengthen ties between academic mathematicians and the DOE laboratories.

Mathematics of Computation Initiative

The 1984 Report's recommendation for a special mathematics of computation initiative—resonating with the recommendations of earlier reports, such as the "Lax Report"[2] —was implemented to a large degree. This initiative was meant to encourage mathematics graduate students and new faculty to develop the new mathematics that will be needed to effectively use the many supercomputers now in use or planned. An NSF summary of federal funding for computational mathematics research shows an increase from $4 million to $12 million over the period from 1982 to 1987.[3] In particular, the NSF created a new program in computational mathematics—with FY 1987 expenditures of nearly $3 million—to focus attention on this goal. The AFOSR and DOE doubled their budgets in this area, while the ARO increased its effort by 50%. DARPA's new program, which did not exist in 1982, supported $1 million worth of computational mathematics in FY 1987. In addition, the NSF supercomputing centers are major resources for this endeavor. Clearly, a good infrastructure for support of this initiative has been established.

Federal Progress Toward Achieving Quantitative Goals of the 1984 National Plan

The goals for federal support set forth in the 1984 Report are the quantitative elements of its National Plan for renewal. These estimates of what it would take to restore balance between support for mathematical sciences research and support for related fields were derived from an analysis of the needs of the inner core of the research enterprise in the mathematical sciences. This core was determined[4] to consist of 2600 senior investigators[5] and a renewal pipeline of 1000 graduate students, 200 postdoctorals, and 400 young investigators. Section IV.B of Appendix A adds details to these goals. The annual cost of support in 1989 was estimated to total $225 million.[6]

Table 2.2 summarizes gains made in four key categories over the five years since the 1984 Report was published. Several comments are in order about increases in total support for the field. In actual dollars, total federal support has climbed from $79 million in 1984 to $133 million in 1989, an increase of 68% over the five years. As described above, support by the two major funders, NSF and DOD, increased by roughly the same percentage but through different mechanisms—at NSF through consistent increases in the budget of its Division of Mathematical Sciences and at DOD principally through the creation of two new programs, one at DARPA and the other at NSA.

TABLE 2.2 Federal Support of Mathematical Sciences Research—Progress Over Five Years, 1984 to 1989

Category of Support	1984 National Plan Goal	1984 Level	1989 Level	Percent Change, 1984 to 1989	Percent of 1984 National Plan Goal Reached by 1989
Number of researchers supported					
Senior investigators	2600	1800	1900	+6	73
Postdoctoral researchers	400[a]	132	188[b]	+42	47
Graduate research assistants	1000	411	661[b]	+61	66
Total dollars (millions)	225[c]	99.6[d]	133	+34	59

[a] The 1984 National Plan calls for awarding 200 two-year postdoctorals annually, resulting in a population of 400 at any given time.
[b] Most recent counts comparable to the 1984 numbers, from fall 1988. (NSF Division of Science Resources Studies, personal communication.)
[c] 1984 National Plan goal adjusted for inflation using Higher Education Price Index; revised National Plan goal is $250 million.
[d] In 1989 dollars, using Higher Education Price Index.

As Table 2.2 shows, the actual dollar increase of 68% translates into real growth of about 34%, most of which has gone into support for graduate students and postdoctorals,[7] in accordance with priorities set in 1984. Nevertheless, we are still far from the goals of annually funding 2600 senior investigators, 200 new postdoctoral researchers, 1000 graduate research assistants, and 400 young investigators. Furthermore, the balance called for in the 1984 National Plan between support for senior investigators, on the one hand, and emerging scholars, on the other, has not yet been achieved.

Actual dollar increases of 68% in five years (95% over the six years from 1983 to 1989) represent bona fide gains for the mathematical sciences (34% and 46%, respectively, in real dollars). This occurred in a period when funding for the sciences was less than robust. However, apparently significant incremental increases can be misleading when the base level is very small. Table 2.2 shows vividly that there is still a long way to go. For example, total support for the field in 1984 stood at 44% of the goal recommended in the 1984 Report, and in 1989 it stood at 59% of that goal, after adjusting for inflation.

Table 2.3 updates Figure 1.2 and Table 1.1 and covers a longer period than does Table 2.2. These counts omit summer postdoctoral positions and other grants that do not reflect the spirit of the 1984 National Plan's recommendations. The absolute numbers of federally supported graduate research assistants and postdoctorals in the mathe

TABLE 2.3 Selected Ph.D. Pipeline Comparisons for 1980 and 1988

Point of Comparison	Chemistry	Physics	Mathematical Sciences
Graduate research assistants federally supported			
1980	3733	2976	421
1988	4673*	3591*	661
Annual Ph.D. production			
1980	1538	862	744
1988	2018	1172	749
Postdoctoral researchers federally supported			
1980	2255	1210	57
1988	2587	1280	188
Ratio of federally supported graduate research assistants to Ph.D. degrees produced (1988)	2.32	3.06	0.88
Ratio of federally supported postdoctoral researchers to Ph.D. degrees produced (1988)	1.28	1.09	0.25

* 1986 figures; later data not yet available.
SOURCE: National Science Foundation, Division of Science Resources Studies.

matical sciences shown in Table 2.3 remain only small fractions of the figures for comparable groups in chemistry and physics. The ratios presented in Table 2.3 more pointedly illustrate how the supported research time during the training years falls short in the mathematical sciences. Table 2.4 shows that in 1987 only 18% of full-time mathematical sciences graduate students received research-related support, compared to the 45 to 50% of graduate students in the physical sciences and engineering who had such support. Clearly, researchers in training in the mathematical sciences still have a difficult time gaining the depth and breadth of experience common among researchers in other sciences.

TABLE 2.4 Type of Support for Full-Time Graduate Students in Doctorate-Granting Institutions, 1987

	Number of Graduate Students	Number with Teaching Assistantships	Number with Reasearch Support*	Fraction with Reasearch Support
Biological sciences	37,734	8,210	22,114	0.586
Physics	11,075	4,089	5,660	0.511
Chemistry	15,664	7,005	7,630	0.487
Engineering	61,194	11,005	27,550	0.450
Social sciences	48,699	9,745	13,644	0.280
Computer sciences	13,578	3,258	3,612	0.266
Mathematical sciences	12,354	7,089	2,231	0.181

* Includes research assistantships, fellowships, and traineeships.
SOURCE: National Science Foundation, Division of Science Resources Studies (personal communication).

The number of federally supported individual investigators in mathematics, estimated as under 1800 in 1984,[8] stood at about 1900 in 1989 (Table 2.5). The 1984 Report discussed at some length (pp. 61–64) its conservative estimates that there are at least 2600 highly productive researchers in the mathematical sciences and that the field needs a cadre of about 2600 fully active, federally funded researchers in order to be balanced with other fields that use mathematics. Supporting an additional 700 investigators not only would increase their *individual* productivity, but would also encourage a larger fraction of mathematicians to stay highly productive, thereby increasing the *field's* overall productivity.

TABLE 2.5 Number of Mathematical Sciences Research Investigators Supported by Federal Grants and Contracts in FY 1988

Agency	Research Investigators Supported
Department of Energy	91[a]
Department of Defense	
ONR	223
NSA	41
ARO	213
DARPA	112
AFOSR	342
Total non-NSF research	1022[b]
National Science Foundation	
Total NSF research	1364[c]
Total federal agency count	2386
Less duplicates	-480[d]
TOTAL INDIVIDUAL INVESTIGATORS SUPPORTED	1906

[a] Since the field's renewal efforts most critically depend on university departments, 94 DOE laboratory investigators are omitted from this number.
[b] All non-NSF investigator counts listed are 95% of agency figures because those agencies advise that at least 5% of their grants are non-research.
[c] The NSF Division of Mathematical Sciences also supports 56 investigators for non-research activities such as conferences and other special projects, who are omitted from the total shown.
[d] Based on discussions with cognizant federal program officers, this committee conservatively estimates that at least 480 investigators appear on more than one federal grant.
Acronym key: AFOSR = Air Force Office of Scientific Research, ARO = Army Research Office, ONR = Office of Naval Research, DARPA = Defense Advanced Research Projects Agency, NSA = National Security Agency, NSF = National Science Foundation.
SOURCE: Program officers' counts, personal communication.

The infrastructure of the mathematical sciences is composed primarily of people, with funded investigators constituting the supporting structure for the field. Federal support for this core is the major infrastructure requirement of the discipline. However, other elements—graduate research assistants, postdoctoral researchers, funds for travel, and support for collaborative research—are becoming increasingly important now as the field unifies internally and expands externally, because those trends require greater breadth on the part of researchers. Funding for computer time and for hardware fixed costs is a rapidly growing requirement. The evolving needs of the infrastructure are addressed by the second thrust of the 1984 National Plan as summarized in Chapter 1.

Apropos of balance with other fields of science, the fraction of scientists engaged in R&D in educational institutions who receive federal support remains significantly lower in the mathematical sciences than in chemistry or physics and astronomy. According to the 1987 NRC survey of doctoral recipients,[9] 37% of the mathematical scientists in educational institutions who identified R&D as their primary or secondary activity had federal support. Comparable figures for chemists and for physicists and astronomers were 56% and 75%, respectively.

RESPONSE OF THE UNIVERSITIES

Few, if any, universities responded to the recommendations in the 1984 Report by conducting comprehensive reviews of the circumstances of their mathematics departments and formulating plans for improving those circumstances as part of strengthening the health of the discipline. It is difficult to say whether this was due to a lack of strong initiative by individual departments, to inertia of university administrations, or both. It is safe to say, however, that the general pattern of reaction does not seem to reflect awareness of the serious problems that must be overcome to achieve the renewal of the U.S. mathematical sciences enterprise.

University administrations were also urged in the 1984 Report to act as proponents of mathematical sciences research, interceding with government agencies and seeking new government and industry initiatives that could benefit their mathematical sciences departments. Again, very few universities appear to have responded.

The universities that did respond were principally those where deans, provosts, and presidents were made aware of the key issues by their mathematical sciences department chairs, often acting in tandem with a federal agency or with professional society representatives. This committee's canvassing of departments indicated that such a response was not the norm, however. Actions taken by university administrations have included support for departmental computer hardware, some start-up grants for new hires, and increased salary offers to allow the departments to compete for top-quality faculty. These measures have alleviated some of the strains on these departments.

University associations have taken no action in response to the recommendations in the 1984 Report. The report, with its stark descriptions of the circumstances of mathematics departments, was brought to the attention of leaders in university associations but did not find its way onto their active agendas.

RESPONSE OF THE MATHEMATICAL SCIENCES COMMUNITY

The group that has done the most and yet still has the most to do is the U.S. mathematical sciences community. It is useful to look at its response to the 1984 Report at two levels, the response of its leadership and the response of individual members of the community.

Response of Leadership

The mathematics community is engaged in a multistage, multiyear critical examination of its roles in research, education, and public policy. This ambitious undertaking, which will take several major steps forward in 1990, began in 1980 when leading mathematicians became alarmed over markedly decreased flows of talent and resources into their field, and into science and technology more broadly. They stimulated the development of a postdoctoral program and two new research institutes supported by NSF funding. They mobilized the professional societies in mathematics and enlisted the aid of the NRC in analyzing the forces undermining the infrastructures of mathematics research and education, for the purpose of developing national plans to reverse the trends of declining Ph.D. production, erosion of federal support, deterioration within mathematics departments, increasing student and public apathy toward mathematics, and growing complacency within the field itself about some of its responsibilities. The focus for the analyses was not the past, however; it was the opportunities that mathematics provides for the future well-being of science and technology, the nation, and its individual citizens.

The comprehensive assessment that these actions set in motion will continue throughout the 1990s and is generating in successive steps the plans and organizational mechanisms needed at the national level to renew and continuously maintain the vitality of this country's broader mathematical sciences enterprise. In 1990 the mathematical sciences community will have before it for widespread discussion organizational plans for its many future roles—in research, in precollege education, in college and university education, and in relating its work to various other communities. These discussions will be important elements of what is being called within the community the Year of National Dialogue.

Highlights of the major steps taken by the mathematical sciences community since 1980 to bring the dialogue to its present stage are as follows:

1980	*New postdoctoral fellowship program*—The NSF launches its Mathematical Sciences Postdoctoral Research Fellowships program to increase postdoctoral opportunities in mathematics.
1981	*Research institutes*—The Mathematical Sciences Research Institute at the University of California-Berkeley and the Institute for Mathematics and Its Applications at the University of Minnesota are created with NSF backing, further expanding the NSF's emphasis on the infrastructure for mathematical research.
1981	*The David Committee*—The NRC, the principal operating agency of the National Academy of Sciences (NAS) and the National Academy of Engineering, establishes a prestigious committee of scientists and engineers, chaired by Edward E. David, Jr., to review the health and support of research in the mathematical sciences in the United States.
1982	*The Browder briefing panel*—At the request of the White House, the NAS's Committee on Science, Engineering, and Public Policy (COSEPUP) establishes panels to brief the Science Advisor to the President on research opportunities in six fields. First to report is the Mathematics Panel, chaired by William Browder, which points out that mathematics is flourishing intellectually but that its research infrastructure is eroding rapidly.

1983	*The Joint Policy Board for Mathematics*—The American Mathematical Society (AMS), the Mathematical Association of America (MAA), and the Society for Industrial and Applied Mathematics (SIAM) create a nine-member joint executive action arm, the Joint Policy Board for Mathematics (JPBM), to begin implementing the recommendations of the David Committee. The JPBM emphasizes unity across the discipline, one of the five basic recommendations to the mathematics community later made by the David Committee.
1984	*Renewing U.S. Mathematics: Critical Resource for the Future*—The 1984 "David Report" highlights the flowering of mathematics and its uses since World War II and calls attention to serious signs of trouble: (1) the impending shortage of U.S. mathematicians and (2) a marked imbalance between federal support of mathematics research and support for related fields of science and engineering. Based on a careful analysis, it calls for more than a doubling of the FY 1984 federal support level and lays out a ten-year implementation plan, with specific roles for government, universities, and the mathematical sciences community.
1984	*The Board on Mathematical Sciences*—In December 1984 the NRC establishes the Board on Mathematical Sciences (BMS) to provide a focus of active concern at the NRC for issues affecting the mathematical sciences, to provide objective advice to federal agencies, and to identify promising areas of mathematics research along with suggested mechanisms for pursuing them. The BMS has become an important mechanism for drawing together representatives of all the mathematical sciences.
1985	*The Mathematical Sciences Education Board*—At the urging of the mathematical sciences community, the NRC establishes in October 1985 the Mathematical Sciences Education Board (MSEB) to provide "a continuing national assessment capability for mathematics education" from kindergarten through college. A 34-member board is appointed that is a unique working coalition of classroom teachers, college and university mathematicians, mathematics supervisors and administrators, members of school boards and parent organizations, and representatives of business and industry. This step reflects another of the basic recommendations of the David Committee: strong involvement of all sectors of the mathematics community in issues of precollege education.

1985	*Board on Mathematical Sciences' department chairs' colloquium*—An annual series of colloquia for chairs of mathematical sciences departments, begun by the Science Policy Committee of the AMS, is taken over and extended by the BMS. These successful meetings enable department chairs to pool their ideas and experience, focusing the ensemble into a valuable action group for addressing problems common to many mathematical sciences departments.
1986	*The Joint Policy Board on Mathematics, Washington, D.C., office*—The JPBM's Washington, D.C., activities come to embrace enhanced congressional contact and a Vigorous public information effort. An office of governmental and public affairs is opened and it helps launch National Mathematics Awareness Week, which is to become an annual April event. Contact with the media and resultant coverage of mathematics are increased, thus starting the long-term coordinated effort, recommended by the David Committee, to increase public information and understanding.
1986	*Board on Mathematical Sciences' Science and Technology Week Symposium* —This symposium, which is held annually at the NAS's Washington, D.C., facility, highlights the role of research mathematics in the sciences and engineering for an audience of scientists and policymakers.
1987	*Department of Defense advisory panels*—The BMS advisory panel to the AFOSR releases a report assessing the AFOSR mathematical sciences program. The BMS Panel on Applied Mathematics Research Alternatives for the Navy (PAMRAN) produces a report for ONR on selected research opportunities relative to the Navy's mission.[10] The BMS advisory panel to the Mathematical Sciences Program of the NSA is formed.
1987	*Project MS 2000*—At the urging of JPBM, and under the supervision of the BMS and MSEB, the NRC launches a comprehensive review of the college and university mathematics enterprise through the Mathematical Sciences in the Year 2000 (MS 2000) project. This is analogous to the David Committee's review of the health and support of mathematics research nationally.
1988	*100 Years of American Mathematics*—The occasion of the centennial of the AMS is used to develop a year-long series of related events promoting discussion within the mathematical sciences community of major issues it faces in research and education.

1988	*Office of Scientific and Public Affairs*—The American Statistical Association's (ASA) public information office is established to provide an interface between statisticians, the public, and the federal government.
1989	*Challenges for the '90s* [11] —The ASA report outlining significant application areas and societal problems for statisticians to explore in the 1990s is released.
1989	*Everybody Counts* [12] —The first BMS-MSEB-MS 2000 "report to the nation" on the state of mathematics education in the United States, kindergarten through college, based on the MSEB's precollege work and on preliminary work of the MS 2000 project. It emphasizes the potential of a modified mathematics education for contributing to the national welfare and outlines a national strategy for bringing about needed change in the 1990s.
1990	*Renewing U.S. Mathematics: A Plan for the 1990s*—This report, which is a five-year update of the 1984 Report. It describes emerging research opportunities and new challenges for government, universities, and the mathematical sciences community to continue the program to renew U.S. mathematics.
1990	*Second report to the nation*—The final report of the MS 2000 project, to appear near the end of 1990, will lay out a national plan for revitalizing mathematics education in U.S. colleges and universities.

Response of Individuals in the Research Community

The many mathematicians and statisticians who have peopled the advisory committees to federal agencies over the last six years have, by and large, exhibited the same bold imagination and concern with critical examination of their field that are reflected in the decade of initiatives just listed. They have continued to emphasize most strongly the support of graduate student and postdoctoral researchers. This strategy may be working: Ph.D. production appears to be turning upward after many years of decline.

Not surprisingly, the beneficial effects of the strategy to improve the research infrastructure are not experienced by many mathematicians. What is seen in university mathematical sciences departments is that the percentage of high-quality mathematicians with federal support is lower than the corresponding percentage in other fields of science and that the number of principal investigators supported has remained inadequate over the last six years. Hardly surprising, therefore, is the bona fide concern over the strategy to be followed in the next five or six years. This concern is most prevalent among investigators doing research in pure mathematics, the group for which the largest gap exists between the number of highly qualified researchers and the number with federal support.

NOTES

[1] What is now the Division of Mathematical Sciences at the NSF grew out of the former Division of Mathematical and Computer Sciences, which was divided in 1983.

[2] *Report of the Panel on Large-Scale Computing in Science and Engineering,* P. D. Lax, chair (National Science Foundation, Washington, D.C., 1982).

[3] NSF Division of Mathematical Sciences, personal communication. Amounts quoted are in actual dollars and have not been adjusted for inflation.

[4] *Renewing U.S. Mathematics: Critical Resource for the Future*, National Research Council (National Academy Press, Washington, D.C., 1984), pp. 57–65.

[5] There are some 10,000 mathematicians and statisticians in the mathematical sciences research community, of whom one-half are productive and about one-quarter highly productive, according to criteria spelled out in the 1984 Report.

[6] This figure results from updating the 1984 estimated budget of $180 million with the Higher Education Price Index published in *Statistical Abstract of the U.S., 1990*, Bureau of the Census, U.S. Dept. of Commerce, Washington, D.C.

[7] Support has also been given to the mathematics of computation initiative, which is not itemized in Table 2.1. The number of postdoctorals in 1984 was already double the counts given in the 1984 Report (dating from 1980 and 1981) because of initiatives begun concurrent with that report's preparation.

[8] *Renewing U.S. Mathematics: Critical Resource for the Future*, 1984, p. 95.

[9] Survey of Doctorate Recipients project, National Research Council (personal communication).

[10] A second PAMRAN opportunities report is scheduled for release in 1990.

[11] *Challenges for the '90s* (American Statistical Association, Washington, D.C., 1989).

[12] *Everybody Counts: A Report to the Nation on the Future of Mathematics Education*, National Research Council (National Academy Press, Washington, D.C., 1989).

3

Research Progress and Prospects

A survey of modern science and technology shows the mathematical sciences supporting crucial advances and giving rise to a wealth of creative and productive ideas. The mathematical achievements of this century, among the most profound in the history of the discipline, have been fundamental to the development of our technological age. And beyond their practical applications, the development of these mathematical sciences can be counted among the great intellectual achievements of humankind.

The mathematical sciences constitute a discipline that combines rigor, logic, and precision with creativity and imagination. The field has been described as the science of patterns. Its purpose is to reveal the structures and symmetries observed both in nature and in the abstract world of mathematics itself. Whether motivated by the practical problem of blood flow in the heart or by the abstraction of aspects of number theory, the mathematical scientist seeks patterns in order to describe them, relate them, and extrapolate from them. In part, the quest of mathematics is a quest for simplicity, for distilling patterns to their essence.

Of course, the nonmathematician who tries to read a mathematics research paper is bound to see the terms as anything but simple. The field has developed a highly technical language peculiar to its own needs. Nonetheless, the language of mathematics has turned out to be eminently suited to asking and answering scientific questions.

Research in the mathematical sciences is directed toward one of two objectives, and in some cases toward both: (1) to build on and expand the core areas of the discipline and (2) to solve problems or create problem-solution techniques for the increasingly numerous areas of science and technology where mathematics finds applications. Thus mathematical sciences research spans a spectrum from the examination of fundamentals to the application-driven solution of particular problems. This chapter surveys selected recent research achievements across this spectrum and mentions a sampling of current research opportunities that build on and promise to extend recent progress.

These opportunities for progress are real and exist in every major branch of the mathematical sciences. What is unusual about the mathematical sciences at this time is that, collectively, they are poised to make striking contributions across the whole spectrum of science and engineering.

THE MATHEMATICAL SCIENCES YESTERDAY

This potential for progress is the result of the remarkable growth of the mathematical sciences along three more or less parallel paths, stemming from a branching point in the 1930s, when mathematics as a "pure" discipline entered a new era characterized by reexamination of its foundations and exploitation of powerful new tools of abstraction. The result was an extraordinary flourishing of the discipline and an acceleration of the development of its major branches through the post-World War II years. The pace was so rapid that specialization increased, and for a while it seemed that topologists, algebraic geometers, analysts, and other groups of mathematical scientists could barely speak to one another. Each was inventing powerful new mathematical structures to unify previously disparate ideas and shed new light on classical problems.

These developments were accompanied by independent spectacular advances in applied mathematics and statistics during the same period. A major stimulus for these efforts was World War II, which presented an array of scientific and technological challenges. In communication, control, management, design, and experimentation, the power of mathematical concepts and methods was felt in the postwar years as never before.

A third line of inquiry rooted in the 1930s resulted in the evolution of the computer. The original work of a handful of mathematicians and electrical engineers some 50 years ago gave rise to a new discipline—

computer science—and a new tool, more powerful than any in history, for storing, processing, and analyzing information. Few people today need to be told of the impact computers are having on society. But many people need to have it pointed out that the computer is very much a mathematical tool that extends the reach and power of mathematics. The computer has already had an enormous impact on applied mathematics and statistics, and more generally on science and engineering (see section below, "Computers in the Mathematical Sciences").

THE MATHEMATICAL SCIENCES TODAY

In more recent years several dramatic changes have begun to occur within the discipline. Ever more general and more powerful methods and structures developed within pure mathematics have begun to reunify its various branches. The gap between pure and applied mathematics has also begun to close as more of the new methods are used in other fields—for example, in biology, medicine, and finance, as well as in fields usually thought of as mathematical. And the computer continues to stimulate the need for new mathematics while opening unprecedented new directions and methods for mathematical exploration per se. The immensity and richness of the methods and ideas developed by pure and applied mathematicians and statisticians over the last 50 years constitute a huge resource being tapped by the intellectual machine of science and engineering.

It is this image of the mathematical sciences today that one should have in mind while reading this brief survey of the state of the field as a whole. This is an unusual time in the history of the discipline. The simultaneous internal unification and greater awareness of external applications have brought the mathematical sciences into an era of potentially greater impact on the world around us.

This chapter is a companion to Appendix B, which contains more-detailed, brief descriptions of 27 important research areas that have produced significant accomplishments in recent years and that offer opportunities for further research. The committee emphasizes that the achievements and opportunities discussed in Appendix B are not intended to be comprehensive,[1] nor are they intended to suggest a specific agenda for funding research in the mathematical sciences. The aim is rather to demonstrate by example the vigor and comprehensiveness of current mathematical sciences research, and how the mathematical sciences are reaching out increasingly into all parts of science and technology even as the core areas of the mathematical sciences are expanding significantly. The selection of topics discussed in Appendix B illustrates the very real progress made across that spectrum in just the last five years.

The many applications of mathematics—which are most readily visible to the larger scientific community—can be realized only if the discipline as a whole remains strong and vibrant internally. Mathematics has indeed been interacting with other disciplines in healthy and productive new ways while simultaneously receiving an infusion of new ideas.[2] This process is accelerating, and the accounts in Appendix B illustrate the extent and import of cross-disciplinary research today. There is now an ever-increasing interest in applied problems—an interest that was perhaps not so evident a decade or two ago. At the same time mathematics has developed a substantially greater sense of internal unity and has displayed healthy cross-fertilization of ideas between subdisciplines. The intellectual energy produced by these two trends—looking externally for new problems and unifying internally—represents perhaps the greatest opportunity of all for the mathematical sciences over the next five years.

COMPUTERS IN THE MATHEMATICAL SCIENCES

From the convenience of a hand calculator, to the versatility of a personal computer, to the power of parallel processors, computers have ushered in the technological age. But they have also ushered in a mathematical age, since computers provide one of the main routes by which mathematics reaches into every realm of science and engineering. Computers have profoundly influenced the mathematical sciences themselves, not only in facilitating mathematical research, but also in unearthing challenging new mathematical questions. Many of the research advances described in this chapter and in Appendix B would not have been possible without computers and the associated mathematics that is concurrently being developed.

It is sometimes thought that once computers are powerful enough, mathematicians will no longer be needed to solve the mathematical problems arising in science and engineering. In fact, nothing could be farther from the truth. As computers become increasingly powerful, mathematicians are needed more than ever to shape scientific problems into mathematical ones to which computing power can be ap plied. And as science and engineering attempt to solve ever more ambitious problems—involving increasingly large and detailed data sets and more complicated structures—entirely new mathematical ideas will be needed to organize, synthesize, and interpret.

Computers are fundamentally connected to mathematics, in their physical design, in the way they organize and process information, and in their very history. The concept of a machine that could perform calculations automatically dates at least to the early nineteenth century. This concept became practical through the farsightedness of such mathematicians as Alan Turing, famous for cracking the German Enigma code during World War II, and John von Neumann, who was the driving force behind the design and building of the first computer.

Computer scientists continue to draw on theoretical mathematics, since advances in computing power are dependent upon mathematical ideas. Faster electronic components are continually appearing, but advances in hardware alone will not improve computing speed and efficiency, and most experts agree that the development of efficient software is not keeping pace with hardware development. Designing and analyzing the efficiency of computer algorithms are largely mathematical tasks. As machines become faster and computer memory sizes become larger, asymptotic improvements in the efficiency of algorithms become more and more important in practice. Recent research in theoretical computer science has produced significant improvements in specific algorithms and also new approaches to algorithm design, such as the use of parallelism and randomization.

The impact of the computer on the mathematical sciences has particularly broadened the domain of the mathematical modeler, who can now reliably simulate quite complex physical phenomena by computer. Widely used in all sciences and in engineering, and a research area in its own right, computer modeling plays a major role in the development of critical technologies such as the fabrication of micro-electronic circuits and the understanding of fluid flow. Developing appropriate simulations for a given technology invariably involves a high degree of scientific knowledge as well as sophisticated mathematical tools to describe and evaluate the model. Validation of these models may require statistical tests and comparison with an analytically produced limiting-case solution. Ultimately the model itself has to be tuned to physical data to confirm or improve its aptness for representing a physical process or phenomenon.

Finally, and by no means least important, the computer is beginning to have a significant impact on areas of core mathematics through its use in the visualization of underlying mathematical structures. Its use in proving theorems is evidenced by the recent proofs of the four-color theorem and of the Feigenbaum conjecture.

ACCOMPLISHMENTS AND OPPORTUNITIES

The committee has selected for presentation a collection of specific recent research achievements (Appendix B) that open up new opportunities for the future. It is emphasized that this is a partial list only and that lack of space precludes a fuller and more comprehensive survey. These examples of research progress and opportunities are as follows:

1. Recent Advances in Partial Differential Equations
2. Vortices in Fluid Flow
3. Aircraft Design
4. Physiology
5. Medical Scanning Techniques
6. Global Change
7. Chaotic Dynamics
8. Wavelet Analysis
9. Number Theory
10. Topology
11. Symplectic Geometry
12. Noncommutative Geometry
13. Computer Visualization as a Mathematical Tool
14. Lie Algebras and Phase Transitions
15. String Theory
16. Interacting Particle Systems
17. Spatial Statistics
18. Statistical Methods for Quality and Productivity
19. Graph Minors
20. Mathematical Economics
21. Parallel Algorithms and Architectures
22. Randomized Algorithms
23. The Fast Multipole Algorithm
24. Interior Point Methods for Linear Programming
25. Stochastic Linear Programming
26. Applications of Statistics to DNA Structure
27. Biostatistics and Epidemiology

A description in some detail of the specifics of each of these is given in Appendix B. What follows here is a discussion of how these achievements and opportunities (referred to by the Appendix B section number that also corresponds to the numbers in the listing of topics above), as well as some others not included in Appendix B, fit into the overall landscape of the mathematical sciences and their many and varied applications. It is hoped that this brief narrative will convey an appreciation of the breadth, scope, and usefulness of the mathematical sciences and how they are changing the contours of science and technology. At the same time it is hoped that this discussion will illustrate the vitality of mathematics as a discipline and show not only how ideas flow from the core of mathematics out to applications but also how the applications of mathematics can, in turn, result in ideas flowing to the core areas of the discipline. These interchanges affect almost every area of core mathematics.

For instance, developments in such core areas as number theory, algebra, geometry, harmonic analysis, dynamical systems, differential equations, and graph theory (see, for instance, Sections 1, 7, 8, 9, 10, 11, 12, and 19 in Appendix B) not only have significant applications but also are themselves influenced by developments outside of core mathematics.

The Living World

The mathematical and the life sciences have a long history of interaction, but in recent years the character of that interaction has seen some fundamental changes. Development of new mathematics, greater sophistication of numerical and statistical techniques, the advent of computers, and the greater precision and power of new instrumentation technologies have contributed to an explosion of new applications. Testifying to the impact of mathematics, computers are now standard equipment in biological and medical laboratories. Several recent fundamental advances seem to indicate a revolution in the way these areas interact.

The complexity of biological organisms and systems may be unraveled through the unique capability of mathematics to discern patterns and organize information. In addition, rendering problems into mathematical language compels scientists to make their assumptions and interpretations more precise. Conversely, biologists can provide a wealth of challenging mathematical problems that may even suggest new directions for purely mathematical research. Great differences in terminology and in the cultures of the two fields require a core of researchers with understanding of both areas.

Mathematical techniques for understanding fluid dynamics have made possible computational models of the kidney, pancreas, ear, and many other organs (Section 4). In particular, computer models of the human heart have led to improved design of artificial heart valves. Mathematical methods were fundamental to the development of medical imaging techniques, including CAT scans, magnetic resonance imaging, and emission tomography (Section 5). In the neurosciences the mathematical simulation of brain functions, especially through computer modeling, has come close enough to reality to be a powerful guide to experimentation. For instance, mathematical models have recently helped to elucidate studies in the formation of ocular dominance columns, patches of nerve cells in the visual cortex that respond to signals from only one eye. In addition, advances in neural network simulations are starting to have a significant impact on predicting how groups of neurons behave.

Recently DNA researchers have collaborated with mathematicians to produce some striking insights. When a new experimental technique allowed biologists to view the form of DNA under an electron microscope, researchers saw that DNA appeared tangled and knotted. Understanding the mechanism by which DNA unknots and replicates itself has led to the application of knot theory (a branch of mathematics that seeks to classify different kinds of knots) to DNA structure. At about the same time a breakthrough in knot theory gave biologists a tool for classifying the knots observed in DNA structure (see Section 10 for details). In addition, researchers are developing three-dimensional mathematical models of DNA and are applying probability theory and combinatorics to the understanding of DNA sequencing (Section 26).

Computers have brought sophisticated mathematical techniques to bear on complicated problems in epidemiology. One major effort is the mathematical modeling of the AIDS epidemic. Analysis of data on transmission of the human-immunodeficiency virus that causes AIDS has shown that HIV does not spread like the agents of most other epidemics. Various mathematical methods have been combined with statistical techniques to produce a computer model that attempts to account for the range of factors influencing the spread of the virus.

However, because of the complexity and size of the problem, researchers are finding current computational power inadequate and are looking for mathematical ways of simplifying the problem (Section 27).

The Physical World

The physical sciences, especially physics itself, have historically provided a rich source of inspiration for the development of new mathematics. The history of science has many examples of physical scientists hunting for a theoretical framework for their ideas, only to find that mathematical scientists had already created it, quite in isolation from any application. For example, Einstein used the mathematical theory of differential geometry, and, more recently, algebraic geometry has been applied to gauge field theories of physics.

Often the mathematical equations of physics cannot be solved precisely, and so their solutions must be approximated by the methods of numerical analysis and then solved by computer. Other problems are so large that only a sample of their solution can be found, with statistical techniques putting this sample in context. For this reason, the computer has become an indispensable tool for a great many physical scientists. The computer can act as a microscope and a telescope, allowing researchers to model and investigate phenomena ranging from the dynamics of large molecules to gravitational interactions in space.

The equations of fluid dynamics fit into the broader class of partial differential equations, which have historically formed the main tie between mathematics and physics. Global climate change is a topic of intense debate, and greater quantitative understanding—through the use of mathematical modeling and spatial statistics techniques—would greatly help in assessing the dangers and making reliable predictions (see Sections 6 and 17). One of the striking characteristics of today's applications is the range of mathematical subjects and techniques that are found to have connections to physical phenomena, from the application of symplectic transformations to plasma physics (see Section 11) to the use of topological invariants in quantum mechanics (see Section 12). As the various subfields of the mathematical sciences themselves become increasingly interconnected, new and unexpected threads tying them to the physical sciences are likely to surface. Over the past decade the highly theoretical area of Lie algebras has illuminated the physical theory of phase transitions in two dimensions, which has applications to the behavior of thin films (see Section 14). The study of chaotic dynamics, which employs a range of mathematical tools, has demonstrated that unpredictable behavior can arise from even the simplest deterministic systems and has been used to describe diverse phenomena, such as the interfaces between fluids (see Section 7). Investigation of quasicrystals, a category of matter combining properties of crystals and glasses, utilizes the mathematical theory of tiling, which describes ways of fitting geometrical figures together to cover space.

Other areas of science and engineering have benefited from the close connections between the mathematical and physical sciences. Because of the increased power of instrumentation technology, many phenomena can be observed with a precision that allows questions to be formulated in terms of mathematical physics. In fact, computational methods in fluid dynamics have made it possible to model a host of phenomena in chemistry, astrophysics, polymer physics, materials science, meteorology, and other areas (see Sections 1, 2, 3, 4, 6, and 23). The degree of precision achieved by these models usually is limited partly by the modeling and physical understanding, and partly by the available computing power. Improvements to the mathematical model or algorithm can often significantly increase the actual computing power achievable with given hardware, and hence the degree of model accuracy.

Theoretical physics has often posed profound challenges to mathematics and has suggested new directions for purely mathematical research. One spectacular instance of cross-fertilization came with the advent of string theory. This theory proposes the intriguing idea that matter is not made up of particles but rather is composed of extended strings. Algebraic geometry, a highly abstract area of mathematics previously thought to have little connection to the physical world, is one of the ingredients providing a theoretical framework for string theory. In addition, string theory is supplying mathematicians with a host of new directions for research in years to come. Section 15 in Appendix B provides details.

The Computational World

Much of the research on algorithms is highly mathematical and draws on a broad range of the mathematical sciences, such as combinatorics, complexity theory, graph theory, and probability theory, all of which are discussed in Appendix B. A striking and recent algorithmic advance is the development of interior point algorithms for linear programming (Section 24), a mathematical method used in many business and economics applications. Linear algebra and geometry were used in the development of these algorithms, which have found many applications, such as efficient routing of telephone traffic.

In addition to the design of algorithms, the mathematical sciences pervade almost every aspect of computing: in designing hardware, software, and computer networks; in planning for allocation of computing resources; in establishing the reliability of software systems; in ensuring computer security; and in the very foundations of theoretical computer science. In addition, all kinds of computations are dependent upon the branch of mathematics known as numerical analysis, which seeks to establish reliable and accurate means of calculation. For example, a current success in numerical analysis is the application of wavelet analysis (Section 8), which grew out of a body of theoretical mathematics, to produce faster signal processing algorithms. Another area of current research is computational complexity, which mathematically analyzes the efficiency of algorithms.

In statistics, modern computational power has permitted the implementation of data-intensive methods of analysis that were previously inconceivable. One of these, the resampling method—which can be thought of as a Monte Carlo method in the service of inference—is finding a wide range of applications in medical science, evolutionary biology, astronomy, physics, image processing, biology, and econometrics. The subject is still in its infancy, and one can expect more sophisticated developments to be stimulated by new applications. Computers and statistics are symbiotic in other ways as well. The statistics community is becoming involved in the statistical analysis and design of computer models arising in science and industry. The use of randomization in algorithms (see Section 22) has proved to be highly successful in certain kinds of applications and has stimulated new research in the properties of pseudorandom number generators. Meanwhile, other statisticians are assisting computer science by developing statistical techniques for characterizing and improving software reliability.

As the language of computer modeling, mathematics is revolutionizing the practice of science and engineering. In many instances, computer simulations have replaced costly experiments, for instance in aircraft design (Section 3). From visualization of the folding of protein molecules (see Section 26) to calculation of combustion patterns (see Sections 2 and 3), mathematical analysis combined with computing power has produced profound insights. The development of reliable and accurate simulations requires both understanding of the scientific problem at hand and knowledge of the mathematical tools to describe and evaluate the model.

Operations researchers have applied mathematics to a range of industrial problems such as efficient scheduling and optimization of resources, and statistical methods are now commonplace in evaluating quality and productivity (see Sections 18 and 23). Control theory—an interdisciplinary field drawing on mathematics, computer science, and engineering—has applications to such problems as autopilot control systems, chemical processing, and antilock brake systems on cars.

The advent of communications technologies has depended on mathematical developments. Image processing, acoustical processing, speech recognition, data compression, and other means of transmitting information all require sophisticated mathematics. One surprising example of the application of theoretical mathematics to such areas came recently from a branch of number theory dealing with elliptic curves. It turns out that a research result from that area has produced an entirely new approach for efficiently packing spheres. Because communications signals are sometimes modeled as higher-dimensional spheres, this result will help in improving the efficiency and quality of transmissions.

Computer graphics have opened a whole world of visualization techniques that allow mathematicians to see, rotate, manipulate, and investigate properties of abstract surfaces. In particular, the subject of the mathematical properties of soap films—"minimal surfaces" that are visible analogues of the solutions to optimization problems in many fields—has witnessed a recent breakthrough and a resurgence of interest because of computer modeling, as described in Section 13. Computer visualization techniques have also contributed to understanding the mathematics of surface tension in crystalline solids. Computers are now routinely, used in investigating many questions in number theory, a subject that examines properties of the integers and often finds application to such areas as computer science and cryptography. Symbolic manipulators—computers that perform operations on mathematical expressions, as opposed to numerical calculations—

are powerful new instruments in the tool kit of many mathematical scientists and are increasingly being used in the teaching of mathematics.

THE UNIFYING SCIENCE

The mathematical sciences have not only served as part of the bedrock on which science and engineering rests, but have also illuminated many profound connections among seemingly disparate areas. Problems that initially seem unrelated are later seen to be different aspects of the same phenomenon when interpreted in mathematical terms. In this way mathematics serves to unify and synthesize scientific knowledge to produce deeper insights and a better understanding of our world.

The mathematical sciences themselves are unifying in profound ways that could not have been predicted twenty years ago. The computer has lent unprecedented technological power to the enterprise, but mathematical sciences research still proceeds largely through individual creativity and inspiration. As the discipline becomes increasingly interconnected, progress in mathematics will depend on having many mathematicians working on many different areas. History has taught us that the most important future applications are likely to come from some unexpected corner of mathematics. The discipline must move forward on all of its many fronts, for its strength lies in its diversity.

THE PRODUCTION OF NEW MATHEMATICS

It is enlightening to note how so many of the research topics mentioned here and in Appendix B—for instance, the developments in partial differential equations, vortices, aircraft design, physiology, and global change—have developed out of the mathematical research described in the 1984 Report, both in its body and in Arthur Jaffe's appendix, "Ordering the Universe: The Role of Mathematics." The recent developments in number theory and geometry follow naturally from the Mordell conjecture, which was featured prominently in the 1984 Report. One can likewise see in the report of five years ago the roots of the recent developments in topology and noncommutative geometry. Although wavelet analysis was not mentioned in the earlier report, its roots in Fourier analysis were discussed. Similarly, the development of new algorithms was a prominent topic in the 1984 discussion, although the particular algorithms featured in Appendix B were not available then. These examples dramatically illustrate both continuity and innovation in mathematics.

It should be noted that much of the work discussed in Appendix B is the product of individual investigators. Mathematics is still very much "small science," with a tradition of individuals pursuing independent research. This is a strength because it allows the total enterprise to span a great many topics, remain flexible, and be responsive to the rest of science. However, the breadth required for many investigations calls for more collaborative work, which is practicable for mathematicians regardless of distance but is hindered in practice by the absence of adequate support for the occasional travel that is required. The recent innovations by the NSF (research institutes and science and technology centers) and the DOD (university research initiatives) provide valuable alternatives, both for established investigators and postdoctorals.

The continued production of valuable new mathematics requires not only that the number of individual investigators be increased as recommended in the 1984 National Plan, but also that the entire field reach out to the rest of the scientific world. Collaborating with researchers in other fields and making an effort to understand applications and improve the mathematical sophistication of others help the mathematical sciences become increasingly robust and valuable. The potential demand—in terms of the number of possible applications—for mathematics research is great, but the actual demand may be limited by failure of the mathematics community to communicate with others. Mathematics educators must design courses that meet the needs of the many students from other disciplines. Likewise, mathematical science researchers must write reviews and textbooks that are accessible to nonmathematicians. More of them need to make the extra effort to read journals and attend conferences outside their fields, to learn how to communicate fluently with potential users and collaborators, and to actively seek new opportunities for their work.

Finally, it is important to reemphasize that the research achievements and opportunities mentioned here and in Appendix B were selected from among a number of possibilities to illustrate the vigor and breadth of the mathematical sciences. This compilation is not intended to be comprehensive, nor is it intended to be a research agenda for the future. Many excellent new ideas and proposals will come to the fore as part of the natural development of the discipline, and these can and will compete for the attention of active researchers. The prospects are indeed bright.

NOTES

[1] See, for example, *Mathematical Sciences: Some Research Trends*, Board on Mathematical Sciences (National Academy Press, Washington, D.C., 1988) for a very different set of topics. Other recent, more specialized reports also list significant research opportunities. These include the BMS advisory panel reports (to the Air Force in 1987 and the Navy in 1987 and 1990) mentioned in Chapter 2; the American Statistical Association report *Challenges for the '90s*, listed in Chapter 2; and *Operations Research: The Next Decade* ("CONDOR report"), *Operations Research*, Vol. 36, No. 4 (July–August 1988), pp. 619–637.

[2] The NRC Board on Mathematical Sciences is producing a series of cross-disciplinary reports to foster this trend. The Institute for Mathematical Statistics has also addressed the trend in its report *Cross-Disciplinary Research in the Statistical Sciences* (Institute for Mathematical Statistics, Haywood, Calif., 1988).

4

The Problem of Renewal

The key problem facing the mathematical sciences today remains what it was in 1984: renewal. The pressing concerns of renewal are, Where will the mathematical talent come from? How can young talent be attracted to and retained in the career path? How can researchers be helped to remain active and be encouraged to serve as mentors for the next generation? The problem of renewal is crucial because of the increasing demand for mathematical scientists as educators and researchers.

DEMAND FOR MATHEMATICAL SCIENTISTS

Mathematics and familiarity with mathematical modes of thought are the foundations on which are built education in other scientific disciplines, and increasingly education in various areas of business, economics, and social science. Mathematical scientists are needed as educators to satisfy this growing demand, as well as to provide the increasingly sophisticated training of new mathematical scientists needed in increasing numbers by many quantitative areas of our complex society. United States Ph.D. production (supplemented by a large influx of foreigners) is at present barely sufficient to meet the current needs of our educational institutions, and demographers warn that faculties will need to grow after the year 2000 as the children of the baby-boom generation reach college age.[1]

Mathematical scientists are necessary also as researchers, because the expanding use of mathematics in all quantitative fields, the heightened mathematical sophistication of users, and the explosive growth in computer modeling are all fueling the demand for mathematics research.[2] Since World War II the trend toward quantification has affected not only traditionally quantitative areas but also such fields as biology, business, and economics. This trend seems to be continuing and even to be increasing, and it may be regarded as a natural phase of development that follows after observation, classification, and other qualitative methods alone become inadequate. Mathematics is vital to this progression because it is the language in which fundamental concepts and relationships can be precisely specified, manipulated, and extended for greater understanding. The spread of computer modeling has also generated a commensurate demand for mathematical expertise: mathematical scientists often provide critical steps in the process of developing computer models and algorithms, and they also address issues such as convergence criteria, error bounds, and expected asymptotic behavior, which are important for purposes of validation and control. Mathematicians need broad training in order to be responsive in this research environment.

Thus, to avoid serious declines in scientific and technological education, as well as shortages of urgently needed mathematical scientists at all levels, mathematical sciences Ph.D. production will likely have to increase in the near future. Assuring that the profession can attract bright young people is a goal to be addressed now, before large incremental demands for additional faculty and new mathematics strike.

SHORTFALL IN SUPPLY

The 1989 book *Prospects for Faculty in the Arts & Sciences*,[3] by W. G. Bowen and J. A. Sosa, warns of near-term faculty shortfalls in U.S. colleges and universities. For instance, the authors project 9300 faculty openings in mathematics and the physical sciences in the period 1997 to 2002, but fewer than 7500 candidates, with the result that a maximum of only 80% of available faculty slots will be filled, assuming current student to faculty ratios. Bowen and Sosa project a very flat supply of mathematics Ph.D. degree holders seeking U.S. academic employment over the next 15 years, averaging just 356 annually.

Doctoral degree production in the mathematical sciences declined steadily over many years, falling from a peak of 1281 in 1972 to a low of 688 in 1985. During recent years the percentage of U.S. citizens receiving a Ph.D. in the mathematical sciences has dipped below 50%. Some evidence, albeit inconclusive, suggests that the increase in sup port given to graduate students and postdoctoral researchers since the early 1980s is beginning to have an effect. After three years of essentially flat Ph.D. production, data from the spring and summer of 1989 show that the total number of Ph.D.s awarded had increased by 12% over the previous year, to equal approximately the level of production in 1978. In addition, women constituted a record 24% of the U.S.-citizen doctoral degree recipients. Whether or not these changes mark the beginning of a bona fide turnaround, the rate of influx of talent into the field will remain a high-priority concern for a number of years.

REASONS FOR THE SHORTFALL

Changing Demographics

The problem of renewal is made more difficult by the shifting demographics of the United States. The report *Workforce* 2000 (Hudson Institute, Indianapolis, Ind., 1987) has brought to public attention the dramatic changes occurring in the U.S. population and in the work force on which the economy depends. Its message that only 15% of net entrants to the work force between 1985 and the year 2000 will be native-born white males has surprised many people, driving home the point that in the future groups other than white males will provide much of the new talent for the nation.

This is a major issue in all the sciences, which are now so heavily dominated by white males. Science cannot continue to depend on the brain power of white males; their participation rate in the sciences would have to increase greatly to offset their declining numbers among work force entrants. Therefore, all branches of science must greatly increase efforts to attract and cultivate women and minorities. In mathematics, where women hold less than one Ph.D. in five and the numbers of blacks and Hispanics are almost vanishingly small, such efforts will need to be very intensive. The issues were vividly portrayed in the human resources chapter of the recent NRC report *Everybody Counts*.[4]

Cultural and Educational Problems

The problem of renewal in the mathematical sciences, exacerbated by changing demographics, must be seen and attacked in a broader context than that of graduate student recruitment and support. Attention must be paid to the entire mathematical pipeline, a requirement emphasized in *Everybody Counts*:

> The underrepresentation of minorities and women in scientific careers is well documented and widely known. Less widely known is the general under-representation of American students in all mathematically based graduate programs. Evidence of disinterest in mathematics permeates all racial, socioeconomic, and educational categories, although the level of disinterest varies greatly among different groups. Young Americans' avoidance of mathematics courses and careers arises from immersion in a culture that provides more alternatives than stimulants to the study of mathematics. Without motivation and effective opportunity to learn, few students of any background are likely to persevere in the study of mathematics....
>
> Developing more mathematical talent for the nation will require fundamental change in education. Our national problem is not only how to nurture talent once it surfaces, but also how to make more talent rise to the surface. Although more must be done, the United States is reasonably successful in tapping and channeling the highly visible talent springs which develop without special support from formal schooling. But these sources are inadequate to our national need. We must, in addition, raise the entire water table.[5]

The forthcoming final report of the MS 2000 project will detail many crucial recruitment and educational reforms needed into the twenty-first century. All will require substantial input from the mathematics profession for planning and implementation.

What Discourages Talent

How do young people choose a career in the mathematical sciences? A very few young people with mathematical talent come into contact with interesting aspects of the subject and become committed to mathematics at an early age, but this is very much the exception. Most young people who decide to study mathematics make the commitment much later, balancing their aptitudes against the possible disciplines to pursue while weighing the quality of life offered in each profession. Obtaining information about mathematical careers is often difficult for the prospective mathematics major, because most teachers and other students are poorly informed about the possibilities. An unusually enthusiastic high school teacher, a professor in the early years of college, or some family friend or relative in the profession is the usual adviser. The picture they convey to the young student necessarily contrasts the joy of doing mathematics with the difficulty of obtaining support for graduate and postdoctoral studies, the heavy teaching loads even in the predoctoral years, and, after becoming established in research, the sudden decrease in the midyears in the ability to obtain research support. All this is in sharp contrast to career opportunities in the other sciences and in engineering.

The Leaky Pipeline

The mathematical sciences career path includes education from secondary school through the completion of the Ph.D., and professional development beyond that. In assessing the career path, this committee considered the quality of undergraduate and graduate education, the efficacy and efficiency of the process by which students become researchers and teachers, and the opportunities for continued professional growth throughout a mathematician's career. Renewal efforts critically depend on what takes place in doctorate-granting departments, and that is the milieu this committee addresses. Although problems exist throughout the career path, this discussion focuses on those that directly affect the production of research and researchers.

On a national level, evidence such as that shown in Figure 4.1 documents the leaky educational pipeline in mathematics. About half the students in the mathematics pipeline are lost each year. (Only U.S.-citizen Ph.D.s are shown in Figure 4.1 because the chart represents the flow of U.S. students through the mathematics pipeline.) In high school and college, mathematics acts as a filter rather than as a pump; students are deterred, and mathematical talent is not identified and encouraged. As for graduate studies, the ratio of doctoral degree recipients to bachelor's degree recipients is lower in the mathematical sciences than in many other fields: over the period from 1971 to 1985, this ratio averaged 4% for the mathematical sciences, whereas it was 6% for engineering, 8% for the life sciences, and 15% for the physical sciences.[6] Clearly, talent and productivity are being lost throughout the mathematics pipeline.

Many career path shortcomings affect the production of Ph.D. mathematicians. In graduate school, only 18% of mathematical sciences students receive research funds to support themselves, compared to 28% in the social sciences, 45% in engineering, and 50% in the physical sciences (see Table 2.4). Upon receipt of a doctorate, the neophyte mathematical scientist does not generally have the benefit of postdoctoral research training, but moves directly into an assistant professorship. Only 21% of mathematical sciences junior faculty (assistant professors and instructors) active in R&D received federal support in

1987, compared with 53% in chemistry and 67% in physics and astronomy.[7] Further along the pipeline, only 18% of Ph.D. mathematical scientists in four-year colleges and universities surveyed in 1985 could call research their primary activity, as compared to 33% of chemists and 42% of physicists and astronomers.[8] The correlation between research funding for junior researchers and research activity in later years is striking. In addition, with the current reward structure, it appears that the 82% of mathematical scientists who consider something other than research their primary activity are undervalued.

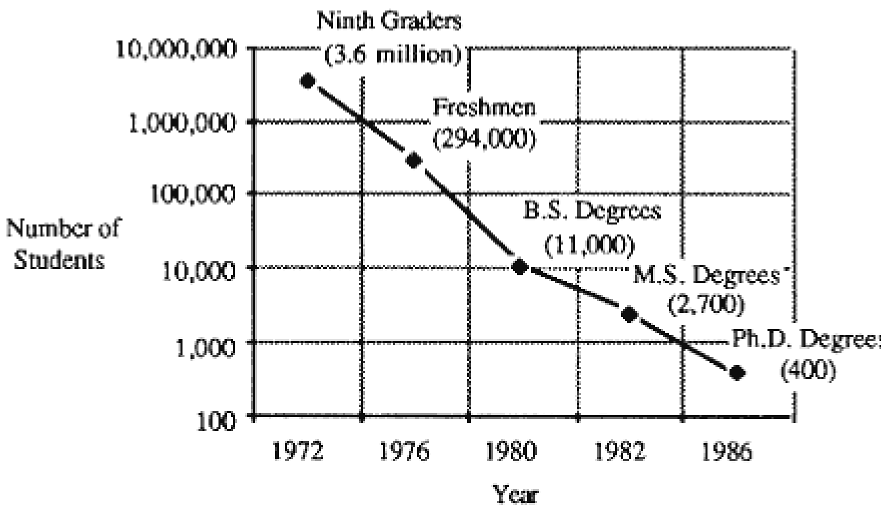


Figure 4.1
U.S. students in the mathematics pipeline.
Source: From Mathematical Sciences in the Year 2000 project, reprinted from National Research Council, A Challenge of Numbers: People in the Mathematical Sciences (National Academy Press, Washington, D.C., 1990), p. 36.

Mathematical sciences departments have adapted to these conditions but are unable to overcome them. While preparing this report, the committee asked four department chairs to write essays giving anecdotal accounts of problems and solutions. The problems described often stemmed from the fact that departments in the mathematical sciences have the broadest mission in the university, comprising research and graduate education, undergraduate education, upper-and lower-division service, and community outreach and education. Partial solutions often came from cooperation between departments and their administrations in choosing priorities among these missions and setting mutually satisfying goals.

How an increase in funding—in this case, from the university—can lead to marked improvements in overall departmental quality is reflected in one department chair's statement:

> Impending shortages of mathematicians and resulting increased competition between universities have made it more vital than ever to establish a first-class senior faculty.... [This] enabled us to argue for and achieve a general increase in salary levels.... In several cases research activity has improved as a result of the new climate in the department. Indeed, in some instances this has resulted in federal funding for those who had been off the rolls for some time. Faculty improvement has begun to have an effect on our resources for the future, especially with regard to the quality of our graduate students.... we now find a small group worthy of any institution.

The Reward Structure

Another chair's essay pointed out a problem with the academic reward structure: "A number of faculty develop instructional material, textbooks, and software [yet they] receive little recognition for these efforts, and a portion of the faculty attach negative weight to these activities. With our many missions, we have a responsibility to reward excellence in a broad range of activities." Another stated that "promotion or tenure without grant support is extremely difficult." Later, the same writer noted, "The implementation of [better courses for elementary and secondary school teachers] will require the participation of active mathematicians, although this is not always easily achievable because of the possible detrimental effects on one's career."

The current reward structure may be inferred from the results (Table 4.1) of a 1985 Conference Board for the Mathematical Sciences (CBMS) survey, which asked university department chairs to rate the importance of various professional activities to promotion or salary decisions.

ADDRESSING THE SHORTFALL

Recruitment

Recruitment requires an active effort on three fronts: improving the quality of the career path within mathematics, improving the external appeal of the profession, and performing recruiting drives. The first two lay the groundwork to maximize the effectiveness of the third, which is not discussed here. The quality of the mathematical sciences career path must be improved in order for recruitment efforts to succeed. Bowen and Sosa state:

Table 4.1 Department Chairs' Valuation of Professional Activities, 1985

	Valuation by Department (Percent)			
	Mathematics		Statistics	
Professional Activity	High*	Low*	High*	Low*
Published research	96	0	100	0
Talks at professional meetings	42	5	25	11
Supervision of graduate students	34	7	81	0
Classroom teaching performance	70	3	71	6
Undergraduate/graduate advising	9	22	21	21
Service to dept., coll., or univ.	31	5	31	11
Activities in professional societies or public service	22	8	31	6
Expository or popular articles	22	13	14	19
Textbook writing	9	35	12	50

* "High" means 4 or 5 on a scale of importance running from 0 to 5; "Low" means 0 or 1.
SOURCE: Adapted from National Research Council, *A Challenge of Numbers: People in the Mathematical Sciences* (National Academy Press, Washington, D.C., 1990).

> While many variables affect decisions to pursue graduate study, students are surely more likely to seek Ph.D.'s, and to think seriously about teaching and research vocations, when employment opportunities in academia are attractive. The historical record offers strong support for this simple line of reasoning.... [T]he number of newly awarded doctorates in almost every field increased dramatically in the 1960s. It is no coincidence that those were also the years when the number of academic appointments was growing rapidly, faculty salaries were rising, and financial aid for graduate study was widely available. Subsequently, the grim academic labor markets of the 1970s were accompanied by a sharp decline in the number of new doctorates earned, especially by U.S. residents.[9]

It is reasonable that, in the absence of strong counter-influences, these same correlations hold for individual fields. A career in the mathematical sciences suffers by comparison with those in other fields, due to long-term effects of funding imbalances, so that recruiting efforts in mathematics are hindered. The 1984 National Plan and this committee's updated recommendations address precisely these problems.

Equal in importance to the effort to improve the career path is the need to improve the external appeal of the profession. The mathematics profession must reach out to students and the general public to show the value and accessibility of mathematics; the image of rigid, unquestionable theorems should be complemented by that of excited, creative, and inspired people developing *new* mathematics. This is part of the role of the Board on Mathematical Sciences, the Joint Policy Board for Mathematics, and the professional societies, but is also a challenge for individuals throughout the mathematical sciences community. The beauty, history, and excitement of mathematics are seldom conveyed to students; in fact, too often they receive the impression that mathematics is all 150 years old and stagnant, and that individuals cannot contribute except in limited and long-term ways. Thus the field appears uninteresting and intimidating.

Mathematics educators at all levels can reverse these negative images. Students who see computer-oriented work as glamorous need to learn that, without mathematics, the power of computers could not be applied to many real-world problems. Mathematical work crucial to global warming studies, aircraft design, or medical imaging devices should capture the attention of students who think that mathematics is irrelevant to modern developments. Two high school student winners of the 1988 Westinghouse Science Talent Search carried out new mathematics work, exemplifying the fact that newcomers can contribute. Advances such as wavelets and Karmarkar's algorithm show that it need not take decades for research to bear fruit. Finally, students should know that some 525,000 persons have received some mathematical sciences degree in the United States in the last 40 years. Most are still in the work force, yet three-fourths of them are working in areas other than the mathematical sciences. Clearly, a mathematical sciences degree provides a flexible foundation.

Many parts of the 1984 National Plan would aid recruitment by improving the career path to bring it more in line with those of other sciences, by increasing the attractiveness of a life in mathematical research, and by increasing the cadre of active, enthusiastic researchers, who serve as recruiters as well as mentors and positive role models.

Replenishment

Recruitment is just the start of renewal. If renewal is to be achieved, young people who choose to specialize in the mathematical sciences must first be well trained and then must be encouraged to remain active throughout their careers. The 1984 National Plan addresses both of these goals. The former can be accomplished by providing more research time for graduate students and postdoctorals, and by ensuring support for established researchers who will act as mentors. The latter can be attained by supplying a sufficient number of grants to encourage continued active research and professional development.

The laboratory sciences generally provide longer periods of direct interaction with faculty mentors than do the mathematical sciences: close contact in a research context begins early in a graduate student's career and extends beyond the doctorate for additional training. Beginning graduate students in the laboratory sciences may learn as much from advanced graduate students and postdoctorals as they do from the principal investigator—the group provides a mutually supportive and nurturing learning environment for all. The challenge for the mathematical sciences is to create an analogous environment for their own graduate students and postdoctorals.

The 1984 National Plan stipulates financial support for graduate student and postdoctoral research training, and, by recommending an increase in the number of established, funded investigators, provides for an environment that fosters mentor-apprentice interaction. Professors with good abilities and track records as mentors should be encouraged by, for example, being provided with their own postdoctoral funds, either individually or in groups.

An apprenticeship period is particularly important now because breadth is becoming vital to research in the mathematical sciences. Researchers have become more problem oriented, and so each investigator must be familiar with a wide variety of mathematical tools: unification of the field relies on, and demands, broader experience. Many mathematical scientists must also be adept in areas of engineering, biology, management science, or other disciplines. This breadth is apparent in much of the work profiled in Appendix B. The mathematical sophistication of researchers in many fields is increasing, with the result that mathematical sciences research problems are appearing more frequently in a nonmathematical context. The graduate student and postdoctoral research time stipulated in the 1984 National Plan would provide the quality training needed to renew the intellectual base of mathematics.

Unless its members possess diverse skills and interests, the field will not be able to respond to the demand for new mathematics arising in novel areas. The scientific and technological competitiveness of the United States is ever more dependent on our national ability to respond quickly to new developments, with minimal time to intellectually "retool." Unifying the mathematical sciences and linking them more strongly with other quantitative fields are important goals in today's globally competitive environment.

Replenishment of the field also demands that the potential of well-trained Ph.D. degree holders be realized. This can be fostered in part by supporting a larger cadre of individual investigators, including young investigators, as recommended in the 1984 National Plan. Research funding can also enable travel and workshop attendance, which expose researchers to new ideas, quicken their response to new research directions, and provide intellectual invigoration that can improve their ability to act as mentors. The availability of summer support would encourage a larger cadre of university mathematical sciences faculty to remain active in research. Although not all of these faculty members will carry on research throughout their careers, far too many currently cease such efforts within the first few years after receipt of the Ph.D.

Broadening the Reward Structure

The multiple roles played by mathematical sciences departments—providing general and specialized education for a large fraction of the college and university population, influencing society's mathematical knowledge through elementary and secondary school teacher training and expository writing, and developing new mathematics and future mathematicians—are all essential. Therefore, a corresponding reward structure is needed, so that people are encouraged to devote their energies to whichever of these valuable tasks best suit them. This change would make more efficient use of the human resources in the field. A revamped structure should include rewards for the following activities:

- *Teaching*—particularly conscientious and effective teaching at all levels, including the lower-division service courses and courses for prospective teachers. Tailoring courses for nonmajors, directing and improving the teaching abilities of graduate student instructors, and designing computer-oriented classes and their software are activities that must be encouraged.

- *Mentoring*—guiding and enhancing the education of undergraduate majors, graduate students, and postdoctorals. Good mentors devote time and energy to this process, honing their methods and maintaining broader interests than those necessary to do research alone, so as to give their apprentices the breadth and depth required of today's mathematicians.
- *Outreach*—collaborating with people in other fields, recruiting good students, particularly from among women and minorities, and communicating with local education professionals and with industry. Since the long-term health of mathematics depends on maintaining strong ties with the other sciences, recruiting top-quality people, and satisfying the mathematical needs of society and industry, departments and universities should encourage—and reserve funds for—all of these outreach efforts.

In short, there should be a broader spectrum of respectable careers available to people educated in the mathematical sciences.

NOTES

[1] Current mathematics faculties cannot accommodate this growth. The number of full-time mathematics faculty at research universities actually decreased by 14% over the period 1970 to 1985; the simultaneous 60% increase in course enrollment was handled by tripling the number of part-time faculty.

[2] The demand for nonresearch mathematical scientists is also growing. The number of secondary school mathematics students will rise before the college population does, requiring more mathematics teachers. And the projected demand for mathematical scientists at all levels is expected to increase by 29% between 1986 and 2000, compared to a 19% growth in overall employment. Many of these people will be employed in science and engineering. Considering all degree levels, the employment of mathematical scientists has already tripled over the period from 1976 to 1986, showing a 10% annual growth rate second only to that for computer specialists among the science and engineering fields.

[3] Bowen, W.G., and Sosa, J.A., *Prospects for Faculty in the Arts & Sciences* (Princeton University Press, Princeton, N.J., 1989).

[4] National Research Council, *Everybody Counts: A Report to the Nation on the Future of Mathematics Education* (National Academy Press, Washington, D.C., 1989).

[5] National Research Council, *Everybody Counts*, 1989, pp. 17–18.

[6] Data from Table 2.1, National Research Council, *A Challenge of Numbers: People in the Mathematical Sciences* (National Academy Press, Washington, D.C., 1990).

[7] Data from the Survey of Doctoral Recipients project office, National Research Council (personal communication).

[8] *Doctoral Scientists and Engineers: A Decade of Change*, NSF 88-302 (National Science Foundation, Washington, D.C., 1988).

[9] Bowen, W.G., and Sosa, J.A., *Prospects for Faculty in the Arts & Sciences*, p. 162.

5

Recommendations

The 1984 Report described serious deficiencies in the situation of the mathematical sciences (see Appendix A). These shortcomings were reflected in the inability of the mathematical sciences research community to renew itself by attracting a suitably large and talented cohort of students, and they suggested the prospect of declining productivity in research activities. The 1984 Report recommended a plan for renewal, the National Plan for Graduate and Postdoctoral Education in the Mathematical Sciences, which called for mathematical sciences funding to balance that in the principal disciplines it supports, namely, the physical and life sciences and engineering. That 1984 National Plan has been only partially carried out: funding has risen to some $130 million per year, a figure that is about $100 million per year short of the 1984 plan's goal for 1989.

PRIMARY RECOMMENDATIONS

The committee believes it is imperative to meet the goals set out in the 1984 National Plan, but the funding to meet those goals should be increased to $250 million per year, $225 million to cover the present cost of the 1984 National Plan, plus $25 million to support coherent programs that can effectively address the career path problems. In the committee's judgment this funding level, if achieved within three to five years beginning in FY 1991, will result in a reasonably balanced situation, one that will allow the mathematical sciences community to replace retiring members and also supply the growing needs of industry and government. Note that the recent report by W.G. Bowen and J.A. Sosa[1] estimates that the supply to demand ratio for mathematics and physical sciences faculty in the 1990s will be only 0.8. This projection is doubly worrisome for the mathematical sciences with their existing renewal difficulties.

Increased research funding alone will not be adequate to assure the renewal of the mathematical sciences. Other serious deficiencies in the mathematical sciences career path make it less attractive to students than the paths in the other sciences and in engineering. These deficiencies include markedly less opportunity for faculty research, fewer graduate research positions with stipends, and fewer postdoctoral research positions. Then, too, students seem to perceive a sink-or-swim attitude among many mathematical sciences faculty members.[2] These deficiencies exist despite efforts to increase graduate student funding and postdoctoral opportunities over the past five years. The drop-out rate from the mathematics career pipeline (beginning at the undergraduate level and terminating at the doctoral level) averages 50% per year, which is markedly higher than the corresponding rates in the other sciences and engineering.

A significant part of any increased funding over the coming five years should be used for coherent programs operated by departments, faculty groups, or even individual faculty members to (1) improve recruiting of qualified students, particularly women and minority students, (2) keep students within the field by providing mentors at every educational level, (3) provide research opportunities at all stages of students' careers, and (4) provide improved research opportunities for junior faculty and better access to research facilities and collaborators for senior faculty. The reward structure for mathematicians should be modified to credit involvement in such activities. Comprehensive, integrated programs should be encouraged and even solicited by funding agencies as part of their mathematical sciences activities. The National Science Foundation has already taken steps in this direction.

Thus this committee's three primary recommendations are as follows:

I. **Implement the 1984 National Plan, but increase the level of federal funding for the mathematical sciences to $250 million per year**. (The 1984 plan's goal of $180 million per year has risen due to inflation to $225 million, to which this committee has added $25 million per year for implementing Recommendation II.)

II. **Improve the career path in the mathematical sciences to continue to attract sufficient numbers of talented people and to use the entire human resource base more effectively.** Implementation of the 1984 National Plan by itself would accomplish much toward this goal. The committee estimates that $25 million per year of the federal funds called for in Recommendation I will significantly augment the 1984 National Plan through the funding of coherent programs aimed at *directly* encouraging young people, especially women and minorities, to enter and remain in mathematical sciences careers. Mathematical sciences departments should give increased recognition to faculty who act as mentors, who contribute to education, and who interact with collaborators from other disciplines, while universities should do more to help their mathematical sciences departments meet their multifaceted missions; these actions would improve the career path and thereby *indirectly* encourage young people to enter and remain in mathematical sciences careers. Cooperation between university mathematical sciences departments and their administrations is critical for successful implementation of this recommendation.

III. **Because a wealth of striking research problems—many with potential applications to modern science and technology—currently challenges mathematical scientists, and be cause added intellectual stimulation will contribute to the renewal of the field, increase to 2600 (the level recommended in the 1984 National Plan) the number of senior investigators supported annually.** This goal is implicit in Recommendations I and II, but it demands clear emphasis.

Mathematical sciences research has been highly productive over the past five years. Furthermore, mathematicians have become increasingly interested in transferring new mathematics into applied fields and in working with users of mathematics. These trends are bringing core and applied mathematics closer together as well as integrating formerly distinct fields of mathematics. The resulting vigor has been augmented by the rise of computation as a tool in research. Indeed, the pace of research in the mathematical sciences is accelerating. Thus the increase in productivity from additional funding is likely to be disproportionately large. In the United States there are some 1900 federally supported senior investigators. The committee estimates that an additional 700 highly productive mathematical sciences researchers are not supported. These people, who represent an opportunity to sustain the vigor and productivity of the field, should be given adequate funding.

Finally, the committee emphasizes that a vigorous mathematical sciences enterprise in the United States is essential to addressing the educational shortfalls so widely perceived by the public and their representatives. Too few primary and secondary school teachers are qualified to teach mathematics. Yet it is at this level that students often decide that they can or cannot undertake careers in science or engineering. Mathematics is perceived as a barrier to students who might otherwise make ambitious career choices: this is especially true for women and minorities.

Mathematics faculties in colleges and universities directly and indirectly affect the quality of primary and secondary school mathematics teaching. Preparation and continuing education for these mathematics professors must be improved if the United States is to remain competitive in science and technology. Mathematics education is crucial to achieving international competitiveness in all the sciences. Major initiatives, as suggested above, are critical to any serious attempt to address the educational problems so often lamented publicly. The health and vigor of the mathematical sciences is a vital index in judging the prospects for national attempts to solve the science-based problems of U.S. society.

DIRECTED RECOMMENDATIONS

Federal Agencies

Agencies Collectively

Continue to encourage the internal unification of the mathematical sciences and their outreach to other fields. Support efforts toward community-wide implementation of the career path improvements called for in Recommendation II. Continue to push for adequate funding for the mathematical sciences and especially for the support of significantly more senior investigators.

National Science Foundation

Begin to increase the number of supported senior investigators in the mathematical sciences. Continue to increase the number of supported graduate student researchers and postdoctoral researchers. Work with national groups to address issues involving human resources.

Department of Defense

Push for real growth in the mathematics budgets of the Air Force Office of Scientific Research, the Army Research Office, and the Office of Naval Research. Continue the progress of the DARPA and NSA programs. Persuade the leaders of Department of Defense agencies to appreciate the importance of the mathematical sciences for national defense and to understand that the long-range prospects for the defense of the country must rest on a strong, continuing research base.

Department of Energy, National Institutes of Health, and National Aeronautics and Space Administration

Reevaluate programs to take advantage of the role the mathematical sciences can and do play. Increase support for the mathematical sciences, which currently is concentrated too much in the NSF and DOD. This can have an adverse impact on the nation's total science, engineering, and technology research and education, especially if DOD funding of mathematics does not increase. Recognize that the future quality of technology bases affecting agency missions is dependent on the mathematics being done now.

Office of Science and Technology Policy

Send a clear message to the federal agencies that reversing past declines in the mathematical sciences is a continuing national priority.

Universities

Recognize the central importance of healthy mathematical sciences departments to any university. Conduct in-depth reviews of the circumstances of mathematical sciences departments and work with department chairs to develop and emphasize plans for departmental improvement. Discuss and clarify the department's mission and goals and the administration's expectations of faculty members. Plan coordinated action to address career-path and reward-structure issues and undergraduate and graduate education standards.

In addition, work as intermediaries between mathematical sciences departments and local government and industry. Apprise state technology offices of the importance of mathematics to the quality of education and to the local economy. Make industry aware of the contributions that mathematical scientists can make as both researchers and teachers.

Department Chairs and University Administrators

Make special efforts to recruit women and minorities. Reassess and broaden reward structures so that they reflect the broad missions of mathematical sciences departments: research, service teaching, undergraduate and graduate education, and contributions to the national precollege mathematics education effort. Reevaluate the use of graduate teaching assistantships, being mindful of the twin goals of high-quality undergraduate instruction and well-balanced Ph.D. training.

Mathematical Sciences Community

Maintain the tradition of first-class research. Focus more attention on career-path problems (Recommendation II). Offer better training, including a commitment to a system of mentors for graduate students and postdoctorals. Create programs that provide the breadth necessary for today's mathematics research and applications. Establish guidelines for evaluating and improving mathematical sciences Ph.D. programs. Recognize the breadth of the mathematical sciences academic mission.

NOTES

[1] Bowen, W.G., and Sosa, J.A., *Prospects for Faculty in the Arts & Sciences* (Princeton University Press, Princeton, N.J., 1989).

[2] This observation has been made about faculty members in all of the sciences; see, for instance, Kenneth C. Green, "A Profile of Undergraduates in the Sciences," *American Scientist*, Vol. 77, No. 5 (Sept.–Oct. 1989), p. 478.

Appendixes

Appendix A

Executive Summary of the 1984 Report*

1. Background

The Ad Hoc Committee on Resources for the Mathematical Sciences was established in June 1981 by the National Research Council's Assembly of Mathematical and Physical Sciences[1] to review the health and support of mathematical research in the United States. Preliminary evidence presented to the Assembly by its Office of Mathematical Sciences had suggested that in the nation's major universities external support for mathematics had lagged considerably behind corresponding support in other fields of science. The evidence was sufficiently dramatic that the charge to the Committee contained more emphasis on financial support than is usual for a review of the health of a scientific field. Committee members with a range of scientific interests and experience were chosen to ensure that this review would be carried out with a broad perspective.

Early in our Committee's deliberations, we came to three important realizations:

- Mathematics is increasingly vital to science, technology, and society itself.
- Paradoxically, while mathematical applications have literally

* Reprinted from *Renewing U.S. Mathematics: Critical Resource for the Future* (National Academy Press, Washington, D.C., 1984), pp. 1–10.

exploded over the past few decades, there has been declining attention to support of the seminal research which generates such benefits.

- Opportunities for achievement in mathematical research are at an all-time high, but capitalizing on these will require major new programs for support of graduate students, young investigators, and faculty research time.

These perceptions guided the activities of our Committee as we pursued our charge.

II. The Mathematical Sciences

A. Strengths and Opportunities

The period since World War II has been one of dazzling accomplishments in mathematics. The flourishing of the discipline has run hand-in-hand with burgeoning applications, which today permeate the theoretical fabrics of other disciplines and constitute important parts of the intellectual tool kits of working scientists, engineers, social scientists, and managers. These developments were nurtured by cooperation between the universities and the federal government, and fueled by a national commitment to strengthening scientific research and education. The injection of federal funds into universities, combined with a pervasive sense of the importance of research, attracted numbers of the best young minds in the country into science and mathematics and propelled the United States into world leadership in the mathematical sciences.

The field expanded and diversified enormously during this period. Mathematical statistics matured. Operations research was born. Mathematics in engineering flowered with prediction theory, filtering, control, and optimization. Applied mathematics extended its reach and power, and the discipline of mathematics grew at a breath-taking pace.[2]

Since World War II, the impact of mathematics on technology and engineering has been more direct and more profound than in any historical period of which we are aware. When we entered the era of high technology, we entered the era of mathematical technology. Historically, the work of Wiener and Shannon in communication and information theory highlights the change. The mathematical under pinnings of the computer revolution, from von Neumann onward, and the sophisticated mathematical design of the fuel-efficient Boeing 767 and European Airbus airfoils further exemplify the increased impact of applied mathematics.

The discipline of mathematics also advanced rapidly and contributed to the solution of problems in other fields of science. Fundamental questions in algebra, geometry, and analysis were addressed with ever-increasing conceptual generality and abstraction; new interactions between parts of contemporary mathematics and physics, as in gauge field theory, remind us of the payoff of mathematics for other sciences. Indeed, in the span of little more than the past two years we have seen four Nobel Prizes awarded to U.S. scientists for largely mathematical work, much of it employing mathematical structures and tools developed over the last few decades: Chandrasekhar in astrophysics, Cormack in medicine (tomography), Debreu in economics, and Wilson in physics.

Major research opportunities for the future exist in the study of nonlinear phenomena, discrete mathematics, probabilistic analysis, the mathematics of computation, the geometry of three-and four-dimensional manifolds, and many other areas.[3] The infusion of mathematics into society will continue and accelerate, creating further opportunities and increased demand for mathematical scientists.

B. Prospects for the Future

There are reasons to be quite concerned about the future, in spite of current vitality and past achievements. In mathematics, the country is still reaping the harvest of the investment of human and dollar resources made in the mid-to-late 1960s. Investments since that time have not been adequate to assure renewal of the field, to provide the seminal work supporting expanded applications, or to pursue the remarkable opportunities in prospect.

During the past few years, concern about the future of mathematics has been reflected in an unprecedented probing and searching within and by the mathematical sciences community. The state of mathematics, its applications, and its future promise have been assessed in:

- the report of the COSEPUP Research Briefing Panel on Mathematics presented to OSTP and NSF

- its supplementary report to DOD and the DOD-University Forum
- reports to the NSF Advisory Committee for the Mathematical Sciences by J. Glimm, on the future of mathematics, and I. Olkin and D. Moore, on statistics
- the G. Nemhauser/G. Dantzig report on research directions in operations science
- the report of the NSF/DOD Panel on Large-Scale Computing in Science and Engineering
- reports of the NRC Committees on Applied and Theoretical Statistics and on the Applications of Mathematics.

In all of these the theme recurs: in mathematics itself and in its capabilities for application there is a multitude of major opportunities, but the resources, people, and money are not available to capitalize on them.

Our Committee has found the support situation in mathematics to be worse than the preliminary evidence suggested:

Since the late 1960s, support for mathematical sciences research in the United States has declined substantially in constant dollars, and has come to be markedly out of balance with support for related scientific and technological efforts. Because of the growing reliance of these efforts on mathematics, strong action must be taken by the Administration, Congress, universities, and the mathematical sciences community to bring the support back into balance and provide for the future of the field.

III. The Weakening of Federal Support

A. How It Happened

In many ways, the history of support for mathematical research resembles that of other sciences: a rapid buildup of both federal and university support through the 1950s; some unsettling changes in the early-to-mid-1960s; then a slackening of federal support in the late 1960s and early 1970s, because of increased mission orientation of federal R&D and reductions in federal fellowships; and finally, more than a decade of slow growth.

However, mathematics faced special problems, owing to its concentration at academic institutions and its dependence for federal support on two agencies: the National Science Foundation (NSF) and the Department of Defense (DOD).[4] In the mid-1960s, increased focus on mission-oriented research (a change accelerated by the 1969 Mansfield Amendment) caused DOD to drop nearly all of its support of pure mathematical research and parts of basic applied work as well. Then dramatic reductions in federal fellowships beginning in 1971 removed virtually all federal support of mathematics graduate students and postdoctorals. Compensation for these two types of losses could only be made at NSF, but at NSF constant dollar support of mathematical research decreased steadily after 1967. *We estimate the loss in federal mathematical funding to have been over 33% in constant dollars in the period 1968–73 alone; it was followed by nearly a decade of zero real growth, so that by FY 1982 federal support for mathematical sciences research stood at less than two-thirds its FY 1968 level in constant dollars*.[5]

While federal support for related sciences also dipped in 1969–70, these sciences received (constant dollar) increases in NSF funding in the years 1970–72 and thereafter, as well as support from other agencies; mathematics did not.[6] This resulted in the present imbalance between support for mathematics and related sciences:

Comparisons of Federal Support in Institutions of Higher Education for Three Fields of Science, 1980

	Chemistry	Physics	Mathematical Sciences
Doctoral scientists in R&D	9,800	9,200	9,100
Faculty with primary or secondary activity in R&D	7,600	6,000	8,400
Faculty in R&D federally supported	3,300	3,300	2,300
Approximate annual Ph.D. production	1,500	800	800
Graduate research assistants federally supported	3,700	2,900	200
Postdoctorals federally supported	2,500	1,200	50

SOURCES: NRC Survey of Doctoral Recipients, National Science Board—Status of Science Review.

B. Why It Escaped Notice

Three things made it difficult for mathematicians and policy-makers to quickly grasp the full extent of the weakening of support for mathematics:

- After the sharp decline of 196$1$2$33, universities increased their own support for many things which earlier would have been carried by research grants. It was only after financial problems hit the universities in the mid-1970s that the severe lack of resources became evident.
- The growth of computer science support masked the decline in mathematics support because of the federal budget practice of carrying "mathematics and computer science" as a line item until 1976.
- The explosion of the uses of mathematics caused funding to flow into applications of known mathematical methods to other fields. These were often labelled "mathematical research" in federal support data. The category grew rapidly, masking the fact that support for fundamental research in the mathematical sciences shrank.

C. Its Consequences

The absence of resources to support the research enterprises in the country's major mathematical science departments is all too apparent. In most of them, the university is picking up virtually the total tab for postdoctoral support, research associates, and secretarial and operating support; as a result, the amounts are very small. Graduate students are supported predominantly through teaching assistantships, and (like faculty) have been overloaded because of demands for undergraduate mathematics instruction, which have increased 60% in the last eight years. The number of established mathematical scientists with research support, already small in comparison with related fields, has decreased 15% in the last three years. Morale is declining. Promising young people considering careers in mathematics are being put off.

Ph.D.'s awarded to U.S. citizens declined by half over the last decade. A gap has been created between demand for faculty and supply of new Ph.D.'s. It may well widen as retirements increase in the 1990s. There is the prospect of a further 12% increase in demand for doctoral mathematical scientists needed for sophisticated utilization of super-computers in academia, industry, and government.

The most serious consequence has been delayed. In a theoretical branch of science with a relatively secure base in the universities, sharp reduction in federal support does not leave large numbers of scientists totally unable to do their research, as might be the case in an experimental science. There is a considerable time lag before there is a marked slowing down of research output. The established researchers and the young people who were in the pipeline when reduction began carry the effort forward for 15 or 20 years, adjusting to increased teaching loads, to decreased income or extra summer work, and to simply doing with fewer of most things. If the number of first-rate minds in the field is large at the onset of the funding reduction, an effort of very high quality can be sustained for quite some time.

This is what has been happening in the mathematical sciences in the United States for over a decade. The situation must be corrected.

IV. Future Support

A. The Needs of Research Mathematical Scientists

The research community in the mathematical sciences is concentrated heavily at academic institutions spread throughout the country. Over 90% of productive research mathematicians are on the faculties of the nation's universities and colleges. Their numbers equal those of physics or chemistry, some 9,000–10,000.

To pursue research effectively, mathematical scientists need:

1. research time
2. graduate students, postdoctorals, and young investigators of high quality
3. research associates (visiting faculty)
4. support staff (mostly secretarial)
5. computers and computer time
6. publications, travel, conferences, etc.

During the fifties and sixties, these needs were effectively met by the injection of federal funds for research into universities. That spurred remarkable growth and propelled the United States into world leadership in the mathematical sciences. The erosion of support since the late 1960s has slowed momentum and decreased the rate of influx of outstanding young people into the mathematical sciences.

B. A Plan for Renewal

What has been described makes it evident that realization of the potential for mathematics and its applications requires a substantial increase in extra-university support. Because there is often an indirect relation between mathematical developments and their applications, significant support from industry will not be forthcoming. Thus, the role of government is crucial.

Incremental budgetary increases of the usual sort cannot deal with the severe inadequacy of support. We estimate that the federal support needed to strengthen mathematical research and graduate education is about $100 million more per year than the FY 1984 level of $78 million. Significant additional resources are needed in each of the six basic categories we identified earlier. The resources will:

- allow mathematical scientists to capitalize on the future opportunities provided by the dramatic intellectual developments now occurring
- provide for the attraction and support of young people to help renew the field
- sustain the work of established researchers.

As the framework for this, we have determined through analysis the elements of a program to renew U.S. mathematics. This program can be carried out through expansion of support to the $180 million level over the next five years. This National Plan for Graduate and Postdoctoral Education in the Mathematical Sciences has these features:

- Each of the approximately 1,000 graduate students per year who reaches the active level of research for a Ph.D. thesis would be provided with 15 months of uninterrupted research time, preceded by two preceding summers of unfettered research time.
- Two hundred of the 800 Ph.D.'s per year would be provided with postdoctoral positions averaging two years in duration at suitable research centers.

- There would be at least 400 research grants for young investigators (Ph.D. age three to five years).
- At least 2,600 of the established mathematical scientists who, with the young investigators, provide the training for the more than 5,000 total Ph.D. students and the 400 total postdoctorals would have sufficient supported research time not only to conduct their own research, but also to provide the requisite training for these young people.
- Support would be provided for associated research needs of the investigators.

We believe this plan to be consistent with the priorities set by the mathematical sciences research community through several self-studies in the last few years.

C. Implementation

It will be up to the Administration and Congress to decide what national priority to assign to these needs. We would remind them that what is at stake is the future of a field central to the country's scientific and technological effort. While the uses of mathematics in other fields have been supported, somehow the needs of fundamental mathematics were lost sight of for over a decade. Since there is about a 15-year delay between the entry of young people into the field and their attainment of the expected high level of performance, this decade of neglect alarms us. We urge immediate strong action, in the form of a five-year "ramping up" of federal support for the mathematical sciences (18% real growth annually, for five years). An effort to renew mathematics support has already begun at the National Science Foundation. This must be continued for five more years, with a parallel effort at the Department of Defense. This will bring support back into balance and allow for renewal, provided Department of Energy resources going to the mathematics of computation are significantly increased to sustain the initiative which we recommend in this field.

Appropriate utilization of present and future resources requires a well-thought-out and consistent set of priorities in the expenditures of funds. Recommendations of this type have recently been set forth in the COSEPUP Mathematics Briefing Panel Report prepared for OSTP and its companion report specifically for DOD, as well as recent reports of the NSF Advisory Committee for the Mathematical Sciences.

We have built on these community efforts to systematically and consistently direct funding trends. The efforts must continue, to ensure the most efficient and fruitful utilization of resources.

Success will also depend on action and understanding within the nation's universities. For too long, they have been silent about the fact that the level of external support for research in their mathematical science departments is markedly out of balance with the general level of support for science and engineering in the country. The disparity is reflected in the working circumstances of their mathematical faculties and graduate students. As added resources become available, they must be used in part to ease the strain on the mathematical science departments, which embody mathematical research in the United States.

Still, the group which has the fullest agenda before it is the mathematical sciences research community. It is obvious to anyone that if a field gets into the sort of extreme situation we have described, the associated research community must bear much of the responsibility. We urge the mathematical scientists to greatly step up efforts to increase public awareness of developments in the mathematical sciences and of the importance of the broad enterprise to the nation; to set their priorities with long-term needs in mind, and to develop mechanisms for effectively presenting their needs to the universities, to the Administration and to Congress—all with a renewed commitment to the unity of the mathematical sciences.

Notes

[1] Now the Commission on Physical Sciences, Mathematics, and Resources.

[2] In addition, computer science developed from roots in mathematics and electrical engineering, then spun off to become a separate discipline. It is important in reading this report not to confuse computer science with the mathematical sciences. The relationship of the fields is discussed in Appendix A [of the 1984 Report].

[3] These research opportunities are discussed in detail in Chapter II [of the 1984 Report].

[4] The two agencies account for 93% of support. Today, the role of the Department of Energy in supporting work at the interface of mathematics and computation is of ever-increasing importance, however.

[5] FY 1968 was not a peak budget year for mathematical research. It is the year in the period 1966–70 for which we have the most accurate data.

[6] Chemistry and physics constant dollar budgets at NSF dipped in 1969–70, then increased by over 25% in the years 1970–72, and continued to grow until the late 1970s.

Appendix B

Recent Research Accomplishments and Related Opportunities

This appendix describes research achievements in the mathematical sciences and outlines prospects and opportunities that these achievements open up. The richness and diversity of these achievements, which span core mathematics and a wide range of applications, point to the vigor, creativity, and depth and breadth of current research in the mathematical sciences. The unification and cross-fertilization of areas within core mathematics, increased reaching out to applications (which often uncovers unusual and unexpected uses of mathematics), and the growing role of the computer are all themes that are illustrated in these descriptions.

It should be emphasized that this list is only a selection that is not intended to be complete or comprehensive, nor is it intended to be an agenda for the future. Many important achievements and opportunities are not discussed for lack of space. If past patterns continue, a number of new achievements that we cannot visualize now will open up yet newer opportunities. It is interesting and significant to note how many of the achievements described in this appendix were not even suggested in the appendix "Ordering the Universe: The Role of Mathematics" in the 1984 Report.

The committee would like to thank the following individuals for their assistance in preparing this appendix:

W. Ballhaus, N. Breslow, R.L. Bryant, D.M. Burns, S. Childress, W. Cleveland, R.R. Coifman, G.B. Dantzig, H. Flaschka, J. Geanakoplos, J.G. Glimm, L. Gordon, L.F. Greengard, J. Harris, T. Holst, W-C. Hsiang,

A.M. Jaffe, A. Jameson, N. Jewell, D.S. Johnson, R.M. Karp, H. Kocak, A. Kupianinen, H.B. Lawson, F.T. Leighton, C.E. Leith, G.L. Lieberman, A. Majda, A. Marden, B. Mazur, W. Murray, F.M. Odeh, C.S. Peskin, P. Seymour, L.A. Shepp, T.C. Spencer, P. Switzer, M.S. Waterman, S. Winograd, and J.A. Yorke.

The following topics are discussed:

1. Recent Advances in Partial Differential Equations
2. Vortices in Fluid Flow
3. Aircraft Design
4. Physiology
5. Medical Scanning Techniques
6. Global Change
7. Chaotic Dynamics
8. Wavelet Analysis
9. Number Theory
10. Topology
11. Symplectic Geometry
12. Noncommutative Geometry
13. Computer Visualization as a Mathematical Tool
14. Lie Algebras and Phase Transitions
15. String Theory
16. Interacting Particle Systems
17. Spatial Statistics
18. Statistical Methods for Quality and Productivity
19. Graph Minors
20. Mathematical Economics
21. Parallel Algorithms and Architectures
22. Randomized Algorithms
23. The Fast Multipole Algorithm
24. Interior Point Methods for Linear Programming
25. Stochastic Linear Programming
26. Applications of Statistics to DNA Structure
27. Biostatistics and Epidemiology

Synopsis of Topics

In the first section, "Recent Advances in Partial Differential Equations," the items discussed are formation of shocks in non-linear waves, recent advances in elliptic equations, free boundary problems, and finally some remarkable advances in exactly solvable partial differen tial equations. "Vortices in Fluid Flow" (Section 2) continues some of these general themes to discuss vortex motion in fluid flow, a phenomenon of great importance in many applications, including the accurate tracing of hurricanes, the study of blood flow through the heart, the efficient mixing of fuel in internal combustion engines, aircraft flight, and the manner in which radiotelescopes sense distant galaxies through the motion of galactic jets.

"Aircraft Design" (Section 3) illustrates the use of computational fluid dynamics, a technique that has matured so that it is now seen as the primary aerodynamic design tool for any problem. Analogous computer models are described in Section 4, "Physiology," which discusses computational fluid models of the heart and other organs. Modern medical scanning techniques using X-rays or nuclear magnetic resonance depend critically on algorithms deriving from the mathematics of the Radon transform. Recent progress in emission tomography is based on some newly developed algorithms of a very different sort that have probabilistic elements; these developments are described in Section 5, "Medical Scanning Techniques." Finally, "Global Change" (Section 6) discusses the key role played by computational fluid dynamics in global circulation models that are used in the analysis of climate change on a worldwide scale.

Section 7, "Chaotic Dynamics," shows how ideas of Poincaré on aperiodic orbits for ordinary differential equations, complemented by ideas from topology, differential geometry, number theory, measure theory, and ergodic theory, plus the ability of modern computing facilities to compute trajectories, have led to a body of core mathematics that has many interesting and important applications.

"Wavelet Analysis" (Section 8) outlines how classical ideas growing out of Littlewood-Paley and Calderón-Zygmund theory have been developed within core mathematics and then have led to new and very efficient numerical tools for analysis of a wide variety of problems. Algorithms based on wavelet analysis promise to significantly speed up communications and signal-processing calculations. The discussion titled "Number Theory" (Section 9) centers on a classic area of core mathematics that is actively and vigorously moving forward, spurred on in part by the resolution of the Mordell conjecture in the early 1980s. Advances of great significance for the future include new results on the local-to-global problem in number theory and in arithmetic algebraic geometry, and significant progress on Fermat's last theorem. Section 10, "Topology," notes important advances in major problems in low-dimensional topology, including remarkable connections with Yang-Mills theory, and recent advances in knot theory that involve a striking and unexpected connection with von Neumann algebras and mathematical physics.

Section 11, "Symplectic Geometry," is devoted to important recent developments in that field, including the use of nonlinear elliptic equations to establish a form of the Heisenberg uncertainty principle, the discovery of new kinds of symplectic structures, and a basic advance in the understanding of regions of stability for area-preserving maps of the plane.

"Noncommutative Geometry" (Section 12) describes a broad spectrum of very interesting developments involving a link between analysis and geometry and how the ideas of differential geometry extend to a noncommutative setting. This is an excellent example of cross-fertilization between areas within core mathematics and the building of an internal unification.

The availability of powerful computers is stimulating research in core mathematics. Section 13, "Computer Visualization as a Mathematical Tool," indicates how computer graphics can be used as a tool in minimal surface theory and other areas of geometry to enhance understanding and provide experimental evidence stimulating conjectures.

The exposition in Section 14, "Lie Algebras and Phase Transitions," displays the rich and deep interaction between this topic in statistical mechanics and a number of areas of mathematics, including especially the Kac-Moody Lie algebras. "String Theory" (Section 15) discusses another topic in physics that relies heavily on developments from core mathematics. Section 16, "Interacting Particle Systems," indicates how systems similar to those discussed in the context of phase transitions can have applications in the study of biological systems, image processing, and for medical and defense purposes. "Spatial Statistics" (Section 17) describes an area that addresses some overlapping problems and uses new statistical tools for handling data in multidimensional arrays. Section 18, "Statistical Methods for Quality and Productivity," discusses problems, opportunities, and new methods for addressing important problems of national interest and significance.

"Graph Minors" (Section 19) surveys some recent results in graph theory, which open up new avenues for research especially important in the design of algorithms. Section 20, "Mathematical Economics," describes some important recent developments and discusses how several parts of core mathematics, especially differential topology, have played key roles in the analysis of general equilibrium theory for incomplete markets, a new departure that is a better model for real markets than the now classic model for complete markets.

The next group of topics have as a common general theme the search for new and efficient algorithms. "Parallel Algorithms and Architectures" (Section 21) concerns the design of algorithms to take advantage of parallel architectures, a problem not only for computer scientists but also for mathematicians working in large-scale computation. Here the idea is to see how problems that seem to be inherently sequential can be parallelized. Section 22, "Randomized Algorithms," describes recent progress in the development of these new kinds of algorithms. Such algorithms are useful in primality testing, with resulting consequences for cryptography, in sorting and searching algorithms, in the design of distributed computing systems, and in many other areas. The subject of Section 23, "The Fast Multipole Algorithm," is a new, very efficient algorithm for computing interactions in many-particle systems. This algorithm will have many applications in the modeling of high-powered electron beam devices and in molecular dynamics, which affects theoretical studies of chemical kinetics.

The next two sections discuss recent advances in algorithms for numerical optimization; Section 24 is devoted to the new and very important interior point methods for linear programming, which provide an alternative to the classic simplex methods and are beginning to have a significant practical impact in the design of telecommunications networks and the solution of large-scale logistics planning and scheduling problems. Section 25 discusses yet another approach—stochastic linear programming, a technique that allows one to include non-deterministic elements in the formulation and solution of a problem. Thereby real problems that involve uncertainties in future behavior or availability of resources can be better modeled.

Sections 26 and 27 discuss an array of applications of mathematics in various additional areas. "Applications of Statistics to DNA Structure" includes as an application the statistical analysis of options for cutting the DNA sequence to aid in the mapping processes, and analyzing the evolutionary process at the genetic level. "Biostatistics and Epidemiology" is devoted to the use of statistics in epidemiology, including survival analysis, analysis of incidence rate and relative risk, and deconvolution techniques for estimating infection rates and incubation periods from observed data.

1. Recent Advances in Partial Differential Equations

An important trend of the last 15 years has been the great progress made in understanding nonlinear partial differential equations (PDEs). Many physical phenomena are described by partial differential equations, e.g., fluid flow, electromagnetic fields, gravity, and heat. Roughly speaking, linear partial differential equations govern small vibrations or small disturbances from equilibrium, while nonlinear equations govern large disturbances. The real world is nonlinear. Since the mid-1970s, understanding of nonlinear equations has grown much deeper. Finally, in the last few years, some of the most important equations from geometry, physics, and engineering have been successfully studied. Many other equations are still too hard, and much more work is needed. Among the important problems solved recently are the following.

Formation of Shocks in Nonlinear Waves

In one space dimension, a small, smooth initial disturbance will be propagated by any truly nonlinear wave equation into a shock after a finite time. In more than four space dimensions, such shocks do not have to form. In three dimensions, "most" wave equations lead to shocks, but only after an exponentially long time. Moreover, an important class of equations (those satisfying a natural geometric property called the "null condition") do not build up shocks. Very recently there have been significant advances for one of the most important nonlinear equations, Einstein's equations for gravitational waves. At large distances and after a long time, one has a detailed picture of how gravitational waves behave. Very difficult and interesting questions remain in the study of Einstein's equations. Of special interest is the formation of black holes.

Elliptic Equations

Another important class of partial differential equations arises in geometry, when one tries to construct surfaces with prescribed curvature. These equations are called nonlinear elliptic differential equa tions. A general theorem on regularity of solutions of elliptic equations with boundary conditions was recently proved, making it possible to treat boundary conditions that arise inevitably in real problems. This result is a basic step forward in the analysis of partial differential equations.

Important progress has been made also on some singular elliptic equations, namely those having "critical nonlinearity." If the nonlinear term in such an equation were made slightly weaker, then the equation could be regarded as a small perturbation of a linear problem, but at the critical nonlinearity this becomes impossible. An outstanding equation of this kind occurs in the Yamabe problem, in which one is asked to deform a curved manifold until it has constant (scalar) curvature. A complete solution to this problem has recently been established.

Free Boundary Problems

An iceberg melting in the sea, the flow of oil and water through a reservoir, and crystal growth are examples of free boundary problems governed by partial differential equations. For the melting iceberg, the temperature flow in the iceberg is governed by one parabolic partial differential equation, the temperature flow in the water around the iceberg by another, and the boundary between ice and water is given by a third equation. The three equations are coupled. What makes the problem very hard is the fact that the domains where the differential equations are satisfied keep changing with time, and are not known ahead of time. Recently proposed techniques have already led to new regularity theorems for free boundary problems and promise further results.

Exactly Solvable Partial Differential Equations

Remarkably, a number of nonlinear PDEs can be solved exactly. These equations admit stable solutions (solitons) that persist even after interaction with other solitons. Recently, equations for solitons have been used to solve the Schottky problem, an outstanding open problem in the theory of Riemann surfaces.

The method used to solve soliton equations may be illustrated by the case of the Korteweg-deVries (KdV) equation, which describes the propagations of water waves in a long, narrow channel. At a single instant of time, we imagine the shape of the water wave to be frozen and rigid. We then bombard the rigid shape with imaginary quantized test particles. By studying how the test particles are scattered, one can reconstruct the shape of the wave. Thus, the scattering data provide an alternative description of the wave at a fixed time. Instead of asking how the shape of the wave changes with time, we can therefore ask how the scattering data evolve with time. When rewritten in terms of scattering data, the KdV equation becomes amazingly simple, and the complete solution may be written down by inspection. In particular, the highly stable behavior of solitons is explained for the case of the KdV equation.

More recently, a number of physically interesting PDEs have been solved completely by analogous methods, including the Kadomtsev-Petviashvili (K-P) equation for weakly two-dimensional water waves, and the sine-Gordon and nonlinear Schrödinger equations. Explicit solutions of the K-P equation successfully predicted the results of experiments in water tanks, and a combination of theoretical and numerical analysis has been applied to model the behavior of a Josephson junction. Remarkable connections have been discovered between explicit solutions for nonlinear waves, exact solutions of statistical mechanics problems in two dimensions, and the Jones polynomials for knots, some of which are discussed below in sections on phase transitions and topology.

2. Vortices in Fluid Flow

Intense swirling or vortex motion is a primary feature of many problems, including the accurate tracing of hurricanes and studies of blood flow through the heart, efficient fuel mixing in carburetors, aircraft flight, and the manner in which radiotelescopes sense distant galaxies through the motion of galactic jets.

Governing much of this flow is a complicated set of nonlinear partial differential equations called the Navier-Stokes equations; these equations are derived from Newton's laws of motion and include the frictional effects of viscosity. Intuition suggests that this frictional effect is extremely small in air or rapidly moving water, and this is confirmed by experiments. The simpler partial differential equations obtained when this coefficient vanishes are called Euler equations. These are accurate enough for studying the movement and accumulation of vortices.

Recent ingenious large-scale computer simulations using these equations reveal unexpectedly that the sheets where vorticity typically accumulates clump and concentrate in an amazing fashion. In response to these discoveries, a new mathematical theory of "oscillations and concentrations" has developed using potential theory and fractal (Hausdorff) measures. New kinds of "solutions" for the Euler equations are being introduced. One outgrowth of this theory is an explicit criterion to check whether numerical calculations for vortex sheets actually converge to solutions of the Euler equations. Convergence has been verified for many calculations of importance.

The vortex sheets in the applications just described involve fluid moving at rapid speed but still much less than the speed of sound. Completely different phenomena transpire when the vortex sheets are supersonic, as they are for the new space planes and for galactic jets in astrophysics. One recent success in the alliance between large-scale computing and modern mathematical theory is the discovery of a new mechanism of nonlinear instability for supersonic vortex sheets. Recent large-scale simulations have demonstrated that all supersonic vortex sheets exhibit nonlinear instability, belying the predictions of stability made in the 1950s and 1960s.

One of the most important problems in fluid dynamics, an extension of the study of vortices, is the understanding of turbulence, which occurs when the frictional effect is extremely small but not negligible. Understanding turbulence requires the mathematical analysis of solutions of the Euler and the Navier-Stokes equations in the limit of small viscosity. This analysis is ongoing.

3. Aircraft Design

Within the last five years, full simulations of a whole aircraft have appeared. Such a computation usually starts with steady Euler equations that accurately describe the flow outside the boundary layer. Such flows are smooth until the Mach number, M, comes close to 1. For Mach numbers in the transonic range—that is, less than but close to 1—small shocks are generated from a typical airfoil that dramatically increase the drag. It is a mathematical theorem that in almost all cases such shocks cannot be avoided. Since the cruising efficiency of a plane is roughly proportional to ML/D, where L is lift and D is drag, it is imperative for manufacturers to design aircraft that minimize shocks. Of course if M exceeds 1, there is no avoiding or even minimizing shocks, and we have the inefficiency of the Concorde. In the past 15 years, great effort has been put into designing two-dimensional airfoil cross-sections that at some cruising speed or range of cruising speeds with M less than 1 have minimal shocks. When a wing cross-section is chosen, the flow at design conditions is computed and compared with wind tunnel results.

To extend the computation to the whole aircraft, new computational capabilities have been added. The complex geometrical configurations demand new methods not only for discretizing the equations but also for handling the enormous volume of data. Currently the challenge is to resolve higher-dimensional shocks and vortex sheets to predict viscous effects as described in the previous section. The most useful end product of simulations is a determination of how surface pressure varies with such parameters as Mach number and the angle of attack. Varying parameters on the computer is much more economical than doing enormous numbers of experiments in a wind tunnel.

The simple model provided by the Euler equations is remarkably good, airplane flight being basically stable. But key elements are missing. Despite considerable effort, there is still no good mathematical model for the turbulent boundary layer, and when one is found it will increase the size of the computation at least as much as by adding a dimension. An ultimate goal of design is to pick an optimal pressure distribution and then find the aircraft shape that corresponds to it. Such inverse problems also increase drastically the computational needs. The hope is that computer hardware speedups and algorithmic improvements will combine to make these goals achievable.

One area of particular note is in the design of aircraft engines. A typical example is a turbomachinery compressor simulation where instantaneous temperature contours are calculated. This computation is based upon time-dependent Navier-Stokes equations. Simulations show viscous wakes created by the blades and how some of the blades chop or break these wakes into different pieces, creating an extremely complex flow pattern. This flow pattern would be difficult or impossible to describe and adjust without dependable mathematical models coupled with computer simulations.

For very-high-altitude high-speed conditions, numerical simulations are also being used for vehicle design. At these altitudes, air can dissociate into its atomic constituents and even eventually ionize, creating a situation that is extremely difficult to simulate in ground-based experimental facilities. As a result, numerical flow simulations, with the appropriate air chemistry models added, are currently being used as an integral part of the design process for many high-speed or atmospheric entry vehicles.

The best summary of the situation has been given by Goldhammer and Rubbert:

> The present state-of-the-art has progressed to the point where the design engineer no longer considers Computational Fluid Dynamics (CFD) to be an irritant imposed on him by a seldom seen researcher, but rather CFD is regarded as the primary aerodynamic design tool for any problem, and the wind tunnel is treated as more of an evaluation and confirmation tool.[1]

4. Physiology

In the realm of physiology mathematical modeling has come into its own over the last ten years. Today there are computational models of the heart, the kidney, the pancreas, the ear, and many other organs. Many of these models rely on fluid dynamics.

For instance, blood flow in the heart is governed by coupled equations of motion of the muscular heart walls, the elastic heart valve leaflets, and the blood that flows in the cardiac chambers. Computer solutions allow one to study both normal and diseased states, and lead to the design of prosthetic devices such as artificial valves and artificial hearts. The methods used have a very general character since they are applicable to any problem in which a fluid interacts with an elastic medium of complicated geometry. Among these are the flow of suspensions, blood clotting, wave propagation in the inner ear, blood flow in arteries and veins, and airflow in the lung. Like much of computational fluid dynamics, this work pushes computer technology

Physiological fluid dynamics has a long and illustrious history. Leonardo da Vinci first described the vortices that form behind heart valves and that enable the valves to avoid backflow by closing while the flow is still in the forward direction. Leonhard Euler first wrote down the partial differential equations for blood flow in arteries. With the recent flowering of computer technology and numerical algorithms, there is unprecedented opportunity to simulate the fluid dynamic functions of the human body at a level of detail sufficient to be of use in the understanding and treatment of disease.

to its limits, and future progress is strongly tied to the further development and availability of supercomputers.

Early research began with the development of a two-dimensional computer model of the left side of the heart. The model was designed for computer experiments on the mitral valve, which has the appropriate symmetry for two-dimensional studies. The computer experiments were successfully compared with physiological experiments, such as those studying the optimal timing of the atrial contraction in relation to that of the ventricular contraction. The computer model was trustworthy enough to use for parametric studies leading to optimal designs of prosthetic cardiac valves.

With supercomputers, it has become possible to extend this work to three dimensions. This raises the prospect of additional applications such as flow through the aortic valve, the mechanical consequences of localized damage to the heart wall, interactions of the right and left ventricle, flow patterns of blood in the embryonic and fetal heart, the fluid dynamics of congenital heart disease, and the design of ventricular-assist devices or even total artificial hearts.

A general-purpose three-dimensional fiber fluid code has already been developed that solves the equations of motion of a viscous incompressible fluid coupled to an immersed system of elastic or contractile fibers, using the vector architecture of the Cray. The fiber-fluid code has been tested on problems involving an immersed toroidal tube composed of two layers of spiraling fibers. In one of these tests, the fibers were contractile (i.e., muscular) and peristaltic pumping was achieved by sending a wave of muscle contraction around the tube. With a sufficiently strong contraction, a small region of entrained fluid was seen being convected along at the speed of the wave.

A three-dimensional fiber-based model of the four-chambered heart and the nearby great vessels is now under construction for use with the general-purpose fiber-fluid code described above. It includes the right and left ventricles, realistic aortic and pulmonic valves complete with sinuses and the beginnings of their respective arteries, and preliminary versions of the mitral and tricuspid valves.

5. Medical Scanning Techniques

Significant progress in inverse problems in medicine has occurred in the last five years. CAT scanning itself is no longer a research topic but is an established, fixed technology. The new advances have occurred in magnetic resonance imaging (MRI) and in emission tomography, which are similar to CAT scanning from a superficial mathematical viewpoint in that they each involve indirect measurements of a three-dimensional quantity or image of interest and then use mathematical inversion of the measured quantities to reconstruct the actual image.

In MRI a large magnet and surrounding coil measure the resonating magnetic field inside the patient, due to an unknown density of hydrogen atoms, which act like little spinning magnets. The mathematics used to reconstruct the hydrogen density uses the inverse Fourier transform applied to the measured signal. This allows the determination of the density of magnetic spins, or the concentration of hydrogen atoms inside the patient, which in turn gives an image of the interior tissue similar to but much better than a CAT scan. Bones appear black instead of white because, while they have a high X-ray attenuation density, they have a low hydrogen density, being largely calcium. Just as in CAT scanning, mathematics is one of the chief technologies in MRI, the main feature being fast and accurate inversion of the Fourier transform.

The new mathematics of emission tomography (ET) is very different from that of either CAT or MRI and involves a nonlinear inversion procedure. In ET a compound such as glucose is introduced into the body with the carbon atoms in the glucose being radioactive isotopes of carbon. Counts of radioactivity are measured in a bank of detectors surrounding the body. One mathematically inverts the detected count data and reconstructs the emission density; i.e., one finds where the radionuclide was deposited by the body's metabolism. A beautiful and elegant new algorithm produces an emitter-density that to a first approximation maximizes the probability of seeing the actual observed counts. This statistically based maximum likelihood algorithm has the great advantage that it addresses the main limitation of ET, namely that it is count-limited. The mathematics involves no Fourier transforms, but instead the convergence of a nonlinear iteration scheme. Given the universality of mathematics, it should not be surprising that the algorithm is new only to ET: it is a known algorithm that first arose in the 1960s in a problem in decryption of Soviet codes. Emission tomography has so far been mainly used not as a clinical tool, but to study metabolism in the human being.

6. Global Change

Of the many environmental issues that have received public attention in recent years, perhaps the most far-reaching is the possible effect of human activity on the earth's climate. The greenhouse theory holds that recent modification of the atmospheric gaseous composition will result in a gradual warming of the earth's surface as well as a cooling of the upper atmosphere, leading to an unprecedented (in historical times) modification of the earth's climate. However, natural climatic changes can mask this increase, and there is a critical need to study quantitatively the magnitude of the greenhouse effect within a global climate model under various scenarios, past, present, and future.

The basic theoretical principles rest on the notion of an equilibrium climate, where incoming solar radiation, which is absorbed in the atmosphere and at the surface of the earth, must equal the thermal energy radiated out into space. This balance determines the average temperature at the surface of the earth. The greenhouse effect occurs when the outgoing radiation is partially absorbed by particles and molecules in the upper atmosphere, or troposphere, principally the top 10 to 15 kilometers.

Three-dimensional general circulation models provide a means of simulating climate on time scales relevant to studies of the greenhouse effect. These models, which numerically solve a nonlinear system of partial differential equations, are being used to compute differences between a climate forced by increases in greenhouse gases and a control or current climate. The underlying equations are Euler's equations, with simplifications to take into account the thinness of the atmosphere relative to the distances across the surface of the earth. Long-term predictions must account for thermal adjustment of the oceans, over a time scale of decades, and models need to be devised that are suitable for this purpose. It is important to track not only mean surface temperatures, but also spatial and temporal changes in temperature variability, which can have equally important consequences. These studies will require accurate codes and precise estimates of sensitivity to forcing by the various greenhouse gases on a variety of time scales. As in other geophysical flow calculations, reliable turbulence models are needed in order to estimate turbulent transport.

At a more theoretical level, a basic goal should be to identify the "minimal" dynamical description of the atmosphere-ocean-land that could, on the time scale of decades, provide reliable estimates of climatic change. Methods from dynamical systems theory (see the next section) can be used to reduce the dimension of the system and thereby isolate an "attractor" involving only the dynamical variables essential to greenhouse studies. Detailed but economical calculations of climate sensitivity might then be accessible, giving a new understanding of the influence various incremental combinations of greenhouse gases have on the equilibrium climate.

7. Chaotic Dynamics

The early observations of trajectories of celestial objects appeared to indicate periodic or, at worst, quasiperiodic behavior that could easily be accounted for in mathematical terms. But at the turn of the twentieth century, Poincaré realized that the behavior of trajectories of celestial bodies could be immensely complicated, displaying a "chaotic" motion, forever oscillating yet irregular and aperiodic. Moreover, Poincaré identified a crucial property of systems with chaotic trajectories—sensitive dependence on initial data, which is of particular importance for scientists because very small errors in the measurement of the current system state would result in very unrealistic long-term predictions.

In 1963, a detailed numerical examination of a specific system of differential equations from meteorology revealed unexpected chaotic trajectories. This work not only pointed out the presence of chaotic trajectories in a specific non-Hamiltonian system but also suggested new directions of research in the theory of dynamical systems. Mathematicians and scientists have come to recognize that the amazingly complicated behavior that Poincaré spoke of, and that was demonstrated in these calculations for new kinds of attractors, was in fact present in a wide variety of practical nonlinear systems from ecology, economics, physics, chemistry, engineering, fluid mechanics, and meteorology.

The advent of the computer was essential to these developments, but equally important were the deep mathematical insights. Indeed, the theory of dynamical systems has a rich mathematical tradition, one that involves many areas of mathematics: topology, number theory, measure and ergodic theory, and combinatorics have all been essential to the understanding of dynamical systems, especially the ones exhibiting chaotic behavior. For instance, in dynamical systems with two or more attractors (that is, several types of long-term behavior depend ing on the initial state), the ability to predict long-term behavior requires a detailed knowledge of the boundary between these different kinds of initial states. These boundaries can be extremely complicated and strange —"fractals." These fractal basin boundaries are currently under investigation by scientists, who need to understand physical systems, and by topologists who see fascinating mathematical structures occurring in a natural way.

A further important development was the realization that regularities can be expected whenever chaos arises through an infinite series of "period doubling bifurcations" as some parameter of the system is varied. Ideas explaining these regularities eventually led to rigorous proofs of the phenomenon. The first rigorous proof carried out the detailed analysis on a computer using a procedure called interval analysis: all calculations are performed with error bounds, so that results lie in intervals. The computer adds or multiplies intervals in which the correct results lie so that all errors are perfectly bounded.

In the analysis of dynamical systems, there is a great need to compute dynamical entities other than chaotic attractors. Mathematicians are now beginning to create new numerical methods for computing the stable and unstable manifolds of which Poincaré spoke. In a related vein, identification of "inertial manifolds" for partial differential equations is a promising route in the quest to reduce the essential dynamics of an infinite-dimensional dynamical system to that of an appropriate finite-dimensional one. Finally, mathematical investigations of dynamical systems without the concern of immediate applicability, such as work describing complicated flows on spheres, have yielded important insights.

8. Wavelet Analysis

Wavelet analysis, a recent and exciting development in pure mathematics based on decades of research in harmonic analysis, is now addressing important applications in a wide range of fields in science and engineering. There are opportunities for further development of both the mathematical understanding of wavelets and their ever-expanding applications.

Like Fourier analysis, wavelet analysis deals with expansions of functions, but in terms of "wavelets." A wavelet is a given fixed function with mean 0, and one expands in terms of translates and dilates of this function. Unlike trigonometric polynomials, wavelets are localized in space, permitting a closer connection between some functions and their coefficients and ensuring greater numerical stability in reconstruction and manipulation. Every application using the Fast Fourier Transform (FFT) could be formulated using wavelets, providing more local spatial (or temporal) and frequency information. In broad terms, this affects signal and image processing and fast numerical algorithms for calculations of integral operators (not necessarily of convolution type).

Wavelet analysis is an outgrowth of 50 years of mathematics (Littlewood-Paley and Calderón-Zygmund theory) during which harmonic analysts, having realized the difficulties inherent in answering the simplest questions involving the Fourier transform, developed simpler flexible substitutes. Independent of this theory within pure mathematics we have seen variations of this multiscale (multiresolution) approach develop (over the last decade) in image processing, acoustics, coding (in the form of quadrature mirror filters and pyramid algorithms), and in oil exploration. As a companion to the FFT it has been used in analyzing rapidly changing transient signals, voice and acoustic signals, electrical currents in the brain, impulsive underwater sounds, and NMR spectroscopy data, and in monitoring power plants. As a scientific tool it has been used in sorting out complicated structures occurring in turbulence, atmospheric flows, and in the study of stellar structures. As a numerical tool it can, like the FFT, reduce considerably the complexity of large-scale calculations by converting dense matrices with smoothly varying coefficients into sparse rapidly executable versions. The ease and simplicity of this analysis have led to the construction of chips capable of extremely efficient coding and compression of signals and images.

9. Number theory

There has been impressive progress in number theory in the past five years on several fronts, which in turn has opened up exciting new opportunities. One achievement has been a significant advance in our understanding of what is known as the "local-to-global" principle of certain algebraic curves. The idea of the "local-to-global" principle, which is contained in the classical theorem of Hasse-Minkowski, has to do with a single homogeneous quadratic equation (in many variables) and its solutions. To illustrate it, take a simple case

$$a \cdot W^2 + b \cdot X^2 + c \cdot Y^2 + d \cdot Z^2 + e \cdot WX + f \cdot YZ = 0,$$

where the coefficients a,b,c,d,e,f are integers. With these coefficients fixed, the question one wishes to resolve is, Are there integer values of the variables W,X,Y, and Z (not all zero) that "solve" the above equation, i.e., for which the left-hand side is zero? The Hasse-Minkowski theorem answers yes, if and only if (1) there is a positive integer D such that for each integer N there are integer values of W,X,Y,Z (depending on N) having greatest divisor D and satisfying the constraint that $a \cdot W^2 + b \cdot X^2 + c \cdot Y^2 + d \cdot Z^2 + e \cdot WX + f \cdot YZ$ is a multiple of N; and (2) there are real numbers W,X,Y,Z that "solve" the above equation.

There are reasons (which aid one's intuitive grasp of such problems) to think of the criteria (1) and (2) as asserting the existence of nontrivial local solutions of the equation, while integral solutions of the equation can be thought of as global solutions. This explains the adjective "local-to-global."

The Hasse-Minkowski theorem has been an enormously fruitful theme for research in the quest of broader domains in which the "local-to-global" principle or some modification of it remains valid. One may ask, To what extent does a similar result hold for homogeneous equations of degree higher than 2? It is an old result that such a principle is false for curves of degree 3.

A substitute for the "local-to-global" principle for equations of the above type ("curves of genus one"), which would be (if precisely controlled) every bit as useful as the original "local-to-global" principle, is the conjecture of Shafarevitch and Tate. This conjecture says that, accounting for things in an appropriate way, the "failure of the local-to-global principle" is measured by a finite group. This Shafarevitch-Tate group, which measures the extent to which the local-to-global principle is not valid, is an important object in its own right: it is the gateway to any deep arithmetic study of elliptic curves, and to the very phrasing of conjectures that guide much research in this area. The conjectures of Shafarevitch-Tate and related ones of Birch and Swinnerton-Dyer comprise some of the great long-range goals of the discipline, and a piece of them has recently been established in a very interesting context.

Another facet of number theory that has seen an enormous amount of activity is arithmetic algebraic geometry. This has figured prominently in work on the arithmetic Riemann-Roch theorem, in the unifying conjectures connecting Diophantine problems to Nevanlinna the ory and to differential geometry, and in recent results giving a new and very "geometric" proof of Mordell's conjecture over function fields—a proof that may translate to an analogous new proof in the number field context.

A third significant development of the last five years consists of the conjectures and results that bring some old Diophantine questions closer to the heart of issues perceived to be central to immediate progress in arithmetic. One might mention the recent conjectures of Serre, which suggest much important work to do in the theory of modular forms. Following these ideas, using a prior ingenious construction, it has recently been shown that the Shimura-Taniyama-Weil conjecture (that all elliptic curves over Q are modular) implies Fermat's last theorem. Finally, a beautiful and simple conjecture (often called the ABC conjecture) has been formulated: there is a universal constant e such that, if A and B are nonzero integers with $C = A + B$, then $|A \cdot B \cdot C|$ is less than the e th power of the radical of $A \cdot B \cdot C$, where the radical of a number is the product of the distinct primes dividing it. An essentially immediate consequence of the ABC conjecture is an "asymptotic" version of Fermat's last theorem. It is also true that a vast number of other deep consequences would follow from the same conjecture.

10. Topology

Two of the most basic problems in topology are the following.

I. Suppose one is given two manifolds (the n-dimensional generalization of surfaces) *M* and *M*'. How can one recognize whether the two manifolds are topologically the same, like a sphere and an elliptical surface, or whether they are topologically different, like a sphere and a torus (inner tube)? We seek invariants to distinguish between different manifolds.

II. Suppose that one manifold *K* is embedded in a higher-dimensional manifold *M* in two different ways. Is it possible to deform one embedding into the other? In the most basic and classical case, one studies an embedding of the circle *K* into ordinary three-dimensional space *M*. Such an embedding is a knot, and the goal is to understand when a given knot can be untied, and more generally when one given knot can be deformed into another. Again, invariants are sought to distinguish nonequivalent embeddings of *K* into *M*.

For manifolds *M* of dimension 5 and above, these problems were essentially solved in the late 1960s and early 1970s. Dimensions 3 and 4 are much harder, and are still far from being completely understood. Nevertheless, there has been dramatic progress on low dimensions in the last few years.

For instance, for problem (I) in 4 dimensions, the role of smoothness of possible equivalences of manifolds *M* and *M′* has come to the fore. Recently the following remarkable example was found: there is a smooth 4-dimensional manifold *M* that is topologically equivalent to ordinary 4-dimensional Euclidean space R^4 by a "crinkly" non-smooth map, but that cannot be transformed smoothly to Euclidean space. Perhaps even more amazingly, it has since been learned that the number of such different examples is uncountably infinite. This phenomenon occurs in no other dimension except 4. It came as a complete surprise, both because dimension-4 behavior is so different from the previously known behavior of other dimensions, and because of the remarkable source of the discovery. In fact, the key invariants used to distinguish the exotic 4-manifold from ordinary Euclidean space have their origin in the study of the Yang-Mills equations, originally introduced in particle physics. Thus, an important connection has arisen between particle physics and topology.

Important invariants for the study of problem (I) were also discovered for dimension 3. These invariants, called Casson invariants, recently shed light on a classical and fundamental problem of topology, the Poincaré conjecture. The Poincaré conjecture states that in 3 dimensions, the sphere is the only possible manifold (closed without boundary) whose simplest topological invariant, the fundamental group, is zero. The analogue of this conjecture has been proved in all dimensions except 3. The 4-dimensional case was done as part of the work described above. However, the 3-dimensional case is very difficult, and the statement may actually be false. A standard attempt to produce a counterexample was based on the so-called Rochlin invariant. The Casson invariants work shows that this line of attack cannot yield a counterexample.

Another remarkable recent development in topology concerns Problem (II), in the classical context of knots. The main invariant in classical knot theory was the Alexander polynomial, developed in the 1930s. A weakness of the Alexander polynomial is that it fails to distinguish between a knot and its mirror image. In 1984, in the course of study of questions on von Neumann algebras (a branch of functional analysis motivated by quantum mechanics), formulas were discovered that bore a striking similarity to classical algebraic formulas from the study of knots and braids. Pursuit of this connection led to the discovery of a powerful new invariant of knots and links, now called the Jones polynomial, which has the important advantage that it distinguishes a knot from its mirror image. It is also easy to compute.

Both the Jones polynomial and the work on exotic 4-manifolds arose through mathematical problems with strong connections to physics. This led to the conjecture that a quantum field theory, an exotic variant of the laws of particle physics, could be constructed, in which the experimentally observable quantities are the invariants described. Such a quantum theory has recently been constructed, and although the work is highly plausible, it has not been rigorously proved. Finding a complete, rigorous proof of these calculations is a challenge for future research.

11. Symplectic Geometry

A fundamental development of nineteenth-century mathematics was Hamiltonian mechanics. A mechanical system composed of many particles moving without friction is governed by a complicated system of differential equations. Hamilton showed that these equations take a simple standard form when the Hamiltonian (the total energy of the system) is taken as the starting point. Hamiltonian mechanics revealed hidden symmetries in classical mechanics problems and was of tremendous importance in the discovery of statistical mechanics and quantum theory.

Today, mathematicians study Hamiltonian mechanics from a global and topological point of view. The basic object of study is a "symplectic manifold," a higher-dimensional surface on which Hamilton's procedure to pass from Hamiltonian functions to differential equations can be implemented. The study of symplectic manifolds is called symplectic geometry, and it has been revolutionized in the last few years. A major breakthrough was the use of nonlinear elliptic equations (see Section 1, "Recent Advances in Partial Differential Equations") and holomorphic curves. This yields a form of the Heisenberg uncertainty principle with many applications, including demonstrating the existence of exotic symplectic structures on Euclidean space and leading to the solution of long-standing conjectures on the number of fixed points of symplectic transformations. The solution of these conjectures is in turn closely related to the Floer cohomology of topology.

If we restrict a symplectic structure to a surface of constant energy, we get a "contact structure." Along with the recent progress in symplectic geometry has come important work on contact structures. In particular, exotic contact structures have been discovered on Euclidean space, distinguished from the standard contact structure by "over-twisting" on embedded discs. Contact geometry has been recently used to show that any open manifold free of obvious topological obstructions can be given a complex structure and embedded into complex N-dimensional Euclidean space. (This works in dimension 6 and above.) The result thus relates complex analysis, contact geometry, and topology.

There is a very substantial branch of mathematics of the border line between symplectic geometry and dynamical systems. It deals with the iteration of area-preserving maps of the plane. Such maps φ are the most basic examples of symplectic transformations. In addition to their theoretical importance in core mathematics, they arise in a range of applications from the orbits of asteroids to the confinement of plasmas in a Tokamak machine. As explained in the section on chaotic dynamics, iteration of φ can lead to highly complicated unstable behavior.

In the area-preserving case, however, the chaotic behavior coexists with a large class of orbits that are completely stable and predictable, and indeed are almost periodic. Such stable behavior occurs on a family of curves, called KAM curves, that surround those fixed points of φ where the map twists. The discovery of KAM curves was a major development of the 1950s and 1960s. It is an important and difficult problem to understand how the plane splits into regions of stable and unstable behavior. A particular case of this problem is to predict the size of the largest KAM curve. This is significant for applications, because the old KAM theory unfortunately could deal only with tiny curves. KAM curves of reasonable size were proved to exist in the last ten years. Recently, with computer-assisted methods, the sizes of the largest KAM curves for (presumably typical) examples of area-preserving maps have been computed to within 10%.

More generally, the state of understanding of the breakdown of stability for area-preserving twist maps used to be that KAM curves are destroyed by nonlinear resonances that occur in orbits of unfavorable "frequency." In the last few years it has been discovered that stable behavior persists even for the resonant frequencies. Although the curves of KAM theory are destroyed by resonances, there remain stable Cantor sets of fractal dimension less than 1. This is an important change in our view of how stability can arise in complicated nonlinear systems. Much remains to be done in this field. Indeed, complete understanding of area-preserving maps of the plane is a remote goal.

12. Noncommutative Geometry

A major mathematical development of the 1960s was to establish an intimate link between the field of analysis and the fields of topology and geometry. This unification of seemingly diverse areas of mathematics set the stage for numerous interrelations and the tone of mathematics today. The development of modern index theory provided this path. As a first step, mathematicians realized that geometric invariants of manifolds could be computed as analytic invariants of certain Laplace operators and Dirac operators on these manifolds. An abstract version of these ideas has become known as K-theory.

In the past five years we have seen a rejuvenation of K-theory, leading to the discovery of cyclic homology, cyclic cohomology, entire cyclic cohomology, and graded (i.e., super) KMS-functionals. These different topics all have been points of view within the new field of non-commutative geometry. Basically, the ideas of differential geometry have been shown to extend to a noncommutative setting. In particular the calculus of differential forms and the homology of currents can be extended to deal with spaces such as the leaves of a foliation, the dual space of a finitely generated non-Abelian discrete group (or Lie group), or the orbit space of the action of such a group on a manifold.

Such spaces are badly behaved as point sets and do not lend themselves to the usual tools of measure theory, topology, or differential geometry. They are better understood by means of associating a canonical algebra to each space. In the case of an ordinary manifold, this algebra is a commutative algebra of functions on the manifold, such as the algebra of essentially bounded measurable functions on the manifold (for measure theory), the algebra of continuous functions vanishing at infinity (for topology), or the algebra of smooth functions with compact support (for geometry). In the realm of noncommutative geometry, these algebras are replaced by noncommutative algebras. In special cases these algebras are von Neumann algebras or

C*-algebras; they lead to the generalization of de Rham cohomology and to its applications, K-theory and index theory.

The basic framework to study such problems is a Z_2-graded algebra, a graded derivation of the algebra, and a trace that satisfies some basic cyclicity axioms. A basic result in this area is the association of a cyclic cocycle to this structure, and the construction of a Chern character for the derivation. In the commutative case, the construction reduces to the ordinary de Rham cohomology theory and its K-theory. In the noncommutative case, the framework is more general.

13. Computer Visualization As a Mathematical Tool

In recent years computer graphics have played an increasingly important role in both core and applied mathematics, and the opportunities for further utilization are enormous. One core area where visualization has been of key significance is in the theory of surfaces. Complex problems that appeared to be intractable have been either completely or partially solved by insight gained from computer graphics.

One such example in surface theory, drawn from the study of soap films, has a long history. A loop of wire immersed in a soapy solution and then withdrawn will support a film of the soap solution characterized by its having the least area among all surfaces that have the given wire loop as boundary. Finding this minimal surface is easily expressed as a problem in the calculus of variations and thus reduced to the study of a certain partial differential equation, the minimal surface equation. While the solutions of this equation are not difficult to describe, at least in the small, the global behavior of the solutions is very delicate, and many questions remain open.

These problems actually have physical significance as well. For example, any physical soap film will not cross itself (i.e., it is embedded), but this property is difficult to determine from the standard representation of the solutions to the minimal surface equation. In fact, up until five years ago, there were only two known embedded minimal surfaces that were complete in the sense that they had no edges. These were the ordinary plane and a surface of revolution called the catenoid. In fact, it had been conjectured that these were the only complete embedded minimal surfaces in three-space.

In 1983 a new example was found of a minimal surface that had the topology of a torus punctured at three points. This surface seemed, by evidence based on the theory of elliptic functions, to be a good candidate for a counterexample to the above conjecture. However, the complexity of the defining equations made a direct attack on the embedding problem difficult. When a computer was used to make sketches of the surface, the surface was seen to have extra symmetries that had been overlooked in the purely analytic description. These symmetries not only made the surface easier to visualize, but also suggested a possible line of reasoning that eventually led to a proof that the surface was indeed embedded, thus disproving the conjecture. Moreover, features of this surface suggested a generalization that allowed mathematicians to construct an infinite family of embedded complete minimal surfaces. These new examples have invigorated the subject of minimal surfaces in general, and recent progress in the subject has been closely linked to computer graphics.

More general calculus-of-variations problems have recently been approached by computer graphics techniques, which are invaluable in formulating and testing conjectures. It is clear that our understanding of global and stability problems in the calculus of variations is being tremendously enhanced by computer graphics. As an example, in 1988 the first computer simulations and visualizations of soap bubble clusters and other optimal energy configurations in three dimensions were computed and displayed. In particular, this allowed close study and experimentation with the geometry of the interfaces. Programs were also developed that in principle allow the interactive construction of minimal area surfaces: draw a knot in space, specify the topological type of surface of interest, and the program will compute and display a beautiful minimal surface in that class. Along similar lines, it has been possible for the first time to compute and visualize some striking crystalline minimal surfaces.

In an entirely different direction, a theory called "automatic groups" has been developed. This is the theory of that class of infinite groups that can be analyzed by finite-state automata, for example, word problems that can be solved by computer; the theory involves issues similar to those used in constructing word-processing programs. Typical automatic groups include the groups of geometry. A computer program has already been used in explorations of the limit set of certain quasi-fuchsian groups. More generally, the theory is required in order to make a catalog of hyperbolic three-manifolds by computer, an effort that is already well under way.

14. Lie Algebras and Phase Transitions

The past five or six years have seen a fascinating interplay between various branches of pure mathematics and the physical theory of phase transitions in a two-dimensional world. It should be noted that physics in two dimensions is not just a theoretical curiosity: surface phenomena and thin films are much-studied experimental realizations of the theories discussed here. The modern era started in 1944 when Lars Onsager solved the Ising model of ferromagnetism for two-dimensional lattices. The Ising model gives the simplest possible picture of a magnet: "spins" that can point only "up" or "down" sit on the sites of a space lattice and are coupled by pairwise short-range interactions favoring parallel alignment. Onsager's solution showed, for the first time, that a phase transition is accompanied by non-analytic behavior of various physical quantities; for example, the spontaneous magnetization vanishes at a rate proportional to $(T-T_c)^\beta$ as the temperature T approaches its critical value T_c, where β is a characteristic exponent. Subsequently, other exactly soluble statistical mechanical systems were found, leading to a large class of completely integrable two-dimensional models.

A remarkable feature found in all these models (and also in heuristic studies of polymer systems, percolation problems, and other two-dimensional systems) was that the characteristic exponents describing the critical nonanalyticities were always equal to rational numbers. A deep result of the mathematical developments during the 1980s is that these rational numbers are now understood to label representations of a symmetry algebra of the system, in much the same way that the mysteries of atomic spectra in the beginning of the century were understood in terms of the representation theory of the three-dimensional rotation group.

One line of the development started with the introduction of a natural set of infinite dimensional Lie algebras (Kac-Moody algebras), central extensions of loop algebras of the classical Lie algebras. At the same time another infinite dimensional Lie algebra, the Virasoro algebra, entered physics in the dual resonance models and string theory. While the dual models lost much of their interest for physicists in the 1970s, there were important mathematical developments that grew from them: for instance, the development of a formula for the determinant of the contragradient form of a highest-weight module of the Virasoro algebra, formulas for the characters of integrable representations of the Kac-Moody algebras, and the explicit construction of these representations in terms of vertex operators.

The statistical mechanical developments started in 1984 with the realization that the conformal invariance expected for a physical system at a critical point is, in two dimensions, realized as a symmetry under two Virasoro algebras. The particular central extension (parametrized by a positive "charge" c) characterizes the physical system. The critical exponents turn out to be the highest weights of the representations of the algebra. It was shown that there are very special representations, so-called degenerate ones for $c < 1$, having special rational weights and charges, and it was argued that some of these correspond to known physical models, the Ising model in particular. Subsequently, it was shown that with the additional physical assumption of unitarity, all the $c < 1$ critical statistical systems could be classified and all their exponents computed. Translating this analysis to physical language resulted in explicit computations of the asymptotic correlation functions for the $c < 1$ theories, thus effectively showing that they are all completely soluble.

Progress in the $c > 1$ theories has since been made using the theory of Kac-Moody algebras. It was shown that these algebras occur as symmetry algebras of a two-dimensional field theory. The conformal symmetry now turns out to be closely connected to the algebra of the Kac-Moody symmetry: the Virasoro algebra is embedded in the enveloping algebra of the Kac-Moody algebra by an algebraic construction. This provides many new concrete Virasoro representations, and more importantly the so-called coset construction. It was shown that a given representation of a Kac-Moody algebra leads to a host of Virasoro representations, corresponding to subalgebras of the Lie algebra. Thus an even more general infinite family of critical statistical systems was identified on the basis of symmetry. The possibility of classifying so many, if not all, statistical mechanical systems exhibiting critical behavior, albeit in two dimensions, would have been considered purely utopian only ten years ago.

15. String Theory

Some of the most exciting developments in recent mathematics have their origin in the physicists' string theory, the so-called "theory of everything." This development offers a classic example where a physical science required a great deal of sophisticated mathematics, much of which had, surprisingly, already been worked out by mathematicians pursuing other "pure" mathematical motivations. The physical theory returned the favor with a cornucopia of new intuitions and insights. The turnaround time for such cross-fertilization may have set speed records in this instance!

To the nonspecialist the most striking feature about string theory is that it replaces the idea that the smallest idealized physical particle might be thought of as a concentrated "point particle" with the idea of exceedingly small but extended strings. A point particle moving through space with time traces out a trajectory, called the "world line" of the particle, which summarizes its physical history. A string moving through space with time traces out a surface, called the "world sheet." The underlying mathematics of surfaces is much more sophisticated than that of curves, so the basics of string theory are much more complex than previous physical theories.

The entry of new mathematics into string theory is forced by principles of invariance—one must eliminate superfluous parameters from the description of the theory. Three such principles emerge: *parameter invariance* on the string (the labeling of positions on the string is physically irrelevant); *conformal invariance* on the world sheet (only angles and not lengths are important prior to the appearance of mass); and *gauge invariance* on the string (physical quantities will be independent of the measuring frames of reference). This last principle has already had a profound effect in physics and mathematics, being the basis of all current descriptions of electromagnetism and elementary particles. Parameter invariance calls upon the theory of an infinite-dimensional symmetry group of the circle, the diffeomorphism group. Gauge invariance calls upon the theory of the infinite-dimensional Kac-Moody algebras and groups. These rose to prominence in mathematics both for mathematical reasons and because of their use in earlier physics as "current algebras." Finally, conformal invariance calls upon a vast wealth of algebraic geometry and moduli theory for Riemann surfaces (Teichmuller theory). This is most surprising, since previously algebraic geometry had seemed largely remote from the physical world.

When matter appears and gravitational effects must be described, current string theory calls for the replacement of points in space-time by very small closed six-dimensional surfaces! In order to reproduce the physics we already know at larger length scales, the geometry of these surfaces will have to obey an analogue of Einstein's equations from general relativity. These had already been studied by mathematicians. Their work was motivated by algebraic geometry and partial differential equations, the latter being classically the main vehicle for exchange between mathematics and physics.

Many mathematical problems for future study have been posed by string theory. The most significant appear related to the study of surfaces considered up to conformal equivalence (as in conformal invariance above), and the topology and geometry of other low-dimensional figures (three-and four-dimensional surfaces). Indeed, Witten has developed a physical dictionary of the entirety of low-dimensional geometry. This dictionary suggests, for physical reasons, a long list of deep questions and constructions. For example, in the study of knots in three-space, this physical picture contains whole new outlooks on even the most subtle recent studies of knots (including the Jones polynomial; see Section 10). On the other hand, string physics is giving more shape and direction to our study of infinite-dimensional geometry. This will be an open task for years to come.

It is eerie and uncanny, both to physicists and mathematicians, that what was considered central and important for pure or aesthetic reasons by mathematicians has proved ultimately to be the same mathematics required by physical theory. It should be emphasized that this mathematics derives from a period considered the most abstract in the history of the field.

16. Interacting Particle Systems

This area of probability deals with configurations of particles that evolve with time in a random manner. Typically, a particle moves, dies, or gives birth according to its specified law, which depends only on the state of the system in some neighborhood of the particle.

Interacting particle systems have their roots in the study of the Ising model described above but now pertain to a wide variety of applications from the study of biological systems to image processing for medical and defense purposes. The contact process, a basic model for the spread of a biological population, was introduced in 1974. In contrast to branching process models, this system allows there to be only a bounded number of individuals per unit area. This physically reasonable constraint makes the system very difficult to study analytically. The basic properties of the one-dimensional case (linear growth of the system when it survives and exponential decay of the survival probability when it dies out) were settled early in this decade, but only very recently have the corresponding facts been proved for the important two-dimensional case.

Variations of the contact process with several types of particles are now being applied to the study of competition of species and host-parasite or predator-prey system. Other models more closely related to percolation are being applied to the study of the recent forest fires in Yellowstone. A third example is the study of the distribution and dynamics of antarctic krill. This last system is particularly interesting since it displays patterns on multiple spatial and temporal scales. The preceding are just three of a growing list of examples that show interacting particle systems are an appropriate framework for understanding the mechanism at work in various ecological phenomena.

17. Spatial Statistics

The keen interest in the development of theory and methods for spatial statistics is strongly driven by an array of applications including remote sensing, resources estimation, agriculture and forestry, oceanography, ecology, and environmental monitoring. The common thread is the characterization and exploitation of proximity. Some of the outstanding opportunities for future progress are outlined here.

In geophysical remote sensing, data arrive usually in gridded form, commonly corrupted by unwanted atmospheric effects and positional and measurement errors, often using multiple wavelength bands making the data multivariate, and almost always in large quantities. Typical questions are, How does one suppress errors, how does one combine the information from different wavelengths, how does one extract patterns, how well is one doing, how far can the data be pushed, and what would be the value of additional data? Statistical approaches to answering all such questions will be profoundly affected by proximity considerations.

Procedures for suppression of unwanted effects must often make do with weak specifications of those effects. Procedures for extracting underlying patterns from the combination of multiband information should be strongly guided by probabilistic models with sufficient rich ness. On the other hand, estimates of statistical precision should be as model-free as possible. These requirements present important challenges to research statisticians, with technical and computational difficulties considerably surpassing those associated with related problems in time series analysis or one-dimensional stochastic processes.

Spatial data for resources estimation and environmental monitoring are typically not obtained on regular grids and are comparatively sparse. With regard to resources estimation, while data obtained from core drilling may have good vertical resolution, the cores may be preferentially located in high-grade zones. What one usually wants is an estimate of total reserves, the frequency distribution of resource blocks, and a rational exploitation plan based on localized resource estimates, together with measures of uncertainty to guide the need for further exploration efforts. The original and still widely used statistical methodology, based fundamentally on Gaussian process modeling, is not particularly well adapted to the highly erratic nature of resources data, and considerably more research is needed to deal honestly with the particular qualities of resources data.

In environmental monitoring, highly impacted areas may be preferentially sampled. The unevenness and selectivity of environmental data present important challenges for statistical modeling and inference. Furthermore, at each monitoring location there will typically be available a time series of data with seasonal variation. What one usually wants to know is how environmental quality is changing. Since data records are usually short in relation to the amount of change that can be directly observed amidst large natural variability, a sound methodology is needed to combine information from multiple monitoring locations. Also the augmentation and rearrangement of monitoring resources require research in statistical design problems that goes well beyond the simple idealized design problems for which we have some answers.

During the last decade substantial strides have been made in the development of appropriate theory and methods for solving spatial problems using statistical tools. Thus the needed research described above has a substantial foundation on which to build. An important recent advance in spatial statistical research is the development and application of flexible spatial Markov lattice models for image processing and the application of powerful techniques such as the Metropolis algorithm for implementation of these models. Other devel opments include spatial smoothing techniques that adjust to the local complexity of spatial patterns, dimensionality-reduction methodology for multivariable spatial data based on spatial structure criteria, development of mathematically tractable non-Gaussian spatial field models for continuous parameter fields, non-linear non-parametric spatial interpolation methods for discontinuous spatial fields, demonstration of the theoretical links between spline methods, kriging methods, and Wiener filters, and cross-validation methodology for calibrating spatial estimation techniques and assessing their precision.

18. Statistical Methods for Quality and Productivity

During the decades since World War II the industries that have raised their productivity and the quality of their products have survived and prospered. Those that have not have done poorly or gone out of business. Statistical methods to analyze production processes are indispensable tools for engineers to increase quality and productivity.

There are four areas of statistical methods that are particularly heavily used: (1) statistical process control, (2) statistical experimental design, (3) reliability, and (4) acceptance sampling. Statistical process control consists of methods for assessing the behavior of an engineering process through time, in particular for spotting changes; the major methods in this category are control charts, a topic in need of new thinking. Most of the methods now in place go back decades and were invented in an era when computation was done by hand and when data were often assumed to be normally distributed since not doing so led to intractable computation. For example, variability in control-chart methods is often measured by the range of the data, an easy number to compute. Control-chart methods need to be rethought from the ground up.

Experimental design is a crucial technology for isolating factors that can be changed to improve processes; thus, to achieve continuous improvement of an engineering process, design experiments probing the process must be continuously run. Most research in this area has focused on understanding how the mean level of a response depends on the factors; models are used in which the variance is either constant or, if it varies, is viewed as a nuisance parameter. But for many engineering processes, variance changes and is as crucial an object as the change in mean level; this is the case, for example, in robust design. Robust design recognizes that the quality of most products depends on a large number of factors. Some factors, referred to as "control parameters," are inexpensive to control, while others, referred to as "noise parameters," are costly or even impossible to control. The goal is to design a product whose performance is insensitive, or robust, to the noise parameters.

Methods of reliability are used to study the lifetimes of components and systems and to relate these lifetimes to factors that determine performance. In this area, the research community needs to develop methods with which engineers can shift from analysis of time-to-failure measures to the analysis of measures of degradation, which are more informative. Also in this area, more work is needed on models for data from accelerated failure-time experiments.

Acceptance sampling consists of methods for inspecting a lot of a product to determine if it is acceptable; in some cases every item in the lot is tested, but more typically a sample of items is selected for testing, and inferences made about the entire lot. New attacks on methods are needed; some past work has suggested that Bayesian approaches are a fruitful avenue to follow.

19. Graph Minors

A minor of a graph is any graph obtained from a subgraph of the original graph by contracting edges. A number of interrelated results on graph minors, some in "pure" graph theory and some relevant to the design of algorithms, are among recent achievements. The results open up new avenues for research and suggest a number of problems and opportunities.

An old and completely solved question in network flow theory supposes that one is given a graph in which some vertices are called "sources" and some are called "destinations." It is to be decided whether there are, say, ten paths in the graph, running from the sources to the destinations and not meeting one another. How can one program a computer to decide this? One way is to list all the paths, and try all combinations of ten of them, but that takes far too long, even for quite a small graph. Fortunately (because this is a very important problem, with a huge number of applications) there is another algorithm more indirect but very efficient. Thus, this problem can be viewed as completely solved.

If the question is changed slightly, and it is posited instead that there are ten sources and ten destinations and one requires the first path to run from the first source to the first destination, the second path to run from the second source to the second destination, what then? This new problem is much more difficult. Even the two-paths problem (with two sources and two destinations instead of ten) is difficult, and until recently the three-paths problem was unsolved. One of the main results about graph minors is that for any number (say ten), there is an efficient algorithm to solve the ten-paths problem. ("Efficient" here is used in a technical sense, meaning "with running time bounded by a polynomial in the number of vertices of the graph.")

A second result was a proof of an old conjecture of Wagner, that in any infinite collection of graphs there will be one containing another. (A graph "contains" another if the second can be obtained from a subgraph of the first by contracting edges.) This is of interest in "pure" graph theory, but it also has algorithmic applications if it is used in combination with the algorithm described earlier. For instance, suppose that one would like to know if a certain graph can physically be constructed, using wires for edges, in such a way that no circuit of the graph is "knotted." No efficient algorithm is known to decide this. But it follows that an efficient algorithm for the problem exists, even though no one has found it yet.

One can show a similar result in great generality that has a considerable number of applications in theoretical computer science. Suppose that it is desired to design an efficient algorithm to test if a graph has a certain property. For some properties it is impossible to find such an algorithm; but suppose that no graph with the property contains any graph without the property. (For instance, the property of being knotlessly constructible satisfies this condition.) Then there is an efficient algorithm to test if a graph has the property, although it may not have been found yet. It should be emphasized that knowing that an efficient algorithm exists, even though one has not yet been found, is an important and significant piece of information.

20. Mathematical Economics

Although mathematical discussion of the operation of markets began in the last century, the first rigorous mathematical description of the fundamental economics in the operation of markets came in the late 1940s and early 1950s. This start culminated in the famous model of general equilibrium (GE) under the hypothesis of complete markets, that all commodities can be bought and sold in markets that meet at the same time, and with perfect credit. In equilibrium, supply equals demand, and each household acts in its own interest, given its budget constraint.

However, the complete-markets model just described suffers from a major drawback. Planning for future contingencies is an essential part of the economic allocation problem, and the GE model can be interpreted as incorporating time and uncertainty by indexing the commodities by event and date. The single budget constraint, however, forces on this interpretation the unrealistic view that all trades are negotiated at once. Recent work on incomplete market models that remedy this difficulty has achieved significant progress and has opened up a number of new questions and opportunities.

In the general equilibrium model with incomplete markets (GEI), agents cannot trade all possible commodities at one time. For simplicity, suppose that there are perishable commodities that can be consumed at time zero, or under any of the S states of nature at time one. Moreover, agents may be wealthy in some states, and poor in others. At time zero, agents are also allowed to trade a limited number of assets that promise delivery in various combinations of the goods, depending on the state of nature. The stock of a firm, for example, is an asset that delivers a large quantity of goods in those states when the production plans work well, and many fewer goods otherwise.

Several very surprising properties can be shown to hold for GEI equilibrium, using tools from differential topology. First, in great contrast to the complete markets model, if there are fewer assets than states, then the GEI equilibria are "generically" inefficient. Indeed the equilibria are inefficient not only because there are missing asset markets but also because the markets that do exist are not used properly.

A more surprising attribute of the GEI model is the special properties of monetary equilibria that it permits. If there is a commodity that has no effect on utility, and is not part of the initial endowment of any agent, such a good has two of the properties of money. If the assets promise delivery in this money, then under the conditions of the inefficiency theorem there are generically S - 1 dimensions of distinct equilibrium commodity allocations. Yet, if the asset market were 'complete," then there would be typically only isolated equilibria.

Removing just one asset creates a jump in indeterminacy from zero dimensions to S - 1 dimensions; this dimension is constant no matter how few assets there are. This result could not reasonably have been predicted, and certainly not convincingly demonstrated, without the aid of tools from differential topology. Note that without a money good, there are typically a finite number of equilibria. In the GEI model, money has a prominent role to play.

There is another application of these mathematical ideas to the GEI model. It was thought for a long time that there might not be a GEI equilibrium in any robust sense. The trick to the correct analysis of the problem was to recognize that the GEI budget constraint can be reexpressed in terms of the span of the monetary payoffs across the S states, and hence in terms of a Grassman manifold constructed from the state space. Arguments from degree theory show that the simultaneous equations defined by GEI equilibrium on this Grassman manifold generically have a solution.

21. Parallel Algorithms and Architectures

Dramatic advances in computer fabrication technology have had an equally dramatic effect on the way that we use computers. For example, the most powerful computers today actually consist of many smaller component processors (i.e., chips) that are integrated together to form a single parallel machine. In a parallel machine, the processors are usually traditional sequential machines that are working together to solve a single large problem. By collecting *N* processors together to work on a single task, one hopes to perform the task *N* times faster than with only one processor. Although it seems intuitive that *N* processors should be able to solve a problem *N* times as fast as a single processor, this is not always possible. In fact, it can be very hard to design parallel algorithms for *N*-processor machines that run *N* times faster than on a uniprocessor machine.

As a very elementary example of what can go wrong when one tries to parallelize a sequential algorithm, consider the "trivial" task of adding two *N*-digit numbers. Addition is certainly easy to perform in *N* steps sequentially, but can we do it in one step with *N* parallel processors? In fact, we cannot. Even worse, at first glance it would seem that we cannot solve the addition problem any faster with *N* processors than we can with one processor because before one can compute any digit in the sum, one needs to know if there is a carry from the next digit to the right and so on. Therefore, the addition example seems to be very discouraging at first, for if one cannot efficiently parallelize addition, then what hope can there be for efficiently parallelizing other problems?

Fortunately, it is now possible to derive fast parallel algorithms for many important scientific and engineering computing problems. For example, in the simple case of addition, there is an algorithm that takes just $\log(N)$ steps using $N/\log(N)$ processors, although, as we have just seen, it is not at all obvious that such an algorithm exists. In fact, dramatic progress has been made in the last five years in uncovering nontrivial and highly efficient methods for parallelizing important problems that seem to be inherently sequential. Examples include algorithms for arithmetic, matrix calculations, polynomial manipulation, differential equations, graph connectivity, pointer jumping, tree contraction and evaluation, graph matching and independent set problems, linear programming, computational geometry, string matching, and dynamic programming.

Much progress has also been made on the problems inherent in designing parallel machines. For example, a variety of clever communication networks have been invented for linking the processors of a parallel machine together, and fast algorithms have been developed for routing the right data to the right place at the right time. This work has been highly mathematical in nature, drawing extensively on techniques from combinatorics, probabilistic analysis, and algebra. Indeed, parallel computers commonly use combinatorial-based interconnection networks and routing algorithms. Again, many opportunities for further advances flow from these already substantial achievements.

22. Randomized Algorithms

Over the past 15 years computer scientists have come to recognize the many advantages of algorithms that toss coins in the course of their execution. For a wide variety of tasks, ranging from testing whether a number is prime to allocating resources in distributed computer systems, the simplest and most efficient algorithms currently known are randomized ones. Therefore, expanding our understanding of such algorithms is a challenge and opportunity for the future.

Almost from the beginning of the computer era, random number generators have been applied to the simulation of complex systems involving queueing and other stochastic phenomena and to the estimation of multidimensional integrals and other mathematical quantities, using various sophisticated sampling techniques known collectively as the Monte Carlo method.

A major factor in drawing the attention of computer scientists to the wider uses of randomization was the discovery, around 1975, of two efficient randomized algorithms for checking whether a number is prime. Each of these algorithms is based on the concept of a witness to the compositeness of a number. A simple illustration of this concept is based on a theorem due to Fermat, which says that if n is a prime number, then, for any integer m that is not a multiple of n, $m^{(n-1)} -1$ is a multiple of n. If this calculation is performed for some m, and one does not get the result predicted by Fermat's theorem, then n is composite (i.e., not prime); in this case, m is called a witness to the compositeness of n. The tests mentioned are based on slightly more complicated kinds of witnesses. The effectiveness of these tests stems from theorems that show that, if n is composite, then most of the integers between 1 and n-1 will serve as witnesses. An interesting aspect of these tests is that they do not provide witnesses for primality, but this weakness was rectified in work that defined witnesses for primality rather than compositeness, showing that if n is prime, most randomly chosen numbers will bear witness to that fact. There are many other randomized algorithms based on the abundance of witnesses.

Randomized techniques have also proved to be a very effective tool for algorithm construction in the areas of sorting, searching, and computational geometry. A simple illustration is the problem of listing all intersections among a set of line segments in the plane. There is a fairly obvious incremental algorithm that considers the segments one at a time and reports the intersections of each new segment with all the previous ones. If the segments are read in a particularly unfortunate order then the run time of this algorithm will be excessively long; however, it can be shown that if the segments are processed in a random order, then with extremely high probability the algorithm will be very fast.

In addition, randomization plays a crucially important role in the design of distributed computing systems, in which many geographi cally dispersed computers connected by suitable communication links work together as a single system. Such systems must cope with the possibility that individual computers or communication links may fail or may run synchronously at different speeds, and must ensure that the overhead of communication between processors will not become an insurmountable obstacle. Randomization is particularly effective in allocating computational tasks to the individual processors and in choosing the communication paths along which data shall flow. It can be shown in a variety of settings that random allocation of tasks to processors and data to memory modules, together with randomized routing of messages, yields near-optimal performance with high probability.

All the applications of randomization that we have mentioned depend on the assumption that algorithms, or computer programs, have access to a stream of independent random bits. More commonly, computers use pseudorandom numbers that are generated from an initial number, called the seed, by some purely deterministic iterative process. These generators are typically subjected to certain statistical tests in order to confirm that the streams of numbers they generate have some of the properties of random sequences, even though they are generated by a purely deterministic process. Currently, a deep line of research into the properties of pseudorandom number generators is being pursued. The goal of this research is to show that, as long as the seed is random, the output of the generator cannot be distinguished from a purely random sequence by any polynomial-time computational test whatsoever.

Finally, recent theoretical research has focused on a connection between pseudorandom generators and the concept of a one-way function, which is fundamental in cryptography. A one-way function is a function that is easy to compute but hard to invert. It has been shown that any one-way function can be used to construct a rigorously justified pseudorandom number generator. Unfortunately, researchers in computational complexity theory have not yet determined whether one-way functions even exist. This is one of the many important problems remaining to be addressed.

23. The Fast Multipole Algorithm

There are great opportunities for progress in algorithms dealing with problems such as particle beams in plasma physics, underwater acous tics, molecular modeling, and even very important aspects of aerodynamics. A basic calculation of central importance in these applications is the calculating of interactions in a many-particle system. These interactions are often long-range, so all pairs of particles must be considered. Because of the latter constraint, the direct calculation of all interactions requires on the order of N^2 operations in a system with N particles. We will refer to this calculation as the N-body potential problem.

There have been a number of efforts aimed at reducing the computational complexity of the N-body potential problem. The oldest approach is that of particle-in-cell (PIC) methods, requiring on the order of N log (N) operations. Unfortunately, they also require a mesh that provides limited resolution and is inefficient when the particle distribution is nonuniform. A more recent approach is that of the hierarchical solvers, which are gridless methods for many-body simulations, having computational complexities also estimated to be of order N log(N).

The Fast Multipole Method (FMM), which has recently been developed, requires an amount of work only proportional to N to evaluate all pairwise interactions to within roundoff error, irrespective of the particle distribution. Like the hierarchical solvers, the FMM is a divide-and-conquer algorithm, based on the idea of clustering particles on a variety of spatial length scales. The method is in fact based on a combination of physics (multipole expansions), mathematics (approximation theory), and computer science, and its use in applications is growing.

There are several immediate industrial applications for the techniques being developed. The payoff should be substantial and almost immediate in the straightforward use of particle simulations. Simulations of this type are performed in the modeling of high-powered electron beam microwave devices (e.g., gyrotrons and free-electron lasers), particle beams, controlled fusion devices, and so forth.

A second immediate industrial application is in molecular dynamics, a technique for studying the properties of fluids (and other phases of matter) by computer simulation. Once initial positions and velocities are chosen for some number of representative particles, their trajectories are followed by integrating Newton's second law of motion. In early simulations, only nonpolar fluids were considered, where the amount of computation per time step is proportional to the number of particles *N*. In polar fluids, the situation is quite different. A coulombic term is added to the potential function and all pairwise interactions need to be accounted for, a standard *N*-body calculation. The FMM allows for the simulation of much larger chemical systems than was previously possible. The study of a protein molecule in water, for example, requires following the motion of tens of thousands of atoms over tens of thousands of time steps. Real gains are possible in the long term, beginning with detailed theoretical studies of chemical kinetics.

24. Interior Point Methods for Linear Programming

Many problems in resource allocation can be modeled by what is called the "linear programming" problem, in which one attempts to optimize a linear function over the vertices of a multidimensional polytope. The traditional Simplex algorithm for this problem, which works by traveling along the boundary of the polytope, has had immense value and influence during the 40 years since it was discovered. It has a significant theoretical drawback, however: its running time can, in pathological cases, grow as an exponential function of the number of variables. Much more desirable, at least from a theoretical point of view, would be an algorithm with polynomially bounded worst-case running time.

In 1976, the first such algorithm, the Ellipsoid method, was discovered. Its running time was $O(n^4 L^2)$, where n is the number of variables and L is a measure of the number of bits needed to describe the input. This algorithm has the additional desirable property that it applies to more general "convex programming." Moreover, it does not require a complete description of the convex body over which optimization is to take place, but merely a "black box" subroutine that, given a point, tells whether that point is in the polytope, and if it is not, will identify a hyperplane that separates the point from the polytope. The Ellipsoid method is thus applicable to a much broader class of problems than is the Simplex method, and its existence has led to a wide variety of polynomial-time algorithms for previously open problems. For linear programming, however, researchers quickly discovered that its improved worst-case running-time bound did not correlate with better performance in practice.

Polynomial-time programming algorithms thus seemed an impractical theoretical nicety. In 1984 this was changed with the introduction of a new breed of polynomial-time linear programming algorithm, based on a clever variant of an old idea. The idea, one that had been abandoned long ago as impractical, is to cut across the interior of the polytope in searching for an optimum vertex, rather than traversing the outside as does Simplex. The difficulty in making such an "interior point" approach work lies in finding the right direction and distance to travel at each step. The solution involves the use of projective transformations and a logarithmic potential function to guide the search, and yields a running time of $O(n^{3.5} L^2)$. The theoretical improvement over the Ellipsoid method running time was not the main story, however; more important, this algorithm (along with several of its variants) appears to be competitive with Simplex when implemented with appropriate sparse matrix techniques. Moreover, it appears to have substantial running-time advantages for large and/or degenerate instances. Indeed, important practical applications have been found in the design of large telecommunication networks and in the solution of large-scale logistics planning and scheduling problems.

Since the first reports of the potential practicality of the approach, there has been a torrent of research on interior point methods. Relations between this algorithm and earlier algorithms have been extensively explored. For instance, the algorithm can be viewed as a type of "logarithmic barrier function" algorithm, or even as an application of Newton's method (in an appropriately transformed space). In the limit of infinitesimal step length, it generates a vector field inside the polytope, all of whose limiting trajectories go to the optimal vertex. In this context, it can be viewed as attempting to follow such a trajectory approximately, by taking short steps along tangent lines. This in turn suggests variants in which one steps along curves that represent higher-order power series approximations to the vector field. Other variants concentrate on approximately following a particular trajectory, the so-called "central trajectory." These latter have led to better and better worst-case running times, with the current champion having a running time of $O(n^{2.5} L^2)$, based on a clever use of recent developments in the field of fast matrix multiplication.

New algorithms and insights continue to pour forth at a rapid rate, and although it seems unlikely that this will lead to polynomial-time solutions for the much harder NP-complete problems, there is much hope that the interior point approach will greatly extend the range of problems for which useful answers can be determined.

25. Stochastic Linear Programming

Deterministic models for linear programming problems and their solutions, ever since their first appearance 40 years ago, have been keeping pace with the extraordinary advances that have taken place in computing power. At the present time, industry and government routinely solve models in which there is no uncertainty; that is, they solve deterministic linear and mathematical programs for planning and scheduling purposes, some involving many thousands of variables with a linear or nonlinear objective function and many thousands of inequality constraints. These assume, for example, that knowledge about what future technologies will be available to choose from is known with certainty. As a result, the solutions obtained from deterministic models are incomplete because they do not properly take account of future contingencies. Although it is easy to reformulate the mathematical models to include uncertainty, the resulting size of the mathematical systems to be solved becomes too enormous in most practical applications. The bottleneck to solving stochastic programs has been (and is) calculating capability.

Therefore, despite the progress made, there remains an unresolved class of decision problems of great importance: that of finding an "optimal" solution to stochastic mathematical and linear programs. Stochastic here means uncertainty about, for example, the availability of resources, foreign competition, or the effects of possible political upheavals. Since such problems concern the optimal allocation of scarce resources under uncertainty, it is of fundamental importance to include uncertainty in the problem setup. If such problems could be solved in general, it would significantly advance our ability to plan and schedule complex situations.

At the present time there is intense activity taking place in the United States and elsewhere to solve certain relevant classes of stochastic linear and nonlinear programs. Important new developments in computer hardware are spurring this effort, particularly the availability of multiple vector processor mainframes and parallel processors. It is hoped that the combination of three techniques—improved ways to mathematically decompose large-scale problems into smaller ones, improved techniques of Monte Carlo (importance) sampling, and the use of parallel processors—will bring about important advances in the relatively near future.

26. Applications of Statistics to DNA Structure

A strand of DNA can be represented as a string of bases, A,C,G,T, that carries the information governing the growth and function of an organism. Great effort has therefore been expended in determining DNA sequences. Rapid sequencing methods were introduced in 1976 and were followed by an explosion in quantitative genetic information. Today over 25 million letters of sequence have been determined, in segments averaging length 1000, from a wide variety of organisms. Improvements in sequencing technology continue to be made, and the associated discoveries in basic biology are staggering.

Two kinds of maps are constructed for DNA: genetic maps and physical maps. Both types are generated from the use of restriction enzymes that cut DNA at specific patterns in the sequence, producing fragments whose lengths can be measured with some degree of inherent experimental error. It was suggested in 1980 that slight variations in DNA sequence could produce differing restriction fragment lengths that could be used as "markers"—traits—that could be approximately mapped to specific locations on specific chromosomes. The amount of data subsequently available has created a number of new statistical problems.

Physical maps give the relative locations of identifiable and clonable pieces of DNA. Availability of a physical map facilitates the complete sequencing of a DNA strand. Given the mapped locations of a complete library of clones—each having a length on the order of several tens of thousands of nucleotides—a number of laboratories in coordination could then proceed simultaneously to sequence the individual clones. We can expect statistical analysis of design options, such as number and choice of cutting enzymes, to yield benefits in the mapping process. The process of physically locating clones along the genome should be substantially facilitated by an understanding of the design parameters and sources of variation inherent in the process.

Obtaining DNA sequence data is only a first step in modern molecular biology. The sequence is next subjected to extensive analysis, to relate it to what is already understood about DNA sequences. Because evolution operates to preserve biological features of importance, including sequence features, these analyses can be very important in understanding the function of specific portions of the sequence. Use of computers to implement complex algorithms is often required; mathematical analysis of algorithms is essential, both to ensure an unambiguous, informative interpretation of the results and to ensure that a programmed algorithm will complete its operations rapidly enough.

The study of molecular evolution has developed with the ability to read DNA and protein sequences. It is just now becoming possible to sample the sequence for a gene within a defined population. This opens up many new questions. How does molecular evolution proceed, in the long term and in the short term? Constructing evolutionary trees and determining rates of evolution can both be accomplished with stochastic process models of molecular evolution. Some of the most central work goes into identifying protein coding regions or genes in DNA. Locating a gene of 600 letters that is spread out in small segments along 10,000 or 20,000 letters of DNA is a daunting but essential task, requiring sophisticated combinatorial and statistical analysis.

The science of molecular biology is currently undergoing rapid treatment. The anticipated quantity of DNA and protein sequence data makes it an area ripe for mathematical development. The nature of the science makes it necessary that mathematical and biological scientists closely communicate. Both sciences will surely benefit from such collaborative effort.

27. Biostatistics and Epidemiology

Epidemiology concerns the distribution and determinants of disease in human populations. It encompasses such varied subjects as the worldwide geographic variation in disease incidence rates, the setting of radiation exposure standards in the workplace, and the evaluation of vaccine efficacy using randomized field trials.

Two distinct study designs, cohort and case-control, are used for much of the research in chronic disease epidemiology. In cohort studies, exposures and covariables are measured on a defined population of disease-free persons, who are then followed forward in time to determine which ones develop or die from the disease of interest. The methods and concepts of survival analysis, particularly the proportional hazards regression model, have greatly affected the statistical treatment of cohort data. They provide a mathematically rigorous framework for elaboration of the key epidemiologic notions of incidence rate and relative risk. Older epidemiologic techniques are given a new interpretation in terms of classical statistical theory, while the way is paved for the development of more flexible and powerful methods of data analysis.

Case-control studies involve samples of diseased and nondiseased persons whose history of exposure is known. The demonstration that the exposure odds ratio calculated from case-control data approximates the ratio of disease incidence rates among exposed and nonexposed was of paramount importance in establishing the scientific validity of this design. More recently, biostatisticians and econometricians independently have developed methods for the analysis of case-control and other data where the sample is selected on the basis of the outcome of primary interest. Further elaboration of this methodology is needed to handle more general stratified designs and for situations where only partial information is available for a large number of the sampled subjects.

Additional work is needed also on methods of analysis of epidemiologic data with dependent outcomes, such as arise in longitudinal studies with repeated measurements on the same person over time or in genetic studies of the patterns of disease in families. Better techniques are needed for assessing the magnitude of measurement errors and to correct for the tendency of such errors to dilute the observed association between exposure and disease. Recent advances in statistical computing and in the statistical theory of quasi-likelihood analysis based on generalized estimating equations promise rapid advances in this field.

Finally, AIDS, the major epidemic of our time, poses urgent challenges to the biostatistician and epidemiologist. One problem is to estimate the past, present, and future rates of infection with HIV so as to determine the future number of AIDS cases. Another is to understand better the patterns of HIV transmission within and between various pools of susceptible individuals so as to be able to plan the optimal organization of community resources for the prevention of further infection. The collection of data related to AIDS is seldom routine and often suffers from a lack of key pieces of information, so that studies are rarely amenable to straightforward analysis. Mathematically, the problem of estimating HIV infection rates, using data on AIDS incidence rates and the distribution of time from infection to diagnosis, can be viewed as an unusual type of deconvolution problem. It is related to problems that occur in the field of image processing, where rapid progress has been made. But the lack or poor nature of key types of data makes it much more formidable.

Note

[1] Goldhammer, M. I., and Rubbert, P. E., "C.F.D. in design: An airframe perspective," *Proceedings of the 27th Aerospace Sciences Meeting, January 9–12, 1989*, Publication Number 89-0092 (American Institute of Aeronautics & Astronautics, Washington, D.C.).