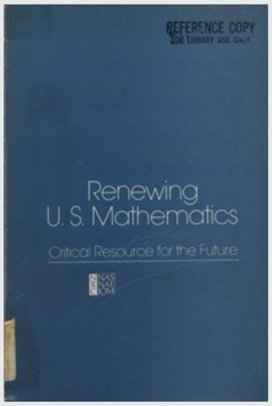

Renewing U.S. Mathematics: Critical Resource for the Future

DETAILS

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Renewing U.S. Mathematics

Critical Resource for the Future

Report
of
The Ad Hoc Committee on
Resources for the Mathematical Sciences

The Commission on Physical Sciences, Mathematics, and Resources
National Research Council

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

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

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OFFICE OF THE CHAIRMAN

It is well understood that several U.S. national objectives--primary examples are national security, economic strength, and the quality of life--are increasingly linked to our success in introducing new technologies that have been made possible by recent scientific advances. It is less fully appreciated that, in many diverse fields, scientific progress is stimulated by advances in mathematics, which defines the foundations of many of the sciences.

In recent years, the group within the National Research Council that oversees our work in the physical sciences became concerned that the nation was not taking full advantage of the potential of the mathematical sciences. Accordingly, the Council empaneled a group of outstanding scientists, many of whom, including the panel chairman, Edward David, represent scientific fields that use the results of mathematical research. The panel's task was to assess the adequacy of U.S. resources in support of mathematics.

Renewing U.S. Mathematics is the product of that assessment. The panel discovered that recent funding increases in the computer sciences actually mask a downward trend in federal support for mathematics itself. The report lays out a bold remedial program that the panel believes is needed if we are to keep the mathematical sciences in the United States at the world forefront.

We should not take for granted the broad practical payoff that derives from advances in pure and applied mathematics. I hope this report will play a part in helping the government, the public, and the scientific community itself to understand the risks we take if we neglect this crucial resource.

Frank Press
Chairman

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May 10, 1984

Dr. Herbert Friedman
Commission on Physical Sciences,
Mathematics and Resources
National Research Council
2101 Constitution Ave., N.W.
Washington, D. C. 20418

Dear Herb:

I am pleased to submit to you the report of the Ad Hoc Committee on Resources for the Mathematical Sciences.

The mathematical sciences research effort in the United States has been the strongest of its kind in the world, with a dazzling record of accomplishments over the last several decades. It has the potential for even more impressive contributions to the technical enterprise in the future as technology and society become increasingly mathematicized.

The remarkable opportunities which exist cannot be realized unless bold action is taken by the Administration, Congress, and the research community to restore extra-university support of mathematical research to a level commensurate with support for the general scientific and technological effort of the country. We were astonished to find that over a 15-year period federal support for this field, fundamental to the country's technology, economy, and defense, deteriorated so significantly that in 1982 it stood at less than two-thirds its 1968 level in constant dollars. Consequently, the field is not renewing itself; the necessary level of research cannot be sustained; and erosion is evident in the major university departments which embody mathematical research.

We have spoken more about resources than is usual in reports of this kind. We believe this emphasis is not beyond our charge and accurately reflects our findings. We have provided an analysis and recommendations for what is necessary for renewal of the field, for sustaining its research effort, and for capitalizing on future opportunities.

Yours truly,

Ed

Att.

The Committee on Resources for the Mathematical Sciences

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The purpose of this committee was to report on the extent, nature, and adequacy of support for research in the mathematical sciences in the United States.

Commission on Physical Sciences, Mathematics, and Resources

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SUMMARY

I. 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 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.

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

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 breathtaking 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 underpinnings 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

[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.

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

[3] These research opportunities are discussed in detail in Chapter II.

- 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

[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.

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 1968–73, 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 supercomputers 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.

I. INTRODUCTION

The reputation and achievements of the American mathematical community place the United States first among the nations of the world in mathematical sciences research. The tools—the concepts and techniques—which mathematical scientists have created, and continue to create, play a vital role in the advancement of science and technology in our country, as well as in its defense and economic development. As these tools are developed and refined, they also feed into a broader mathematical effort in the training of technical manpower and the general education of citizens. It is important to the country that mathematical sciences research remain vigorous and productive.

A. VITALITY OF THE MATHEMATICAL SCIENCES

We shall assess the current strength of mathematical research by:

- discussing the accomplishments of the mathematical sciences, both historically and in terms of their potential contributions to society
- examining the health of the institutions and organization systems through which research is conducted.

The first task is difficult, because much of mathematical research is unfamiliar to people outside the field and is therefore not easy to describe. Mathematical research baffles the general public. Precision and logic, so fundamental to mathematics, appear antithetical to exploration and discovery. Moreover, we often encounter mathematics first through seemingly arbitrary rules which foster the illusion that mathematical techniques or theorems were not searched out but were somehow always there, presumably handed down in one great mathematical utterance some time in the dim past.

Even the scientific or technological public, well aware that mathematics is ever-changing and discovered by people, is unfamiliar with large portions of the subject. A normally affable discussion takes place with regularity over whether the difficulties in understanding mathematics are inherent in the subject or result from the mathematicians' failing to make their subject comprehensible to outsiders. To deal with this communication problem, we have (i) included Appendix A, which describes the varied approaches to research in the mathematical sciences, delineates its boundaries, and discusses the size and other characteristics of the research community; (ii) augmented our discussion of the health of the field in the main text by including as Appendix C a paper by Professor Arthur Jaffe, *Ordering the Universe: The Role of Mathematics*, which talks about the importance of mathematics to science and technology.

The "invisible" character of much mathematical research also suggests that the field is small, but the academic research community is about the same size as that of physics or chemistry: 9,000–10,000 members. Indeed, its faculty component is larger than in those two fields.[1] The scope and diversity of the mathematical sciences are vast. The field's rapid development and expansion keep pace with other sciences and technology. If one does not know what is "out there" in the newer parts of mathematics, it helps to remember that calculus, which seems rather advanced to the public, was at the frontier of mathematics in 1700. The development of mathematics in the ensuing 284 years has been as dramatic as the general development of science and technology.

Our discussion of the health of the institutional structures for mathematical research begins by reviewing their development through post-World War II university-government cooperation. We describe some serious problems, especially those confronting the major university departments. These centers of research are widely spread around the country and form the matrix which holds the mathematical research community together.

B. SUPPORT OF RESEARCH

The discussion of support for the field is divided into two parts: an historical analysis of support up to 1982; and future planning.

What we found is complex and unusual. We identify a substantial support deficiency in mathematical sciences funding compared to its allied fields in the physical sciences and engineering. This deficiency resulted from events in the period 1968–73 and doubled the negative impact of the slow squeeze that various other fields of science felt over

[1] See section IV-D and Appendix A.

the same period. We suggest how to deal with the built-in deficiency and provide funding adequate to capitalize on the exciting opportunities mathematics and its applications offer.

The country's mathematical research community finds itself in a deeply serious, highly unusual situation, despite its current vigor and past achievements. The field is not renewing itself. It lacks the resources to perform the seminal mathematical work on which the future depends. Bold action, by a number of groups, will be required to maintain the health and quality of research and seize the remarkable opportunities currently available. Our recommendations focus on these points.

C. SCOPE OF THE MATHEMATICAL SCIENCES

The mathematical sciences research community includes:

- pure mathematicians, who create the discipline itself;
- applied mathematicians, who develop mathematical tools, techniques, and models to understand scientific phenomena or solve basic technological problems; specialists in numerical analysis and scientific computing;
- statisticians, who develop and apply mathematical techniques to analyze and interpret data for use in inference, prediction, and decision-making;
- mathematicians in operations research who develop and apply optimization techniques to management and decision-making;
- mathematical specialists in fields of engineering, e.g., communication and control theories;
- mathematical biologists, mathematical economists, etc.[2]

D. RELATIONSHIP TO COMPUTER SCIENCE

Computer science is not a branch of the mathematical sciences. It makes pervasive use of mathematics; however, it has its own sources of

[2] See Appendix A for a more complete discussion.

funding and has been recognized as a separate discipline for more than a decade. Prior to that time, academic institutions and federal agencies frequently grouped the theoretical parts of computer science with mathematics under headings such as "mathematics and information sciences," "mathematical/computer sciences," or, in a few cases, "mathematical sciences." Residues of these practices exist today. In reading this report and in using older reports or data on science and science funding, it is essential to maintain the mathematical sciences/computer science distinction.[3]

There will continue to be important intellectual activity along the boundary between the mathematical sciences and computer science, especially in the areas of theoretical computer science and scientific computing.

E. RELATIONSHIP TO EDUCATION

Research and education in mathematics have always been strongly coupled—they still are. Nearly all mathematical researchers also teach at the college level. Many are intensely involved in the early years of mathematics education. Concern for precollege mathematics and science education in the United States is great. The quality of today's mathematical education, at all levels, will determine the quality of tomorrow's research scientists. Literacy in science and mathematics must be the hallmark of any contemporary citizenry.

The full spectrum of mathematics education must be a high-priority item for the mathematical sciences research community. We have been forced to limit the scope of our inquiries and hence have not dealt in detail with the important topic of mathematics education. We are pleased to see the research community contributing to the national dialogue[4] and participating more directly in improving precollege education. Our report urges stepping up these efforts with the strong backing of the professional societies.

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[3] Ibid.

[4] For example, through the Conference Board of Mathematical Sciences' contributions to the National Science Board's Commission on Precollege Education in Mathematics, Science, and Technology.

II. THE MATHEMATICAL SCIENCES: STRENGTHS AND OPPORTUNITIES

The period since World War II has been one of dazzling accomplishments in science and technology, especially in mathematics, which is riding the crest of a wave of development rare in intellectual history. This flourishing of the discipline has run hand-in-hand with burgeoning applications. These applications, unknown before the War, today permeate the theoretical fabrics of many disciplines and make up important parts of the intellectual tool kits of working scientists, engineers, social scientists, and managers.

The mathematical sciences have become enormously diverse. Over the postwar decades, mathematical statistics came to full maturity; operations research was born; discrete mathematics with combinatorial formulations came into prominent use; mathematics in engineering, concerned with control and operations, optimization and design, flourished; numerical analysis allied with computing touched many fields. Traditional applied mathematics also greatly extended its reach and power and the discipline of mathematics itself developed at a breathtaking pace.

We shall discuss in detail only a few of the important developments and promising directions, within the context of changes in the dynamics of the field as a whole.[5] Our comments are amplified by Appendix C, in which Professor Arthur Jaffe's paper, *Ordering the Universe: The Role of Mathematics*, examines in depth the evolution of several areas. Professor Jaffe's personal perspective extends and enlarges upon our general remarks, given under these headings: (a) mathematics and technology, (b) mathematics in and as science, (c) trends, and (d) looking ahead.

A. MATHEMATICS AND TECHNOLOGY

The emergence of "high technology" brought our society into an era of mathematical technology, in which mathematics and engineering interact in new ways. Fifty years ago this was the pattern: mathematics

[5] See the Introduction and Appendix A for a description of the scope of the mathematical sciences. Note that computer science is *not* included among them.

made some tools directly for engineering but basically promoted the development of other sciences, which, in turn, provided the foundations for engineering principles and design. Mathematics and engineering now interact directly, on a broader, deeper scale, greatly to the benefit of both fields, and to technology. Here are six examples of the new pattern.

1. Communication

A mathematical work that marks the beginning of this new era is Norbert Wiener's classic paper, "Extrapolation, Interpolation and Smoothing of Stationary Time Series." Its ideas and results grew out of Wiener's work on gunnery problems during World War II, first appearing as a classified document which, because of its yellow cover and impenetrability to engineers, was affectionately known as "The Yellow Peril." Wiener's work, interpreted by his colleague, Norman Levinson, blended with the pioneering work of Kolmogoroff in the Soviet Union to form communication theory: the study of transmitting, coding, and decoding messages over noisy channels. Their results dealt with continuous signals and were augmented by the very different work of Claude Shannon, the founder of information theory. This collective work found significant application within the communications industry in areas as diverse as analog and digital voice, data, and image transmission; signal processing, in fields from radar interpretation to musical and physiological data analysis; and in data processing itself.

But such developments inevitably have other, far-reaching consequences. For example, the vast seismic oil exploration industry grew directly out of applying the Wiener/Levinson results to design and construction of equipment to filter noise and interpret seismic signals. Signal processing has played a vital role in exploratory geophysics, as it has in resolving bomb testing data, and in predicting earthquakes.

2. Control

Recent years have seen a major extension of the calculus of variations by Bellman, Hestenes, Lefschetz, Pontrjagin, and others, leading to the development of the theory of optimal control. A critical innovation of Kalman's changed the paradigm for filtering by introducing matrix Riccati equations. Optimal control with the Kalman filter played an essential role in guidance and control in the Apollo Program. Continuous, discrete, stochastic, and distributed control methods are now valued engineering tools. Modern problems span the range from operational control of continuous process manufacturing of semiconductor chips to the stability of large space platforms.

3. Management

Industry and commerce now apply mathematics to operations and management, a relationship which evolved from operations research, which itself grew out of logistical analysis in World War II. The optimization techniques of linear programming using George Dantzig's simplex method (1947) improved management decisions in varied industrial and business contexts, from routing tanker fleets, to optimal use of factory machines, to organizing transportation systems. Later developments in nonlinear and integer programming, effective methods for finding maxima and minima of nonlinear functions, broadened the range of applications and contributed to the emergence of operations research and management science as ongoing fields of inquiry. Along with game theory and other concepts, these methods serve as valuable production tools in everything from oil refining and other chemical processes to clothing design and manufacture; they are tools in operations, from bus scheduling, to military tactics, to stock market activities.

4. Design

The fuel-efficient Boeing 767 and European Airbus airfoils have been designed using a process involving an entire spectrum of applied mathematics:

- new physical behavior, such as shock motion and shock/boundary layer interactions;
- a system of nonlinear partial differential equations that change character as flow speeds change from subsonic to supersonic, so that new features of the solution must be understood and calculated;
- new analytical approximations to solutions of the system;

- powerful new numerical methods;
- efficient coding and storage of these methods which enable design calculations to be done economically.

Mathematical formulations and analysis in fluid dynamics —developed since the time of Euler and the Bernoullis—played an essential role.

Mathematical design of this complex type is applied to magnetic data storage disks, nuclear reactors, semiconductor chips, automobile bodies and other products. More powerful analytical and numerical methods, along with cheaper calculational capability, will make mathematics even more important in design.

5. The Computer

The development of computer technology has been strongly influenced by mathematics. The art of computation, numerical analysis, has been an important part of mathematics since it was systematically explored by Newton, Euler, and Gauss. Its importance has increased because of the development of high-speed digital computers. Here, we want to stress the importance of mathematics to the evolution of the machines themselves.

In the 1930s symbolic logic flourished. Church, Gödel, Post, and others studied formalized languages, and the mathematical notion of computability emerged from their work and Turing's. Around 1935, Turing constructed his abstract model for a universal computing machine. These developments provided the intellectual framework for the creation of both the stored program computer (by Von Neumann and his colleagues) and formal programming languages.

Computer science, in contrast to computer engineering, has a strong mathematical base. Mathematics underlies much of computer science and systems thinking: working out paradigms for artificial intelligence, from verifying the correctness of programs to the first robotic theories; developing the inner algorithms for operating system schedules, pagers, and dispatchers; providing the relational algebra and calculus of data bases. These are no accidents of history because understanding the capabilities of a tool which is essentially a calculator requires the kind of facility with precise forms of abstraction which characterizes mathematical thinking.

6. Alternative to Experimentation

Mathematics and computation are now forming a much larger place for themselves as an alternative to experimentation. This is a role that is not new to mathematics, but one that can now be played far more effectively using computational power. Mathematically prepared computational models are used to simulate complex structures, systems, or organizations in a number of industrial research, development, and manufacturing settings. Calculational models are used to design, optimize, and study effective operations in place of building costly petrochemical pilot plants. A large computational system, programmed to solve tough nonlinear partial differential equations, can do much of the work of expensive-to-build and expensive-to-operate wind tunnels. Analysis of a large space station for controllability, structural integrity, and general dynamic behavior must be done before the station is sent aloft. Huge calculations, requiring processing at over a billion computer instructions per second, are currently used to test the logic flow on integrated circuit chips before they are constructed.

In all of these examples, and in many others, the ability to mathematically represent the system or the structure and then the capability for efficient computation, are becoming an economic way to do the work of experimentation. We will see a great deal more use of this new kind of engineering and scientific tool in the future.

Mathematics is on the verge of its greatest involvement in technology.

B. MATHEMATICS IN AND AS SCIENCE

1. The Nonlinear World

Mathematics has always had a close relationship with the physical sciences. Continuum mechanics and mathematical analysis developed together. In the new physics of this century, mathematics has been available in advance of physical concepts (e.g., matrix and group theory for quantum mechanics or differential geometry for general relativity) and has developed with them.[6] In chemistry and biology, mathematics has begun to move forward swiftly in recent years. For example, reaction-diffusion mechanism study in both fields has involved the nonlinear generation of wave patterns, pulses, and shock fronts which are phenomenologically new and require new modes of analysis. In geophysical sciences, analytical approximation to atmospheric, oceanic, and elastic wave motions has produced new interpretations with which to forecast weather and predict earthquakes.

In all these fields, considerable interest focuses on the new, nonlinear phenomena associated with strong force and energy interactions, discrete-continuous interactions, or the more subtle low-energy nonlinearities of the biological world, phenomena which will dominate much of the mathematics of science from now on. We already see this in the fascination with solitons, chaos, and bifurcation and singularity theories.

In some ways, this is a testing time for mathematics because it requires developing far more complex concepts and structures than those of the 19th-century linear world. The work is well begun. Topological and analytical methods of ergodic theory and dynamical systems theory are helping unravel such challenging problems as turbulence.[7]

2. Gauge Field Theory

Mathematical research, driven by its inner dynamics, has developed concepts important for gauge field theory in physics. The physicist C. N. Yang wrote, "I found it amazing that gauge fields are exactly connections on fibre bundles, which the mathematicians developed without reference to the physical world." Algebraic geometry produced all self-dual solutions for the Yang-Mills equations. But the physical theory also had important consequences in topology.

Physicists introduced gauge theories in four dimensions (space-time) as a unifying principle in field theory. The study of Yang-Mills equations of motion led Donaldson to a remarkable description of certain four-dimensional spaces. A little earlier, Freedman, using purely topological methods, had produced a comprehensive theory of four-dimensional manifolds. In all other dimensions there is essentially one mode of doing calculus in a Euclidean space: Euclidean space of dimension n has a

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[6] See Jaffe's paper, Appendix C.

[7] A brief appreciation of where we stand on this problem is given in Attachment 2.

unique differential structure for $n \neq 4$; but an entirely different situation exists in dimension four—there are at least two different structures on four-dimensional Euclidean space. This qualitative difference between dimension four and other dimensions is a startling development for topology, and may also reflect deep physical principles.

3. Global Analysis

Global analysis currently employs not only differential geometry, topology, and Lie group theory, but also partial differential equations, functional analysis, quasi-conformal mapping theory, and ergodic theory. Some of its direct uses have already been commented upon. Its ideas have evolved over a considerable span of time.

Every scientist since Newton's time has resorted to calculus to determine the effects of physical laws. While ideal for analysis of gradual changes, calculus is often mute on large-scale nonlinear ones. Before 1945, global configurations study was still fragnented, its concepts difficult to communicate. To be sure, we must pay homage to the topological ideas of Poincaré, Cartan, and Lefschetz. But only after 1945 were grand syntheses erected from the fundamental structural elements developed since the 1930s (principally in France and the United States). These syntheses led to an almost complete understanding of not only the local geometry, but also the global character of the basic mathematical spaces. These are the homogeneous spaces Felix Klein singled out in his 1872 Erlangen program: geometries in which any point's situation is like any other's. They include spheres and flag manifolds on the one hand (on which compact groups operate transitively) and higher-dimensional generalizations of Riemann surfaces on the other. Homogeneous spaces form the basic building blocks with which to comprehend spaces arising in physics as well as mathematics.

4. Finite Groups

The mathematical concept of "group" was born in 1832 when Galois perceived the importance of systematically studying the general structure of permutations of the roots of polynomial equations. Widespread application of the theory of groups has developed in the ensuing century and a half—application to mathematics, physics, chemistry, and numerous other fields.

A complete classification of finite simple groups is now known. Even more remarkable than the solving of this 100-year-old problem is the nature of the solution itself. A famous 254-page paper by Feit and Thompson, showing that any simple group has even order, touched off a chain of developments which led to the final classification: any finite simple group is an alternating group, or is a finite version of a simple Lie group, or is one of 26 exceptional groups.

The exceptional groups have their own interesting stories. The Mathieu groups play a role in coding theory. The "monster," the last exceptional group whose existence lacked proof, was constructed by Griess in 1981. Its further study has led to a rich set of mathematical problems, involving the relations between the structure of the monster; the Griess algebra, of which the monster is the group of automorphisms; the Leech lattice, in terms of which Frenkel, Lepowsky, and Meurman have reconstructed the Griess algebra; infinite-dimensional Kač-Moody algebras; and classical automorphic functions.

5. The Mordell Conjecture

Mathematicians in algebraic geometry and number theory were astounded in the summer of 1983 to learn that a conjecture of 60 years' standing had fallen under the assaults of a German mathematician, Gerd Faltings. The Mordell Conjecture was first formulated in 1922. It deals with the number of rational points on algebraic curves of genus 2 or higher. It concerns the number of points having rational coordinates on curves defined as the solution set for a polynomial equation in two variables with rational coefficients. Mordell conjectured that the number of rational solutions was finite; Faltings proved it, using the enormous mathematical machinery constructed over decades to attack fundamental questions in number theory and algebraic geometry.

Faltings's proof brought with it progress on the conjecture known as Fermat's Last Theorem. One of the cases covered by the Mordell Conjecture was the equation $x^n + y^n = 1$. Its solution with rational numbers x and y corresponds to finding integer solutions of the equation $a^n + b^n = c^n$, about which Fermat had made his famous conjecture 300 years ago: there are no solutions in positive integers a, b, and c when n is greater than 2. Fermat wrote in his workbook that he had found a truly remarkable proof, which unfortunately the margin was too small to contain. The pursuit of a proof has intrigued mathematicians ever since.

Falting's proof that $x^n + y^n = 1$ has only a finite number of rational solutions is a significant step.

We come full circle in our discussion of the mathematical sciences by noting that number theory, long thought to be the purest of the parts of mathematics, is today of increasing use in constructing algorithms of practical importance in fields such as cryptography. The same is true of various parts of algebra and algebraic geometry. This should not surprise us, if we remember that one of the goals of algebra has always been to reduce problem solutions to algorithms.

C. TRENDS

1. Size and Strength

As the mathematical sciences grew in scope after World War II, the associated research community grew in size and strength. The 1966 *World Directory of Mathematicians* listed 2,900 U.S. mathematical scientists active in research. By 1970, the number had grown to 3,800; by 1982, it had reached 7,600. These totals do not include all of the research mathematical scientists, because the *World Directory* literature search misses several hundred applied mathematicians.

The strength of the research community is attested to not only by the sophistication and significant impact of applications such as the design practices we cited in the aircraft industry, but also by the fact that of the 27 Fields Medals, awarded quadrennially since 1936 at the International Congress of Mathematicians, eleven have gone to U.S. mathematicians, six to France, three to Great Britain, and two to the USSR.

The strength of direct contributions to other fields is reflected in the fact that three U.S. scientists were awarded Nobel Prizes for largely mathematical work in the two-year period 1982–83: Chandrasekhar in astrophysics, Debreu in economics, and Wilson in physics. Not long before, Cormack had been similarly recognized in medicine for his work on tomography.

2. Intellectual Trends

Some intellectual trends which have developed over recent decades prefigure future research.

(a) **The concern with nonlinearity.** We have already discussed a wide variety of nonlinear problems in science, including associated developments in mathematical analysis, topology, etc., so we will only repeat our conviction that the attempt to understand the nonlinear world will dominate large parts of mathematics in science in the years ahead.

(b) **The increased role of discrete mathematics.** For centuries people have been fascinated by puzzles and the algorithms describing the steps for their solutions. Many difficult mathematical problems have such a character. In recent decades this area has become formalized as combinatorics: the study of finite structures in which there are relations between the elements but (usually) no operations of an algebraic sort.

Such problems as network node location, routing of messages, and distribution of information have discrete combinatorial formulations and are of great practical interest. Along with the recognition of problem types and the development of algorithms has come the need to compute. The notion of complexity (degree of difficulty) has developed because many innocent-looking looking questions result in exponentially fast growth of computation as the number of nodes increases. The result has been a powerful and intriguing notion of completeness: can a calculation be done in polynomial time, that is, in a time related to the number of elements of the problem (nodes of a network), or in exponential time—something raised to a power equal to the number of nodes? This abstraction tells us when problems can be computed practically and when they cannot.

(c) **The increased role of probabilistic analysis.** Statistics, placed on solid mathematical footing through the work of Cramer, Fisher, Neyman, Pearson, and Wald, grew as a separate intellectual discipline during the postwar era, solidifying its academic base markedly through the 1970s. Advances moved from decision theory to sequential analysis, theories of robustness, and bootstrap/jackknife methods of data analysis and estimation.

Mathematical statistics is gathering momentum for another move forward. Reliability theory has both military and industrial applications. New statistical theories, taking advantage of modern computing power, are just emerging. Greatly enhanced capacity for handling data has helped develop powerful methods, free of Gaussian assumptions and linear mathematics, to challenge theorists and practitioners alike.

In physics, new classes of probability measures on function spaces have been constructed that describe phase transitions in statistical mechanics and establish existence of quantum fields. The solution of quantum physics problems by probability theory methods has become important to physicists and opened new research in probability theory as well.

Randomness in calculation dates back to the Monte Carlo method from the 1940s. Recently, randomizing algorithms—algorithms that are correct almost all of the time—have produced enormous savings in computer time (numbers of steps) with minimal risk of error. One such algorithm, vastly improving computer security, will soon be hard-wired into silicon chips. Such methods will be essential in the future and are all the more mathematically interesting since they depend on the structural properties of rings of polynomials, number fields, and permutation groups.

(d) **The development of large-scale scientific computation.**[8] Computers already affect all of science, and much of human endeavor. In the future some scientific fields will be completely dependent on the computer's highly accurate, reasonably cheap ability to solve approximately huge systems of equations. This has already happened in meteorology and climatology. New physical concepts, such as renormalization, will require vast calculations for their application. Large computations of this type have always moved with the leading edge of computational technology.

A host of three-dimensional problems exists in geophysics (e.g., oil recovery), aerodynamics, and engineering, which require new computer hardware and operating systems, such as array and parallel processors. These, in turn, demand new numerical analysis and algorithms. There is also challenge in doing the sort of mathematics which anticipates new computing mechanisms and guides in their construction. Mathematics of many kinds must be done as this new scientific computation generation gets under way.

D. LOOKING AHEAD

As always, looks ahead either extrapolate from the recent past or make guesses. Speculations about the future are especially risky in the

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[8] See Appendix A for a discussion of the relationship of scientific computing to the mathematical sciences and to computer science.

mathematical sciences because the field is very broad and its history is filled with unanticipated applications of great practical importance. The breadth of the field requires us to be highly selective in looking ahead, citing only a few of the promising areas, only a few of the opportunities. We shall speculate about some broad new areas of opportunity. Beyond what we describe, remember the diverse and sizeable research activity which continues to generate important concepts and tools for science and technology.

We have described an expanded use of mathematics in fields of science and technology that were already mathematically based, the rapid entry of mathematics into other fields, and the mathematical foundation of the newly formed sciences. This expansion, this mathematization, will continue and accelerate, for several reasons.

1. Proliferation of the Uses of Mathematics

(a) **Data handling and analysis**. In biology, the social sciences, commerce, industry, management, and government, there has always been a large amount of data. Modern data handling now allows for the systematic acquisition, storage, and analysis of the data; the stage is set for the empirical recognition of behavior and phenomena that will lead to rule and principle. Mathematics will play a role in this formative process, then allow generalizations, prediction, and further understanding to develop through solutions of the mathematical problems.

In many fields, one must look for patterns of behavior, rather than a single phenomenon. This is so in experimental psychology, and will be even more so in clinical psychology, and in attempts to formulate the psychological underpinnings of economics and sociological behavior. If one is trying to capture or recreate pattern formation using data from diverse sources, there may be a need for parallel data processing. Parallel processors are now being designed experimentally, and the related mathematics is just getting under way. It may be a skillful extension of existing sequential mathematics or require new approaches and techniques. The mathematics of pattern formation, recognition, transformation, and stability have also been forming in recent years and will move more quickly with increased demand.

(b) **Mathematical education**. Another driving force expanding the use of mathematics will be the large number of people who have received higher level mathematical education in recent years, education which includes the capability of using the mathematics. Students—especially those in MBA programs—emerge from business courses with a knowledge of linear programming, other optimization techniques, and statistics. They are already using these skills in production, finance, management, and marketing. The same is true of students in economics and psychology. Physics, chemistry, and computer science students will need mathematics of ever-increasing sophistication. Experimental scientists will count elements of signal processing, such as the Fast Fourier Transform, among their tools.

This is not, of course, new mathematics; it is the penetration of mathematics into much of the work of the world. It will *engender* the need for new mathematics, as it is doing today.

2. Interaction with Basic Science

In the traditional fields of science and engineering, as we have already mentioned in an earlier section, both discrete and continuous mathematics will contend with nonlinearity. Perhaps general principles of the kind that guided linear mathematics in the past will not be found, but examination of the new mathematical phenomena (chaos, solitons, etc.) has already begun.

A traditional area of contact between mathematics and other sciences has been mathematical physics. The frontiers of research in pure mathematics and in physics drifted apart after the advent of quantum theory some fifty years ago. We are beginning a new era leading to the reunification of many general ideas in mathematics with those of quantum physics. New opportunities for development cross the boundaries between the mathematics of topology, geometry, probability theory, analysis, and differential equations on the one hand, and the physics of quantum field theory, of semiclassical approximations to quantum fields (especially for gauge theories), and statistical mechanics (including the theory of phase transitions) on the other.

Manifestations of this trend include a diverse set of recent results: deeper understanding of integration over function spaces has been achieved as a byproduct of the construction of quantum fields; the development of "phase space localization" in quantum field theory as a tool to study eigenvalue spectra provides for reexamination of classical problems as well; the use of renormalization theory as the basis of a mathematical study of phase transitions and of localization yields striking results; as we mentioned, Yang-Mills theory played a central role in constructing an exotic R^4 and in understanding related topological problems; the new geometric methods developed to understand the positive energy theorem in relativity extend the theory of harmonic maps; the new proof of the index theorem inspired by supersymmetric quantum theory raises the possibility that index theory can be generalized; "anomalies" of quantum physics (classical equations of motion which fail in quantum theory) intrigue mathematicians and physicists all across the country as they attempt to understand them as an aspect of K-theory.

3. Higher-Dimensional Manifolds

A major new possibility in the discipline of mathematics itself is that three-dimensional and four-dimensional manifolds may prove as rich in structure as the two-dimensional underpinnings of complex function theory, with as many applications. If Riemann surface theory and the associated analysis were a guiding concern for the century 1860–1960, so may the study of manifolds of dimensions three and four and related analysis be for the decades ahead. Thurston's work in dimension three and the role of self-duality in dimension four suggest this. The work leading to the nonuniqueness of differential structures on Euclidean space of dimension four, is, we suspect, just the tip of an iceberg.

4. Computing

One of the largest stimulations and challenges for mathematics, and one of its greatest opportunities, will come from computation and computers.[9] The mathematics of computation now means the preparation and analysis of algorithms, the numerical treatment of those algorithms, and the optimal preparation and use of the numerical analysis on computing systems. It means even more.

Qualitative mathematical understanding is required to determine whether the ensuing numerical solutions are meaningful: Are they unique? Are they stable? Is the dependence on conditions and parameters reasonable? We must study the complexity of the algorithms

[9] Again, see Appendix A for the relation of the mathematical sciences to computer science.

to know whether the calculations are economical. Mathematics will be increasingly required in designing almost all aspects of the computing system itself.

5. Changes in the Research Community

In the recent past, the range of applications of mathematics has been dramatically expanded, while the discipline of mathematics itself significantly enlarged its scope and deepened in complexity and abstraction. These developments have increased specialization, and the erection of barriers here and there, to separate "pure" from "applied" mathematics, or the two of them from statistics, operations research, or mathematics in engineering.

We believe that the face of the mathematical sciences is currently changing in two important ways:

- Unifying ideas, blurring the boundaries of the major disciplines, have regenerated a sense of wholeness, despite vast size and scope. Diverse mathematical scientists again see themselves participating in a common enterprise.
- Mathematics is increasingly looking outward, toward its interactions with science and technology.

There is a heightened awareness that sophisticated and abstract systems of mathematical thought, developed only because of man's drive to understand order, turn out with surprising regularity to find application in science. There is increased respect for the wealth of mathematical ideas generated by those who pursue mathematics precisely because of its direct contributions to science or engineering. There is increased appreciation of the continuity of methods and ideas across the spectrum of the mathematical sciences.

The changing face of mathematics suggests that we are entering a new era, that we have just begun to see the power of the mathematical machine created over the last several decades, and that what lies ahead could be even more impressive.

III. INADEQUATE SUPPORT: LEGACY OF THE PAST

The remarkable developments just described were nurtured by a sometimes unarticulated pact between the universities and the federal government, rooted in successful university-government research projects during and just after World War II, and in feverish post-Sputnik commitment to strengthening scientific and technical education in the United States. The resulting injection of federal funds for research into universities, combined with faculty growth accompanying greatly expanded enrollments, attracted numbers of the best young minds in the country to science and mathematics and propelled the United States into world leadership in the mathematical sciences.

Although that leadership continues today, there are doubts about the years ahead. Extra-university support of mathematical sciences research[10] is inadequate to sustain the present quality and level of research effort, much less provide for renewing the field or capitalizing on future opportunities. We will identify marked inadequacies of extra-university support for the most basic needs of research scientists in mathematics, tracing the history of how the present funding situation evolved and describing the impact of weak federal support. Finally, we will extract from this history some basic conclusions which bear on the future.

A. THE RESEARCH COMMUNITY AND ITS NEEDS

The mathematical sciences research community in the United States has more than 10,000 members. About 9,000 of them are on faculties of the nation's universities and colleges. Additional groups are located at the "nearly academic" research centers: the Institute for Advanced Study at Princeton, the Mathematics Research Center at Madison, and two new institutes developing under NSF sponsorship, the Mathematical Sciences Research Institute at Berkeley, and the Institute for Mathematics and its Applications at Minneapolis. There are also several research groups in industry, the most prominent at Bell Laboratories and IBM, with others in the petroleum, aerospace, and defense industries. In government, basic work is being conducted at Argonne, Los Alamos, Oak

[10] In discussions of federal support, it is especially important not to confuse the mathematical sciences with computer science. See Appendix A.

Ridge, Sandia, and Lawrence Livermore National Laboratories, and at the Institute for Defense Analyses in Princeton, the National Bureau of Standards, the National Security Agency, and other agencies. The output of all these groups is extremely important, but we would point out that collectively they house less than 10% of the mathematical sciences research community.

A mathematical research scientist needs: (a) time to think and an appropriate place in which to do it; (b) interactions with developing young investigators (graduate students and postdoctoral fellows); (c) interactions with research associates, e.g., visiting faculty; (d) a certain amount of equipment (usually computational); and (e) support staff (primarily secretarial). Mechanisms for exchanging results, such as publications and conferences, are also important. In these respects, mathematical scientists are much the same as other scientists.[11]

B. INADEQUACIES

Figure 1 shows how research time in universities is paid for in the sciences and in engineering. In contrast to other fields, most mathematical sciences research is carried by the universities; a markedly smaller fraction is borne by the government.[12]

Figure 2 and Table 1 show federal support for graduate research assistants and postdoctorals. [13] In interpreting them, one should be aware of some approximate sizes. Academic research communities number: chemistry, 10,000; computer science, 2,000; mathematics (mathematical sciences), 9,000; physics, 9,000.[14] The approximate annual Ph.D.

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[11] Typically, equipment needs are less for mathematical scientists. Computation is changing this pattern, however.

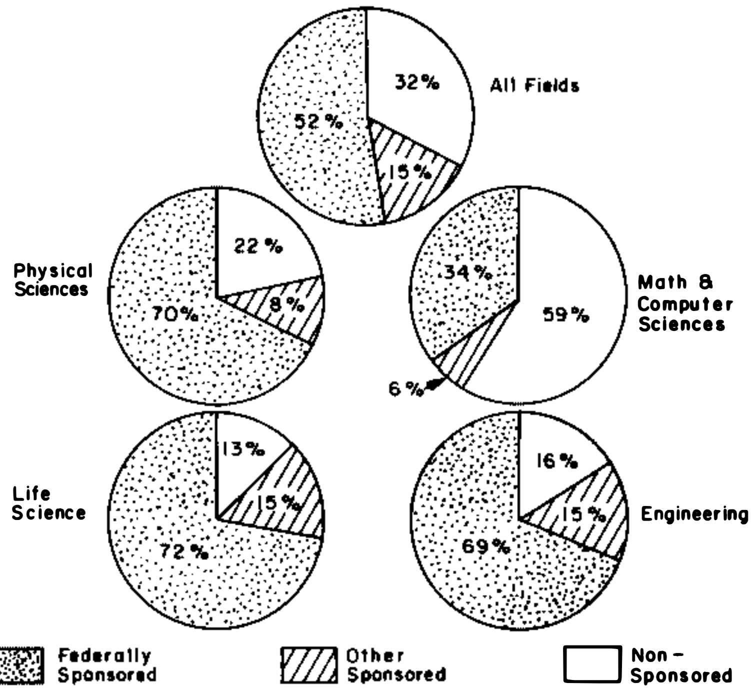
[12] Separate data were not available for the mathematical sciences and computer science. Were computer science removed from the "math and computer sciences" piechart in Figure 1, the federally-sponsored percent would decrease. The effect would be relatively small, however, because the academic research community in the mathematical sciences is much larger than that in computer science.

[13] The data in Figure 2 are only approximate, of course. The federally-supported portions of the columns labelled "mathematics" would be half again as high for the broader field of the mathematical sciences. The qualitative impact of the data would not be affected by this change. Predoctoral fellowships/traineeships could be added to the columns to obtain total graduate students federally supported, adding about 60 to the "mathematics" column and larger numbers to chemistry, physics, and computer science, but producing little qualitative change.

[14] For the 50 major research universities, the mathematical sciences faculty is much larger than those in chemistry and physics. Postdoctorals and research staff make the total academic research groups comparable in size.

Figure 1

Research Time in Universities
November 1978 – October 1979

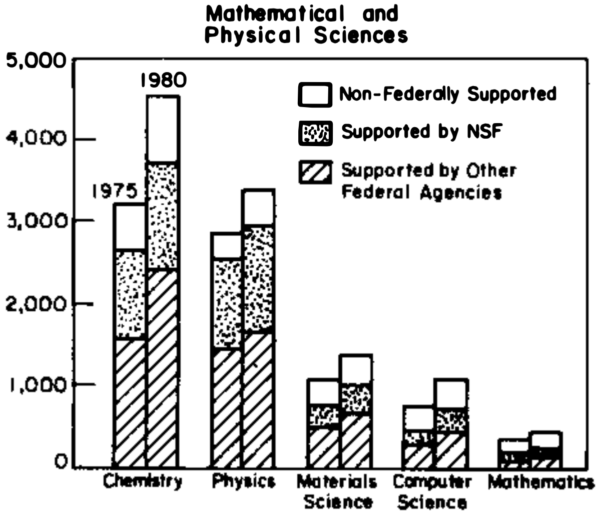


Source: National Science Foundation

productions are: chemistry, 1,500; computer science, 200; mathematics, 800; physics, 800. Thus the explanation for the cross-disciplinary disparities within Figure 2 and Table 1 is not that "mathematics is a small field." The notion that the field is small is prevalent and probably results from the fact that total dollar outlays for mathematics, in terms of industrial and federal budgets, or space and technical staff needs, will always be smaller than those in other sciences because of the great difference in equipment requirements. The mathematical sciences have enormous intellectual diversity and output; nearly all the practitioners are in colleges and universities. As a result, the faculty research group in the field is larger than that in either physics or chemistry. The total academic research communities are roughly comparable in size, as we have noted.

Figure 2

Graduate Students with Research Assistantships Enrolled Full-Time in Doctorate-Granting Institutions



Source: National Science Foundation
Status of Science Reviews, 1983

TABLE 1. Postdoctorals in Graduate Institutions, 1981

	Total	Federally Supported	Non-Federally Supported
Chemistry	2,870	2,465	405
Physics	1,450	1,217	233
Mathematical Sciences[a]	99	56	43

Source: National Science Foundation

[a] This number excludes about 75 university sponsored "research instructors" in mathematics.

Figures 1 and 2, together with Table 1, show that mathematical research funding from the federal sector had become very thin by 1980 because, in the mathematical sciences, research time, graduate students, and postdoctorals together account for a very large fraction of the needed support. Since little money flows into these three categories collectively, little money flows in at all.

As in other fields of science, the university-government cooperation which built up our powerful mathematical sciences research machine required the injection of federal grant funds sufficient to support adequate portions of the mathematicians' research and the work of affiliated graduate students, postdoctorals, etc. In fundamental areas of pure and applied mathematics plus statistics, the government's contribution to the cooperative effort is considerably less today than it was in 1970. We shall describe how this came about and what the effect has been.

C. A BRIEF HISTORY

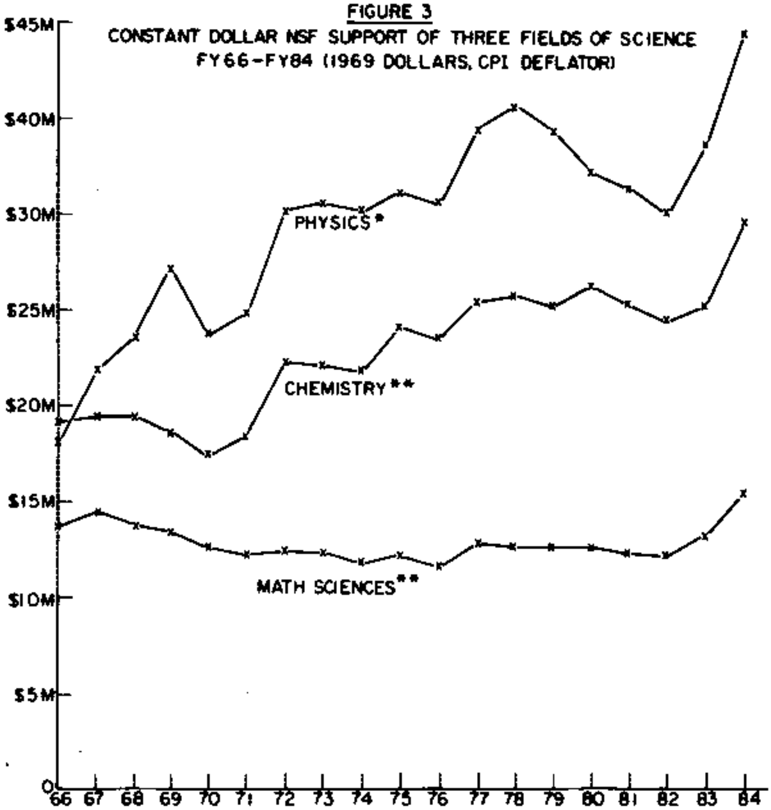
Weakening of federal support for the mathematical sciences began as long as 15 years ago. Government agencies which had supported research in the field began to focus on short-term results and to be impatient with the long periods of time required to bring the fruits of some mathematical research to bear on mission-oriented problems. The 1969 Mansfield Amendment limited investment in basic research by the Department of Defense. Presidential and Congressional actions dramatically reduced numbers of federal fellowships shortly therafter. The National Science Foundation was left to support both the researchers dropped by other agencies and the graduate students in research, but the resources with which to do this were never added to the budget of its Mathematical Sciences Section. Over one-third of the total federal support for the mathematical sciences was lost in just five years (1968–73).

The following decade, 1973–83, showed flat funding levels in constant dollars, while the field doubled in size. Other fields of science grew just as rapidly as did the mathematical sciences during this period. They, too, had to adapt to federal policy changes in the early 1970s and then survive a decade of relatively slow growth of support. But the situation of the mathematical sciences was extreme: (i) lacking industrial support, they turned to NSF when cutbacks occurred; (ii) very few of their people were supported by other agencies, except DOD, where programs had to be reconstructed in response to policy changes; (iii) their budget at NSF grew at a very slow pace.

Figure 3 shows in constant dollars the budgets for the mathematical sciences, chemistry, and physics at NSF, from FY 1966 through FY 1984.

FIGURE 3

CONSTANT DOLLAR NSF SUPPORT OF THREE FIELDS OF SCIENCE
FY66–FY84 (1969 DOLLARS, CPI DEFLATOR)



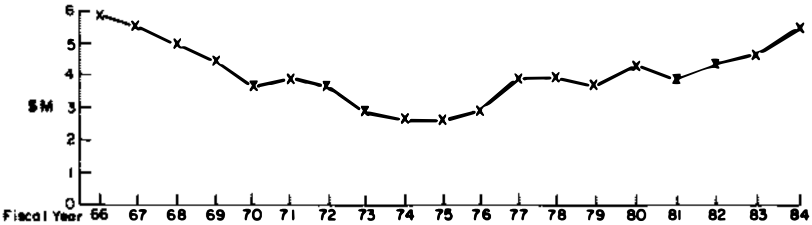
SOURCE: NATIONAL SCIENCE FOUNDATION BUDGET
*SOLID STATE AND LOW TEMPERATURE PHYSICS NOT INCLUDED.
**COMPUTER SCIENCE NOT INCLUDED.

Note that (i) although chemistry and physics dipped at the end of the 1960s, recovery was rapid; (ii) mathematical sciences funding declined until 1973, then stayed extraordinarily flat in constant dollars through FY 1982; (iii) not until FY 1984 did mathematical funding at NSF regain its FY 1968 level.

To fully appreciate the significance of this, one must see what was happening at DOD, and be aware that it was the only other major federal supporter of the mathematical sciences. Figure 4 shows in constant dollars the evolution of Air Force support of academic research in the mathematical and information sciences from FY 1966 through FY 1984.

Figure 4

AFOSR Contract/Grant Program in Mathematics and Information Sciences
(in 1969 Dollars)



Source: Air Force Office of Scientific Research

It decreased about 42% from 1968 to 1973. In fact, the decline began in the mid-1960s, and continued steadily until 1975, dropping 52% from 1966 to 1975. Some recovery has occurred since the mid-1970s, primarily in the information sciences (computer science and electronics).[15]

We can summarize our quantitative conclusions about the history of support as follows. The mathematical sciences provide a most dramatic example of weakening of support through the sequence of post-1968 phenomena described at the beginning of this section. Our findings indicate that

- federal support for the mathematical sciences research enterprise stood in 1982 at less than two-thirds its 1968 level (in constant dollars);[16]
- the principal reduction occurred during the period 1968–73;
- it was followed by nearly a decade of zero real growth in support;
- these budgetary events occurred during the peak in growth of the field—growth in the range and depth of uses of mathematics, with a concomitant doubling of the number of mathematical scientists productively engaged in research.

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15 The mathematical sciences portion of the program stood in FY 1982 at about 75% of its FY 1968 level. Detailed programmatic data are not available for the intervening years. See Appendix B for a discussion of difficulties involved in gathering and interpreting support data.

16 Detailed analyses to support this conclusion are found in Appendix B, section B VIII.

D. REASONS FOR DECREASED FEDERAL SUPPORT

Let us examine more closely why and how these unusual budgetary events occurred.

1. General Reasons

Four reasons why "mathematics" seems to have been the field hardest hit by the general post-1968 trends lie fairly close to the surface:

- Research in the mathematical sciences is concentrated almost entirely in universities and colleges; hence it is very strongly affected by any general weakening of the support of academic research.
- Much (but not all) mathematical research has long-term payoffs; thus the field will be strongly affected by federal policy shifts which emphasize mission relevance or immediate applicability to technologies.
- The long periods of time involved in developing many important mathematical tools make it unlikely that the commercial sector will support large fractions of the research; therefore, relatively little help will be found from industry when there is a weakening of federal support for fundamental research in the field.
- Mathematical scientists (as mentioned earlier) require relatively little in the way of facilities, equipment, or technical staff to conduct their research; hence, their needs are less visible and often seem postponable.

Other reasons will emerge as we probe more deeply.

2. Priorities

The biggest blow to mathematical sciences funding occurred in the early 1970s. Although Congress earmarked some resources for transfer to the National Science Foundation in compensation for reductions in DOD support of mathematics, these resources never found their way into the budget of the Mathematical Sciences Section of the Foundation. In addition, one can see from Figure 3 that there was no budgetary growth in mathematics to compensate for the staggering losses which the field suffered when cutbacks in federal fellowships for graduate students and postdoctorals occurred.[17]

These events were directly related to the way the mathematical community then set its priorities in cooperation with the NSF staff. The community did not press hard for federal graduate student and postdoctoral support, because (a) mathematicians were extremely worried about an oversupply of Ph.D.'s in their field, and (b) at that time, the universities needed more teaching assistants and young faculty, and had the resources to hire them. A hole was left in the NSF budget where support for graduate students and postdoctorals was supposed to go.

In subsequent years, this priority pattern set by the mathematical community and NSF for allocation of resources at the Foundation had another effect: it left support of research activities in the field up to the universities to a far greater extent than they could really bear, after they began to experience financial hardships. This explains why the financial squeeze which is now plaguing the universities has hit the mathematical sciences especially hard; it also explains why the full extent of the funding problems in the mathematical sciences has become clear only in the last few years. University budget reductions and flattenings, although they had different patterns in various institutions, collectively cut back the support for a range of things which in earlier times the federal grants would have carried and which are essential to the research effort: postdoctoral positions, visiting faculty positions, secretarial help, travel, etc.

3. Masking and Inaction

If the finger-in-the-dike role of the universities for a number of years prevented the mathematical research community from grasping the deep seriousness of its federal support problems, why was it not apparent to federal budget and policy makers that something was wrong? A 33% constant dollar drop in support of a major field should have been readily discernible. There were two reasons.

First, during the 15-year period we have been discussing, computer science grew very rapidly. This important intellectual development also

[17] NSF Predoctoral Fellowships and Traineeships in mathematics dropped in number from 1,179 in FY 1969 to 116 in FY 1974. Several hundred NDEA fellowships were also lost.

affected funding patterns. Until seven years ago, computer science was lumped with the mathematical sciences in the federal budget under the banner "mathematics and computer science." The line item was, of course, growing nicely—because computer science, although much smaller than mathematics as a field, was expanding and involved more costly research. It was all too easy not to notice that funding for the "mathematics" part was not growing at all.[18]

We have already mentioned the second reason that the decrease in support was not noticed, namely, that around 1971 federal policy changes regarding the support of young people shifted resources in many fields from the fellowship to the research grant side of the ledger. What we mean, of course, is that there was significant real growth in the budgets for these fields at NSF and other agencies and that part of the added funds were (asked for and used) to increase research assistant and/or postdoctoral support. Since this real growth did not occur in mathematics, the money for graduate students and postdoctorals simply went away, at least at NSF. [19]

We may still ask why, in light of the drop in grant support during 1968–73, there was not an immediate outcry from the mathematicians. We can speculate that they were unaware of what was happening, that the mathematical community lacked the mechanisms through which to act, or even that attitudes about government support, especially from DOD, were affected by the turmoil over the Vietnam War. We can be certain, however, that an important part of the answer lies in the fact that the academic institutions initially carried just enough of the additional burden to obscure the problems. The major universities compensated for some of the lost research funding by maintaining reasonable teaching loads, supporting research during the academic year, providing some visiting faculty positions, picking up some graduate student support, and so forth, because they recognized the significance of mathematics and did not want the working circumstances of mathematical scientists to get too far out of line with those of other scientists. The universities continued to increase their faculties and to use entry-level faculty posi-

[18] An example of this is seen in Figure 4, where post-1975 growth of the AFOSR Math and Information Sciences program disguises the relative flatness of the "mathematics" portion.

[19] During the years 1968–73, the budget of NSF's Mathematical Sciences Section grew at less than 2% per year. In the critical years 1970–72, there was a 9.5% growth compared with growths of more than 40% in fields such as physics and chemistry. See Appendix B.

tions as a partial substitute for postdoctorals. They were able to do this because they were still growing and in a reasonable state of financial health.

E. CONSEQUENCES OF INADEQUATE SUPPORT

1. Impact on the University Centers

By the mid-1970s, the financial squeeze on the universities had begun and the academic job market had tightened in numbers of fields, including the mathematical sciences.[20] Graduate programs in departments perennially strong in research began to shrink; national Ph.D. production dropped from 986 in 1972–73 to 744 in 1982–83, and the percentage of doctorates to U.S. citizens dropped from 78% to 61% during that period. Instructorships and junior faculty positions were reduced. At many mathematics departments, undergraduate enrollments mushroomed, driven by the needs of students in engineering, computer science, and the social sciences. The universities were unable to respond with comparable increases in teaching staff (there was usually no increase, in fact); hence class sizes and teaching loads went up, cutting into faculty research time and placing much greater responsibilities on teaching assistants.

Meanwhile, federal funding for research deteriorated steadily, because funding levels had taken no account of the growth of mathematical sciences. By 1982, federal support per active researcher was a third of what it had been in 1968. Most NSF research grants had been stripped down to support only summer research; hence, there was very little support and no flexibility. There were no funds in the grants to compensate for university cuts. There was little postdoctoral money, virtually no research assistant money to give the overloaded teaching assistants time to concentrate on research during thesis writing, little secretarial or travel money, or even money for duplication of essential documents.

The situation worsened. Even at historically high-ranked departments, the number of established mathematical scientists receiving outside support decreased noticeably over the last several years.

[20] Not in the subfields of statistics or operations research.

The chairman of a prestigious mathematics department wrote in a letter to the Research Briefing Panel on Mathematics[21] in the fall of 1982:

> "Mathematical research has been flourishing in the past decade but the institutional structure of mathematical research is in trouble. Recruitment of young talent for the future looks to be in even more serious trouble. The level of research support has been very low in terms of the percentage of active research people supported, and recent cuts in support have produced signs of a serious deterioration of morale, especially among younger mathematicians."

Another chairman wrote:

> "We are some one hundred in number. We are invariably ranked among the top twelve departments in the country, we continue to recruit good graduate students, and I claim with confidence that of the one hundred at least ninety are seriously engaged in research and scholarship. Yet, after two severe years, we are down from one-half to about one-third of the faculty on NSF grants. Moreover, we have sustained these severe losses without any sense of the prevalent quality of work having declined at all; on the contrary, several colleagues have lost grants in the very year when they have done their best work. Here, for example, loss of NSF grants has reduced departmental income from overhead just when the university, which in any case had always counted on strong departments like ours to earn much of its research support outside, is quite unable to raise the level of state support."

We are seriously concerned. Morale at many of the major mathematical science departments is low, and promising young persons considering mathematical careers are put off.

In most fields of science in the United States, the major university departments are at the center of research activity. In mathematics, there is little elsewhere: there are no national laboratories devoted expressly to the mathematical sciences and no special large facilities providing unique research capabilities. There is less concentration of research than in fields where cost prevents replication of expensive equipment at more than a few institutions. *The network of university centers embodies mathematical sciences research. It is in trouble.*

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[21] Panel of the National Academy of Sciences Committee on Science, Engineering, and Public Policy. Its report is Attachment 1 to this report.

2. Delayed Impacts

The trouble we see could not be described as a crisis; the field is not faced with the imminent collapse of the major university research centers. What we do see is that several basic problems related to inadequate support have built up slowly over the years to near boiling point.[22] This is what comes through vividly in the letters from department chairmen. What also comes through is their clear sense, which we share, that unless something is done to alleviate the funding problem, we cannot expect the field to continue to perform at its customary high level.

The inevitable question is: If increased funding is necessary for the future health of the field, how have the mathematical sciences done so well over the last 10 to 15 years? Part of the answer, as we have noted, lies in the universities' supportive role, which delayed the impact of federal funding reductions. That role, although still strong, has diminished and needs augmentation. We believe the more important point is that we are talking about an almost entirely theoretical branch of science with a relatively secure base in the universities. In such a field, 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. What happens is more akin to malnutrition; the general enterprise begins to slow down. There is a considerable lag time even for the slowing down, when it comes to 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 squeeze, an effort of very high quality can be sustained in this way for quite some time.

This is what has been happening in the mathematical sciences in the United States for over a decade. The field has been living primarily off the investments of human and dollar resources made in the late 1960s.[23] But tangible signs of erosion have surfaced: Ph.D. production

[22] The development of these problems in mathematics was described clearly eight years ago in the Smith-Karlesky study *The State of Academic Science* (Change Magazine Press, 1976).

[23] The most recent U.S. Fields Medalists were people who received their Ph.D.'s around 1972.

has slowed; there are problems in the university centers, as we discussed; the field is not renewing itself.

One may also ask whether the quality and level of the research effort are being maintained now: Can we already see that research output has fallen off? The tangible warning signals and common sense tell us that it must have slowed down somewhat and surely will over the next decade, unless investments of human and dollar resources are increased. In any field of science it is difficult to discern on the time scale of 5 to 10 years whether the rate of generation of basic knowledge has changed. How does one see that an idea which might have been there is not? Presumably, the more creative the potential idea, the less noticeable will be its absence. This seems an especially important point in relation to the mathematical sciences, which develops tools for so many other fields. Without new tools, applications cannot be generated, but this effect may go unnoticed since people tend to abandon problems for which the required techniques are not available.

A physicist walked into the office of one of our Committee members recently, somewhat excited because he had found in the (Japanese) *Encyclopedic Dictionary of Mathematics* a rather complete listing of the homotopy groups of spheres and classical Lie groups. He remarked that this would be "quite useful to us." Understandably he was unaware of the fact that many decades of mathematical creativity, involving large parts of the careers of some of the world's outstanding mathematicians had gone into making that "list." What he would have done had he gone to the *Encyclopedic Dictionary* and found only a few scattered items of knowledge about homotopy groups we do not know, but we doubt that he would have paused to wonder about the level of society's investment in mathematical research over the preceding 50 years.

3. Imbalance in the Scale of Support

Reviewing the field as a whole, with the advantage of historical perspective, we easily perceive that the tools which the physicist, engineer, or biologist will need some 5, 10, or 50 years hence may not be there, given society's present inadequate investment in the mathematical sciences. But what level of support or investment is adequate?

The first answer, we believe, comes from comparing support for the field to support for the rest of science and technology. Some broad-brush comparisons were made in Figures 1 and 2, plus Table 1. Rather telling data were also gathered by the Office of Mathematical Sciences of the National Research Council and presented to the (then) Assembly of Mathematical and Physical Sciences in 1981, supporting the request that led to the formation of this Committee. These data, from the science departments of 10 of the country's major research universities, gave the federal support per faculty member for research needs which all scientists share: research time, graduate students (academic year and summer), postdoctorals, visiting faculty and research associates, secretarial help. The support per faculty member in mathematics was less than one-third that of other sciences, and this was true in every category except research time in the summer. Something was badly out of balance.

Did such imbalances in "major" universities reflect less concentrated use of resources in the mathematical sciences, in the sense that too large a percentage of researchers was supported? No, the opposite is true. Of the academic mathematical scientists in the country with research as their primary or secondary activity, about 20% have some federal support. In chemistry the analogous number is 50%. In physics it is 70%.

The comparison of support for the mathematical sciences with support in other fields is not an issue of fairness. Mathematical research is a vital part of the scientific research effort. Looking at developments in the other sciences offers a scale by which to measure mathematics funding. The imbalances which now exist will lead to deterioration relative to the rest of science, and an inability of the field to continue to generate the concepts and tools needed for future science and technology. This could be particularly serious as society (and, in particular, science) becomes increasingly mathematicized.

For academic mathematicians and their institutions, funding inequities across the sciences create real problems. At every major university, the mathematicians teach more, as do their graduate students, while for virtually anything important to their work, they have less help and less money than their colleagues in other fields of science and engineering.

If mathematicians teach more and have less help, less research is done; if there is practically no postdoctoral support, little postdoctoral education takes place; if virtually all graduate students are supported by teaching assistantships, intense concentration on research for dissertations is less possible; if the direct operating expenses connected with research are transferred to universities, there is less money for teaching staff and burdens increase; and, perhaps most importantly, if a range of such conditions obtains, the field will be less attractive to gifted young people. Should such conditions continue over time, the development of mathematics will be slowed and the scientific/technological effort of the country impaired.

The level of support for mathematical sciences research in the United States has come to be markedly out of balance with the level of support for the country's general scientific and technological effort. Because of the central role of the mathematical sciences in that effort, corrective action to bring the support back into balance must form the base in planning for future funding for the field.

IV. FUTURE SUPPORT

Our discussions of the potential of the mathematical sciences and the history of consequences of its inadequate support provide us with some guidelines for future funding. We will develop these and analyze needed dollar support.

The analysis must do more than consider budget increments. Not only is the general level of support of mathematical research out of balance with that for other sciences and technology, it is weak across the entire spectrum of the mathematical sciences, for every major type of research support need: graduate students, postdoctorals, young investigators, senior investigators, support staff, etc. Our analysis will suggest how to reset levels of support for major research needs and project total dollar amounts necessary to put federal support of the mathematical sciences back on track and capitalize on future opportunities.

A. IMPORTANCE OF MATHEMATICS

Our society is becoming increasingly mathematicized. Mathematical education at all levels must be strengthened. Mathematical research to generate the new tools which science and technology will require must be supported.

B. GENERAL GUIDELINES FOR FUTURE SUPPORT

- Mathematical sciences research is intertwined with mathematical education, in itself of extreme importance to the country; hence the principal channel for support of research in the field should be through continuing university-government cooperation.
- We should support mathematical sciences research on a broad intellectual front, recognizing that mathematics provides tools and personnel for science and technology in many ways. Predictions as to what mathematics will or will not be of practical importance years from now are too often wrong.
- There is a further set of budgetary problems which the mathematical sciences face, problems of how available resources are utilized. These must be dealt with in planning for future support.
- The lack of industrial support for research in the mathematical sciences has weakened overall support to a degree much greater than any potential dollar amounts from that sector might indicate. Relations between the mathematical sciences and industry must be further developed.

1. University-Government Cooperation

The federal government must support the core of the research activity, as it does in other fields of science, and patterns of support must take account of what is required to keep the research operations of the major university departments productive. These departments have enormous undergraduate teaching obligations in addition to their responsibilities in graduate and postdoctoral education—education which affects many fields, not just mathematics. It is very easy to forget that each major department is simultaneously a teaching center and a research institute of international stature—an institute with a large faculty plus a sizeable annual influx of distinguished mathematicians from this country and abroad. The number of these major centers is large since mathematical science is concentrated almost entirely in universities. Teaching overloads and insufficient resources to sustain vital research in these centers of excellence are not exclusively university problems. This should be kept firmly in mind when thinking about federal support for the mathematical sciences.

An abundance of research scientists is required to generate the mathematical concepts and tools which permeate science and technology. Their numbers and support should not be determined by teaching demands—important as they are—but by our best estimate of how many researchers we need to guarantee the intellectual productivity from which these tools develop.

We have referred several times to the significance of the network of university departments and to their current problems. Many important mathematical scientists do not work in departments of mathematics, applied mathematics, mathematical sciences, or statistics. Often they work in operations research groups, or in science or engineering departments (for example, mathematical researchers in mechanics, control theory, or communications theory). Although the problems seem to be most severe in mathematics departments, we want to stress that the entire field is being adversely affected by funding deficiencies.

2. Breadth of Support

We base our conclusion that mathematics needs to be supported on a broad front upon these observations:

- Probably no field regularly provides as many surprises about relevance and applicability as does mathematics.
- Frequently decades of research are necessary to create the conceptual framework which allows even the possibility of a particular mathematical tool to be seen.
- Further years of research may be required to develop a tool usable by other scientists and engineers.
- For much of the long period of research, it may not appear to the outside observer that the pure and applied mathematicians are at work on anything "useful."

The utility of the mathematical sciences is best assessed by considering the contributions of the field as a whole.

Mathematical scientists ply their trade for a variety of reasons. Some want to make tools which impact directly on technology. Others want to understand the physical world and develop methods and models with which to do that. Others pursue mathematics as a discipline in its own right, choosing their areas of inquiry in terms of their potential for applicability. Still others pursue the discipline solely for its own sake, making sets of tools to apply to mathematics itself, developing concepts with which to understand what methods, models, and techniques are possible.

The best work of each type must be supported. The record since World War II shows that we can have confidence in the internal navigational system of the mathematical sciences, which comes from agreement on major problems and directions, and continuously modifies support accordingly. Andrew Gleason described the reliability of the navigational system relative to the rest of science this way:

> "Mathematics is the science of order—its object is to find, describe and understand the order that underlies apparently complex situations. The principal tools of mathematics are concepts which enable us to describe this order. Precisely because mathematicians have been searching for centuries for the most efficient concepts for describing obscure instances of order, their tools are applicable to the outside world; for the real world is the very epitome of a complex situation in which there is a great deal of order."[24]

3. Structural Budgetary Problems

The severe problems in the magnitude of extra-university support for research in the mathematical sciences have developed hand-in-hand with several problems concerning the ways in which available resources either are or are not allowed to be utilized, in keeping with federal policy and the priorities of the mathematical sciences community. Recommendations for dealing with these problems provide further general guidelines for future support:

(a) Long-term federal support for the mathematical sciences, particularly support by the National Science Foundation, must restore a balance between support of summer research time and support

[24] Quoted in Arthur Jaffe's paper, Appendix C.

for research assistants, postdoctorals, research associates (visiting scholars), staff support, computer time, travel, and related year-round expenses.

(b) The number of established investigators who currently have any support at all is too small relative to the strength, excellence, and size of the field.

(c) Federal support for fundamental pure and applied research is too heavily concentrated at NSF. This presents two risks: (i) that mathematics will lose the stimulation provided by technological challenges facing mission-oriented agencies and that the agencies will experience diminished creative work on their problems; (ii) that inadvertently the Foundation will come to control policies which should be made by or with the research community.

(d) Support from the second major source, the Department of Defense, is vital to applied mathematics and statistics. A change in DOD policy 15-odd years ago contributed to the extreme concentration of pure mathematics support in NSF. Current DOD policies, if continued, will further shift the emphasis toward immediate applicability, so that more of fundamental applied mathematics and statistics is "transferred" to NSF, exacerbating the first three problems.

(e) Support from the third major source, the Department of Energy, is of increasing importance at the interface between mathematics and scientific computation. Resources going to the mathematical side of the interface should be increased.

Conclusion (a) was implicit in our earlier discussion of how support inadequacies developed. Here we amplify our remarks about it to take into account what has been happening in the last few years. It must be read together with conclusion (b). Under severe restrictions on the level of funding, support for almost everything except summer research time disappeared from NSF grants, *and* the number of established investigators who had grants was severely constricted. Thus, although the structural imbalance in conclusion (a) is a problem, its solution can be accomplished without serious harm to the research effort only if total resources are significantly increased at the same time.

Both parts of conclusion (c) can be made more specific. We feel that the spectrum of applied mathematics currently supported by NSF through its Mathematical Sciences Division is too narrow, in that it misses much of the interface of mathematics with technology. At the same time, if "purer" mathematicians do not interact with technical problems in mission-oriented agencies, then both mathematics and the agencies lose an important stimulus. Dominance of mathematical funding by NSF can also leave the field highly vulnerable because (i) most fields of science have other significant funding sources, and (ii) the natural tendency within a funding organization is to maintain equity among the fields it supports. This vulnerability concerns us over the long run. It should not be interpreted as a criticism of current events at the National Science Foundation. Indeed, great care is currently being exercised in its Mathematical Sciences Division to get meaningful advice from the research community, and a substantial Administration/research community effort is under way to correct some of the NSF budgetary problems we have described—problems of magnitude as well as of structure.[25] This effort must continue for several more years, so that improvements will not be short-lived.

In conclusion (d) concerning DOD support, our immediate message is clear: if DOD research concentrates even more on immediate applicability or direct mission relevance, fundamental mathematical sciences research will have trouble getting support. DOD will also have problems over the long run: policy decisions which narrow the scope of what DOD supports damage the health of the mathematical sciences and weaken their ability to contribute to the nation's defense effort. A major difference between what is happening now and the events of the late 1960s is that the shift in emphasis and the flatness of overall funding are occurring inadvertently, rather than as a result of deliberate policy decisions related to the mathematical sciences:

- The growth of funding for computer science masked the fact that support for the mathematical sciences was weakening at DOD.
- A new program of "initiatives" or "thrusts" has taken resources away from the "core" programs, those which support fundamental mathematical sciences research.

[25] See detailed discussions of NSF support in Appendix B.

The mathematical research community has been vigorously debating all of these structural issues for the last few years. Discussions between members of the community and officials of federal agencies go on regularly at the NSF Advisory Committee for the Mathematical Sciences. Others have grown out of the activities of the National Research Council's Mathematics Briefing Panel, its report presented to the Office of Science and Technology Policy, and the addendum prepared for the Department of Defense.[26] A constructive dialogue with representatives of the Department of Defense has begun under the auspices of the DOD-University Forum, to discuss a range of issues about DOD support of mathematical research. It is an encouraging step.

Several of these structural issues, including the increasing role of the Department of Energy, are discussed in greater detail in Appendix B.

4. Industrial Support

Industry does not support academic research in the mathematical sciences. This is not likely to change significantly in the near future. Yet we feel it is important for the mathematical sciences research community and the universities to increase efforts to promote industrial interaction and perhaps attract some support, at least enough to fund the interaction.

Industry awareness of the significance of mathematics for technology seems to be increasing. About one-fourth of the Ph.D.'s in the mathematical sciences currently move into industrial careers. The broadly-trained mathematician, even at the pre-Ph.D. level, is highly employable. Some mathematical research groups in industry are proliferating, and the attachment of mathematicians to other groups is growing. As mathematics penetrates into production control and manufacturing through automation, demand for mathematicians will increase; this will place new responsibilities on those who train mathematicians.

The broader academic community in mathematics has done too little historically to promote contact with the users of mathematics. This is changing, as mathematics looks outward. The new NSF-sponsored research institutes at Berkeley and Minneapolis show a substantial interest

[26] The Briefing Panel was established by the NAS's Committee on Science, Engineering, and Public Policy. Its report and the DOD addendum are Attachments 1 and 2 to this report.

in promoting mathematics and science interactions. A unique institute is being started at the University of Chicago to promote such interaction. This effort is all the more notable because it will seek base support from outside the government.

Certain universities—those with strong engineering roles—must reach out through mathematics to engineering and industry. Small, department-affiliated research institutes could bridge the mathematics-industry gap through seminars for department faculty and mathematical engineers or through leaves, to bring industrial mathematicians into the department. The institutes would benefit both education and research. Since industry would profit from both avenues, it might lend them financial support.

C. GUIDELINES FOR RENEWAL

Talented young people are essential for renewing mathematical research. Every effort must be made to maintain the flow of outstanding young people into the field and to see that they receive strong support and excellent training.

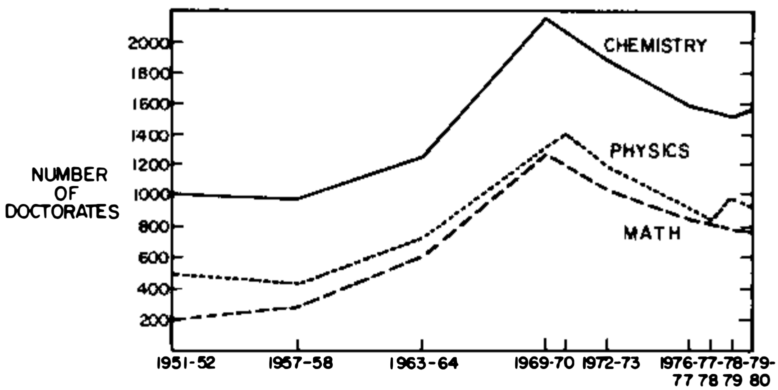
There are quantitative and qualitative questions:

- Are enough highly-talented young people being attracted into Ph.D. programs?
- Are the mathematical sciences turning out enough high-quality Ph.D.'s to replace the most productive present researchers?
- How can the best possible predoctoral and postdoctoral education be provided?
- What level of support is needed for graduate and postdoctoral students, and young investigators?

1. Ph.D. Production

About 200 mathematical scientists annually completed Ph.D.'s in 1950. That figure grew steadily through the fifties to peak at just under 1,300 in 1969-70, thereafter falling off to approximately 800 by the

FIGURE 5
EARNED DOCTORATES IN PHYSICS, CHEMISTRY, AND THE MATHEMATICAL SCIENCES, 1951-52 THROUGH 1979-80



Source: National Science Foundation

late seventies. Figure 5 documents pattern similarities for mathematics, physics, and chemistry.

Table 2 shows numbers of mathematical sciences doctorates from U.S. universities since the peak production year of 1969–70. For the last decade, percentages of Ph.D.'s granted to U.S. citizens are included. The annual number of Ph.D.'s has leveled off at about 800. The percentage who are U.S. citizens has dropped from 78% to 61% in the last decade. *Over the historical period* 1968–82, *examined at some length in this report, the annual number of U.S. citizens obtaining doctorates in the mathematical sciences from U.S. institutions has been cut in half, from over* 1,000 *to fewer than* 500.

2. Employment Prospects

In the late 1960s and early 1970s, Ph.D.'s glutted the field. Most of the young people who entered doctoral programs in the late 1960s were aiming at positions in colleges and universities. These institutions have traditionally employed most new Ph.D.'s in the mathematical sciences and virtually all of those interested in careers in basic research. The academic marketplace in mathematics became oversaturated and stayed that way for a number of years.

TABLE 2. Doctorates in the Mathematical Sciences in U.S. Universities 1971-1983

Year	Total Ph.D.'s	% U.S. Citizens
1970-71	1,217	--
1971-72	1,192	--
1972-73	1,042	78%
1973-74	972	72%
1974-75	992	74%
1975-76	874	75%
1976-77	827	76%
1977-78	809	73%
1978-79	751	74%
1979-80	765	73%
1980-81	799	68%
1981-82	779	65%
1982-83	796	61%

Source: Committee on Employment and Educational Policy, American Mathematical Society (AMS/CEEP). Until a few years ago, some computer science Ph.D.'s were included in the AMS data. These have been excluded from Table 2.

The effects on many young mathematicians were serious. Fewer industrial opportunities meant new careers had to be forged, careers which often made little use of doctoral training. The Ph.D.'s who did find academic employment frequently located in departments considerably farther down the list of national rankings than they had anticipated.

Today the employment situation in the mathematical sciences is brighter. Virtually all of the Ph.D.'s in 1982–83 are working in areas related to their training. About 22% work in other countries. Of those employed in the United States, 48% teach or do research in doctorate-granting departments; 28% are in masters/bachelors-granting departments, and 24% in industry or government.

Additional retirements in the early 1990s should create greater demand for science faculty. The analyses in the 1979 NRC report *Research Excellence Through the Year 2000* projected gradually increasing death/retirement rates for total mathematics faculty between 1979 and 1984 (and a significant increase in the 1990s), but predicted little increased demand for mathematics faculty. The predictions have been wrong thus far and will probably continue to miss the mark in the years ahead. The principal reasons are stated in the report, in its description of the assumptions behind the major studies the report relied on:

> "They assume that enrollments in four-year colleges and universities depend mainly on the number of people in the college ages, that science and engineering enrollments will move approximately as total enrollments do, and that enrollment levels are the main determinant of faculty size. They do not take account of changes in R&D funding as a possible source of variation in faculty size."

The report went on to say:

> "They conclude that the enrollment squeeze coupled with the low retirement rates of the 1980s will cause the annual academic demand for new science and engineering Ph.D.'s at all colleges and universities to drop by nearly 50% between 1978 and 1985, with a further drop in the 1990s."

Table 3 shows the rapid growth of mathematics and statistics enrollments in four-year institutions over the last eight years. We can attribute only part of the growth to elementary computer science courses taught by mathematics faculties. Enrollment in such courses was about 300,000 in 1983.

Table 4 profiles the collegiate-level mathematics teaching community.

Overall demand for Ph.D.'s exceeds supply. The Committee on Employment and Educational Policy of the American Mathematical Society (AMS/CEEP) annually surveys the nation's four-year colleges and universities to determine faculty hiring in mathematical sciences departments. Where nondoctorates are hired, institutions are asked to indicate whether they would have preferred a person with a doctorate. Table 5 shows the results for the last three years.

TABLE 3. Enrollment in Mathematics and Statistics Courses in Universities and Four-Year Colleges--Fall Semester

1960	1965	1970	1975	1979	1983
744,000	1,068,000	1,386,000	1,497,000	1,999,000	2,390,000

Source: Conference Board of the Mathematical Sciences; AMS/CEEP

TABLE 4. Mathematical Sciences Faculty at U.S. Universities and Four-Year Colleges--Fall 1983

With Doctorate	Without Doctorate	Total
14,100	4,400	18,500

Source: AMS/CEEP

TABLE 5. Hiring of Non-Doctorate-Holding Faculty in the Mathematical Sciences--U.S. Universities and Four-Year Colleges

	1980-81	1981-82	1982-83
Full-time faculty positions filled by non-doctorates	700	880	724
Number of such positions where doctorate preferred	350	536	401

Source: AMS/CEEP

TABLE 6. Faculty Hiring in Doctorate-Granting Mathematical Sciences Departments, Fall 1983

Total Faculty	Positions Filled	Percent Filled With New Ph.D.'s
5,600	375	40%

Source: AMS/CEEP

Most of the hires in Table 5 occur at nondoctorate-granting mathematical science departments. There is a shortage of doctorates to fill positions at such institutions.

At the doctorate-granting departments, faculty totals and hiring rates stabilized a decade ago at the levels indicated in Table 6.

Both academic and nonacademic employment for Ph.D.'s may be affected by rapid growth in the mathematics of computation. In Section IV-E we propose an initiative in this area, principally to attract and support young people, and we note that demand for new Ph.D.'s in the subfield could reach the level of 100 per year in the near future.

Let us summarize. There is an excess of demand for Ph.D.'s over supply, created by increasing undergraduate enrollments; Ph.D. production and hiring rates at doctorate-granting departments have been stable for several years; the percentage of U.S. citizens among new Ph.D.'s is decreasing; increased retirement rates in the 1990s will create somewhat greater demand for faculty at doctorate-granting departments; overall demand for Ph.D.'s could increase sharply because of growth in the mathematics of computation.[27]

We conclude that the current Ph.D. production level of 800 per year is unlikely to be adequate to meet demand over the next decade.

3. Prospects for Renewal

Renewal presents problems. Out of 9,000 mathematical scientists in academia identifying research as their primary or secondary activity, 5,500 publish regularly, 4,000 frequently. In the next section, we estimate that 2,600 established mathematical scientists are highly productive.[28]

What is required to renew this last group on an ongoing basis? If the average span of highly productive years is 20–25, renewal requires that 105–130 mathematicians of high research ability be produced annually. Annual Ph.D. production is 800, of whom 22% accept foreign employment. One-fourth of the remainder go into government or industry, with a somewhat lower probability of ending up in basic research. Even discounting that, only 625 remain in the pool from which 105–130 strong mathematical scientists must emerge. Thus one out of every five Ph.D.'s must develop these strengths, a high success ratio (17–21%) for mathematics, computed on a national basis. Regeneration will be difficult.

We can see from this brief discussion that efforts must be stepped up to attract outstanding young people into the mathematical sciences and to nurture them as they move into the field.

Since no significant increase in numbers of talented doctoral students is likely to occur in the immediate future, one of the most pressing needs of the mathematical research community is to increase its support of young people. Those who are working in the mathematical sciences will need to be nurtured in three important ways:

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[27] See section IV-E.
[28] This is the size of the group of established mathematical scientists whom we feel should be federally-supported.

- There must be much wider availability of graduate student support other than teaching assistantships, so that a period of intense concentration on research for dissertations is possible.
- There must be much wider availability of postdoctoral positions at major centers, so that recent Ph.D.'s of high promise deepen their commitment to research and develop the perspective and skills necessary for doing research at a high level.
- There must be an adequate number of research grants for young investigators (Ph.D. age three to five years) after the postdoctoral period (usually a period of two years).

A sizable increase in federal support is required to achieve these objectives. The research community must understand the problem of renewal and the importance of addressing it.

Efforts to attract brilliant young people into the mathematical sciences must move ahead simultaneously. There are several considerations.

Funding for the field can redirect interests over time to attract promising undergraduate and graduate students. If there are insufficient resources to support the field—and we have in mind both university and extra-university resources—the attractiveness of the field to young people is diminished.

The mathematical sciences share problems with many other sciences. One is general salary levels. Pressure from the industrial sector is great; large starting salaries for college graduates in areas such as computing help lead people away from graduate schools and science and into industry. But there are special problems within mathematical science itself.

The imbalance between extra-university funds for mathematical and other sciences suggests that the field is somehow less attractive to gifted young scientists. About the time they enter graduate school, our best and brightest future scientists choose from several specialties. This is the stage at which a young person "interested in mathematics" might easily shift away into another theoretical science, perhaps influenced by his/her perception of the circumstances of graduate students/faculty in various fields.

It will take more than money to attract additional creative young people into mathematics. The universities (the academic mathematicians) must convince students not only of the excitement and relevance of mathematics, but also of the career opportunities which exist. And mathematicians must reflect on their curricula to see if they strike good balances between student interests and the needs of mathematical sciences research. More importantly, professional organizations in the mathematical sciences should buttress universities' efforts through national information campaigns. To take but one example: How well do high school guidance counselors or the public understand that the coming of the computer has greatly increased the demands for mathematical training and research, not lessened them?

4. A Plan for Renewal

We are recommending rapid development and implementation of a National Graduate and Postdoctoral Education Plan in the mathematical sciences, in response to the pressing need for renewal. It would have these features:

- Each of the approximately 1,000 graduate students per year who reaches the level of active research for a Ph.D. thesis would be provided with 15 months of uninterrupted research time, preceded by two 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 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.[29]
- These levels of total support for graduate students, postdoctorals, and young and established investigators would be attained

[29] The number 2,600 is obtained from an analysis in the following section on sustaining research output.

by ramping-up federal funds for mathematical research over five years, at the rate of 18% per year.[30]

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.[31] It is based on the guidelines for renewal which we presented and an approximate annual flow into the system as follows:

1,000 thesis students	→	800 Ph.D.'s	→	200 post-doctorals	→	133 young investigators	→	100 senior investigators

Implementation does not require major modifications of the way funds are dispersed. Most would go through research grants to "senior" investigators. Where appropriate, bloc grants (departmental grants) for graduate student or postdoctoral support could be made.

But implementation does call for modifying expectations and utilization. Universities which currently support virtually all mathematical Ph.D. students through teaching assistantships would need other staff to assume the teaching responsibilities of students who moved into pure research activities for a year. There is a simple way to do some of this at major centers: associate small amounts of teaching with some postdoctoral positions, a long tradition in mathematics. Further coverage of the teaching could come from visiting faculty, for which more support should also be provided. Funding agencies, mathematical science faculties, and university administrations—understanding the overall plan—can adjust. The additional resources should be injected over several years to allow for structural transition.

Another important adaptation for the universities and the mathematical scientists would be to strongly encourage new Ph.D.'s to move into postdoctoral positions as they become available, rather than accepting tenure-track positions immediately after the Ph.D. This may be difficult, simply because it is a change in the recent style of movement through the ranks of the profession,[32] but it can be done if the research

30 See detailed estimates in section IV-F.
31 See, for example, Attachments 1 and 2 to this report.
32 Also because the residual effects of the previously tight academic market tend to push young people into tenure-track positions early.

community understands the need for it and pushes the idea with the universities and the young mathematical scientists.

This major effort can succeed only if everyone involved thinks nationally instead of locally.

D. GUIDELINES FOR SUSTAINING RESEARCH OUTPUT

Underinvestment in mathematical sciences research over the last decade has severely restricted the number of productive investigators who are supported. Figure 6 shows graphically that the number of established mathematical scientists with federal support is out of balance with the numbers for other sciences. In section III-E, we discussed the negative impact this is having on university centers and will have on research output if it continues. Research grants in the field have dropped from 2,100 to 1,800 in the last few years and are still declining.

1. A Basic Estimate

The number of federally-supported (principal) investigators must be reset at a level adequate to sustain research and provide appropriate graduate and postdoctoral education. We estimate 2,600 as the threshold level for the number of established investigators to be federally supported, as follows.

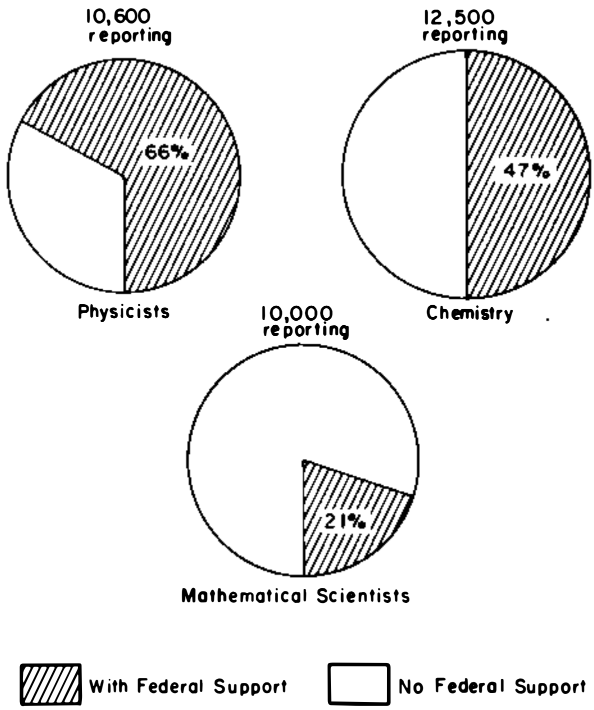
Three review systems operate to monitor research productivity and quality on a national basis:

- professional journals
- peer or panel review of grant proposals
- hiring and promotion practices of universities.

Journals (publication rates) can be used as measures of research activity on a broad-brush basis, but do not have consistent standards within fields, let alone between them. The review processes of federal agencies monitor quality well, but will not help us here. Mathematical sciences research has a demonstrably low level of funding; hence, numbers of people currently supported provide only a lower bound for the estimate we need. Proposal pressure is not a good indicator; after a

Figure 6

Support Status of Doctoral Scientists in Educational Institutions, by Field — 1981



Numbers reporting as fractions of totals in educational institutions: Chemistry 85%; Physics 82%; Math Sciences 81%

Source: Characteristics of Doctoral Scientists and Engineers in the United States, 1981 - National Science Foundation 82-332

long period of underfunding, the proposal review process stabilizes; only those with good prospects of being funded continue to apply.

The standards of universities are not uniform either; however, in most there is intra-university consistency of standards across related fields. Table 7 shows 1980 faculty sizes and the percentages of those with federal suport at 156 doctorate-granting universities in engineering plus the physical, mathematical, and computer sciences. The "mathematics" faculty numbers would need to be scaled up by a factor of 1.3 to get ap-

TABLE 7. Full-Time Faculty and Federal Support Status in Surveyed Departments at 156 Doctorate-Granting Institutions Spring 1980

Field	Total Faculty	Percent Federally Supported
Engineering		
Chemical	1,039	69
Civil	1,886	57
Electrical	2,313	55
Mechanical	1,998	54
Physical Sciences		
Chemistry	3,380	63
Geology	1,394	70
Physics	3,580	67
Mathematical and Computer Sciences		
Computer Science	840	57
Mathematics	4,485	39

Source: Young and Senior Science and Engineering Faculty 1980, NSF 81-319

proximate counts for the broader field of the mathematical sciences, i.e., to include mathematical scientists not in mathematics departments.[33] Note particularly the general consistency of the percentages for most fields other than mathematics, and the much lower percentage for mathematics. One reason for the larger size of mathematics faculty, as noted earlier, is that research in the field is concentrated almost entirely in universities.

Research quality and performance are monitored closely by universities, especially in tenure reviews. The performance level of faculty in the mathematical sciences is assumed to be comparable to that in related fields. It is implausible, then, that the significant discrepancy in percentages of faculty with federal support reflects assessments of quality of research. Nor is it plausible that this discrepancy is based on a lower level of research activity by mathematicians. In fact, the number of mathematical scientists actively involved in research is large.[34]

[33] At the 50 universities with the highest ranked departments of chemistry, mathematics, and statistics, mathematics accounts for about two-thirds of the mathematical sciences faculty. It constitutes three-quarters of mathematical sciences for the broader set of institutions in Table 7.

[34] The literature search used by the American Mathematical Society to prepare

Apply to the mathematics faculty the lowest percentage for those with federal support in other fields, 54%. One obtains 2,400 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 1.3 x 2,400 = 3,100 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 2,700 as an appropriate number of established investigators.

There is one other "system" which operates to monitor research productivity and quality in a field: the judgment of the research community itself. We knew concern to be widespread and deep in the mathematical research community because cut-off levels for research grants had moved so far up into the "excellent" category that the best mathematicians could not discern the difference in quality between those who did and those who did not receive support.[35] Professor 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 2,600–2,900 for the total of "supported" plus "equally qualified."[36]

The range 2,600–2,900 brackets the 2,700 estimate. We adopt 2,600 as the threshold level for the number of established investigators to support.

2. A Crosscheck on Balance

Tables 8 and 9 give comparative academic research support data in chemistry, physics, and the mathematical sciences.

It is not the precise numbers in these tables which interest us. It is the evident imbalance of scale. The markedly lower percentage of

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the list of U.S. entries for the *World Directory of Mathematicians* shows that 4,000 mathematical scientists publish at least three papers per five years. Numbers of papers per year are much smaller in mathematics than in most sciences. Mathematical scientists of high quality will, with rare exceptions, publish at least three papers every five years.

[35] This sense was conveyed in the letters from department chairmen in section III-E-1.

[36] Although this was an informal survey and had a subjective element to it, examination of the raw data from departments with which our mathematician members were familiar showed those chairmen had been conservative and had used high standards.

TABLE 8. Doctoral Scientists Employed in Educational Institutions by Field and Support Status--1979

Field	Total Doctoral Scientists	Those with Primary or Secondary Work in R&D	Faculty with Federal Support	Nonfaculty with Federal Support	% in R&D with Federal Support
Chemistry	14,900	9,800	3,300	1,800	50%
Physics/ Astronomy	12,100	9,200	3,300	3,100	70%
Mathematical Sciences	15,000	9,100	2,300	insufficient cases	25%

Source: Survey of Doctoral Recipients, National Research Council

TABLE 9. Faculty in R&D by Field and Support Status--1979

Field	Senior Faculty in R&D (Full and Assoc. Profs.)	Senior Faculty with Federal Support	Junior Faculty in R&D (Asst. Profs. & Instructors)	Junior Faculty with Federal Support	Total Faculty with Federal Support
Chemistry	6,000	2,800	1,600	500	3,300
Physics/ Astronomy	4,900	2,700	1,100	600	3,300
Mathematical Sciences	5,800	1,500	2,600	800	2,300

Source: Survey of Doctoral Recipients, National Research Council

mathematical sciences faculty supported is seen in Table 8. The general absence of postdoctorals in the mathematical sciences is reflected in Table 9 by the larger size of the junior faculty group—most academic researchers of postdoctoral age in the field were in beginning assistant professorships or research instructorships in FY 1979. We have recommended that 400 postdoctoral positions (200 two-year positions each year) and 400 young investigators (Ph.D. age three to five years) be supported. If we raise the number of established researchers supported to the level 2,600, the total number of researchers supported will be 3,400, or 38% of doctoral mathematical scientists in R&D. This compares with the Table 8 figures of 50% in chemistry and 70% in physics/astronomy.

3. Related Comments

The sort of estimation given here would be difficult in any field. Start from scratch and determine how many biologists, chemists, physicists, or whomever the federal grants system "ought" to support. For the mathematical sciences, we tried a number of estimation methods: careful scrutiny of the groups of mathematicians with different publication rates; comparison of faculty sizes in "distinguished or strong" departments in the Roose-Anderson survey, etc. Our colleagues in different fields raised questions about each. Frequently, their queries were not directed at the method used but at the conclusion, or at the underlying question itself. We speak to three of these queries which came up often:

Why do mathematical scientists really need (federal) research support? Answer: For the same reasons that any theoretical scientist does. The needs are described specifically in the next section, IV-D-4.

How have the mathematical sciences been doing so well for the last 15 years with so little support? Answer: If the number of first-rate minds in a theoretical field is large at the onset of a funding squeeze, a research effort of high quality can be sustained for a decade or more by accommodation and doing without. It cannot be sustained for a much longer time, however. This is discussed in some detail in section III-E-2.

How many research mathematicians does the country need? Answer: Enough to generate the mathematical ideas which will be needed one, five, ten, and fifty years from now. The best way to estimate that number is to balance support for the field with that for related fields.

4. Specific Guidelines

The mathematical sciences do not have enough resources to sustain their research. We have estimated that there are at least 2,600 established investigators whom it is essential to support. These are the mathematicians who will be most heavily involved with the graduate and postdoctoral training necessary for renewal, so it is doubly important that their research be supported. They will need, first of all, research time, especially in the summer. Without the support of summer research they have to seek other employment in order to keep incomes up, and are not available at their institutions to work with graduate students and postdoctorals. Each investigator also needs support staff (say, 1/4 secretary) and a sum (say, $6,000) to cover travel, publication costs, duplication, etc. Computer time/equipment is important for many investigators—crucial in various applied mathematical areas, and in statistics. This need is increasing rapidly. Mathematical scientists also need research associates, visiting scholars from around the world who come to centers to spend substantial time in direct research involvement. This need is not uniform, nor constant, but it is very important in mathematics. We take it to be about one person per year for every 20 investigators. Then there are "communication" needs: publication, travel, summer schools, conferences, mini-institutes, and the larger research institute costs beyond what we have described. Support for faculty leaves is important, and there is a need for resources to allow mathematicians from "outlying" institutions to spend time at major centers.[37]

E. GUIDELINES FOR AN INITIATIVE IN THE MATHEMATICS OF COMPUTATION

Large-scale advanced computers create unusual opportunities in many disciplines. These opportunities are essentially mathematical, although the applications are to other fields of science, such as the atmospheric sciences, physics, computational chemistry, VLSI and circuit design, fluid and solid mechanics, material sciences, astrophysics, the social sciences, and biophysics. In these fields, sophisticated mathematical models are used to simulate complex phenomena. Computational science activity is most important at the interface between mathematical and theoretical science, on the one hand, and experimental science on the other.

Large-scale computers will require new mathematical methods and algorithms for their appropriate exploitation. Moreover, a large cadre of sophisticated mathematical and computational scientists is needed for the proper utilization of these powerful tools. The more sophisticated the computational equipment, the larger the requirements for mathematical and algorithmic methods.

[37] When we come to dollar estimates, we include these under "research associates" and "travel to major centers."

Several studies conducted during the past year have documented the needs and opportunities in the area of scientific computing. Notable among these are the December 1982 *Report of the Panel on Large-scale Computing in Science and Engineering* (Lax Panel), sponsored by the National Science Foundation and the Department of Defense in cooperation with the Department of Energy and the National Aeronautics and Space Administration, and the August 1983 *Report of the FCC-SET Supercomputer Panel.* These reports detail needs for computing resources of all types: local computational facilities, Class VI computers, and networks. Both reports point out that there is a severe shortage of appropriately trained personnel for academic, industrial, and defense needs, and that the base of academic research in this area (computational mathematics, algorithms, software science, and architecture) is insufficient to take advantage of the scientific possibilities made available by the existence of modern Class V and Class VI computers.

In its survey of resources available for research in the mathematical sciences, the Committee has been impressed with the Department of Energy's Applied Mathematics program, as it relates to scientific computing. However, unless this program is significantly expanded, and similar programs properly funded at NSF and DOD, an important research opportunity with vital consequences for science, technology, and defense will not have been capitalized upon. Particularly worrisome are the scarcity of senior personnel in this area and the extremely small number of young researchers and graduate students. The Committee endorses those recommendations of the Lax Panel report which bear directly on the mathematical sciences (which are, with the computer sciences, the central basic research community involved).

A major effort in this area is needed to attract, educate, and support graduate students, postdoctorals, and young researchers, and to provide the computational equipment essential for the proper conduct of this research.

We estimate that an annual investment of approximately $15 million for computational equipment, for its maintenance and support, and for appropriate access to similar equipment, is required for mathematical scientists in scientific computing. Other support of basic research in the mathematics of computation, with particular emphasis on the support of graduate students and young researchers, will be included in our general estimates for the field.

Significant additional resources for the mathematics of computation may be needed in the years ahead. Expectations are that a few hundred supercomputers for academic, industrial, or governmental use will be put in place over the next decade. Each machine will require approximately 10 scientists with sophisticated knowledge of applied mathematics related to computation. Demand for such new scientists may run 500–800 per year. Even though numbers of these scientists will come from computer science, the physical sciences, or engineering, the demand for new Ph.D. mathematical scientists in computing could easily reach 100 per day in the near future. Federal support of a subfield of this size could not be absorbed within the resources we have recommended.

The initiative we have proposed is just that, a first step. The resource needs for the mathematics of computation must be reviewed very carefully each year, in light of the subfield's development in relation to the mathematical sciences as a whole.

F. ESTIMATES OF FUTURE SUPPORT NEEDS

Since the early phases of our Committee's work, we have recognized that the funding situation in the mathematical sciences is so badly out of order that incremental budget thinking could not properly address the question of needs. The support level must be reset at a magnitude appropriate to the size, style, quality, and potential of the field, one commensurate with support for the general scientific-technological effort of the country. The gnidelines we have developed tell us how to get a good estimate of the appropriate levels. Table 10 contains the numbers, which total $180 million per year.[38]

Since FY 1984 federal funding for the mathematical sciences totals about $78 million per year, the recommended level seems high. It is not. It is a conservative estimate of what is required to put support back in balance and provide for the future.

For the wealth of tools the mathematical scientists provide, an investment of $180 million per year seems modest.

[38] FY 1984 level.

TABLE 10. Estimated Extra-university Support Needs of the Mathematical Sciences
(Where Applicable, Benefits and Indirect Costs Included)

I.	Grants for established investigators (excluding graduate students, postdoctorals, research associates)		
	Two months research time	$20,000	
	Support staff (1/4 sec'y)	4,000	
	Travel, computer time, publication costs, duplication, etc.	7,500	
		$ 31,500 x 2,600	= $81.9M
II.	Grants for young investigators (Ph.D. age 3-5 years)		
		$25,000 x 400	= 10.0M
III.	Postdoctorals		
	24 months	$90,000 x 200	= 18.0M
IV.	Graduate students		
	18 months--stipend plus tuition		
		$30,000 x 1,000	= 30.0M
V.	Research Associates (visiting scholars, senior)		
		$90,000 x 130	= 11.7M
VI.	Summer schools, conferences, mini-institutes, travel to major centers, plus research institute costs, excluding postdoctorals		
			11.0M
VII.	Mathematics of Computation initiative		15.0M
VIII.	Other computer equipment		2.5M
		TOTAL	$180.1M

V. RECOMMENDATIONS

We end with our recommendations to various groups about what they should do to provide for the future of mathematical research.

A. TO THE ADMINISTRATION AND CONGRESS

The level of extra-university support for the mathematical sciences is dramatically low. The field is not renewing itself. With its present resources it cannot sustain its output, much less capitalize on the significant opportunities which exist.

The mathematical sciences play a major role in technology, and therefore in defense and the economy. Prospects for industrial support are slim, because much of the research has long-term payoffs. Therefore, the federal role is crucial.

We estimate that it will take an additional $100 million per year in resources to set things back on course and provide adequately for the future. If phased in over a period of five years, it will allow time for needed utilization adjustments in universities and the research community.

The groundwork for a joint government/university/research community effort has been laid by the successful self-studies which mathematical scientists have done over the last few years to evaluate and describe the significance and potential of their field, articulate needs, and set basic priorities.[39]

We have recommended a National Plan for Graduate and Postdoctoral Education as the framework for renewal in the field and for sustaining the research effort. Close cooperation will be especially important in implementing this plan. The research community, at considerable cost to the support of established investigators, has increased support for young mathematicians, even within existing resource limitations. Added resources and university-government cooperation will be essential if the effort is to be continued.

Federal support for basic research in the mathematical sciences is concentrated (62%) in the National Science Foundation and the three service agencies, (31%) in the Department of Defense (AFOSR, ARO, ONR). The support at NSF covers the spectrum of the mathematical sciences and includes 97% of the support of "pure" mathematics. That at DOD is concentrated in applied mathematics and statistics and constitutes nearly two-thirds of the federal support for those subfields. Prospects for increasing support significantly at other mission agencies are slim, except at the interface of mathematics and computation, where the role of the Department of Energy is of increasing importance. Thus,

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39 Report of the Research Briefing Panel on Mathematics (COSEPUP/NAS); the DOD Addendum to its Report; Report by the Committee on the Applications of Mathematics (NRC); Report of the Panel on Large-Scale Computing in Science and Engineering (NSF/DOD); regular reports of the Advisory Committee to the (now) Division of Mathematical Sciences at NSF; Report on Computers and the Future of Statistics, Committee on Applied and Theoretical Statistics (NRC); Statistics: Change and Resources in a Growing Science, report to NSF Mathematical Sciences Advisory Committee by David S. Moore and Ingram Olkin; Operations Sciences at NSF; Status and Opportunities, Proceedings of Workshop on Research Directions in Operations Science, by George Nemhauser and George Dantzig.

any move to increase support significantly must be primarily a two-pronged effort by NSF and DOD.

Strong action has begun at NSF to increase support for the field, especially for young mathematical scientists. This effort must be continued, with large increases in the year-to-year budgeting. A similar effort must be initiated at AFOSR, ARO, and ONR. The mathematical sciences should become a target program in these agencies. What is required is an average 18% real growth per year, for each of the next five years.

Congressional support for the NSF initiative and DOD funding of basic research will be quite important.

B. TO UNIVERSITIES

As the dominant supporters of mathematical sciences research and the nurturers of mathematical education, universities have a special interest in the state of federal research funding in the field. They also have responsibility for improving the current situation. The low level of research funding in the mathematical sciences, as contrasted with that of other fields of science and engineering, causes a number of serious intra-university problems.

Less direct outside support of research time is brought in by mathematical scientists, especially those in so-called "pure" mathematics. Less outside support is provided for secretarial help, for graduate student support, for travel, for supplies, for almost anything connected with research. This throws cost burdens back on the university. Tensions are created, as most deans can testify, because other scientists pointedly note that the institution is paying for a number of items in "mathematics" which investigators in other fields are expected to pay for from their own grants. Deans also feel pressure from mathematicians, who have to teach more, cannot give their graduate students time to think, have inadequate support staff, and no operating expense money. Images are created which suggest that the mathematical scientists may rank lower in their fields than their counterparts in other science departments because the percentage of mathematicians with outside grants is significantly lower.

Why have the universities remained silent in general discussions of federal mathematics funding? Here are some of the reasons.

- Mathematical research is cerebral. Its needs seem intangible when compared with those of other sciences.
- Mathematical science department budgets are justified to trustees or regents solely on the basis of teaching demands, as is graduate student support.
- The mathematical sciences community has not described its federal support problems well enough to make clear that they are nationwide.

The universities can help remedy the funding situation by:

a) Calling to the attention of federal agencies and policymakers the fact that something has gone seriously wrong with mathematical sciences research support. It is evident in the internal dynamics of almost every major American university. This situation must be pointed out.

b) Reviewing the substantial problems in the working circumstances of their mathematical science faculties and morale in the associated departments. University administration and faculty must identify these strains and work together to alleviate them. Such problems negatively affect both mathematical sciences research and mathematical education. University/federal agency discussions and understanding are essential. Increased injection of federal funding into mathematical sciences research will do scant good if followed by university cuts in other areas of support.

c) Using their mathematical faculties to attract industrial support for academic research in the mathematical sciences and to promote interaction between mathematics and its users.

C. TO THE MATHEMATICAL SCIENCES RESEARCH COMMUNITY

This group knows it bears primary responsibility for the future health of mathematical research. Both the self studies of the last few years and recent unified efforts towards improving federal funding demonstrate the community's commitment to present and future research and education.

We want to recommend some agenda items for that future. Each has a time scale of 10 or more years. They are not new, but they are pressing.

- The community, in part through its professional organizations, must promote understanding in universities of the range of problems besetting mathematical scientists and their departments and of their relationship to the lack of research support. The research needs of the mathematicians are not well understood, nor is the fact that attempting to meet them on an adequate national scale requires university-government cooperation.

- Renewal of mathematical sciences research means increased efforts to attract brilliant young people into the field. Larger numbers of Ph.D. students need unfettered research time for theses. Greater numbers of doctorates need postdoctoral experience at major centers before moving into industrial or faculty positions.

- Many Americans do not understand how mathematics works in our culture, science, or technology. Long-term, coordinated effort by the mathematical sciences research community could help nonmathematicians achieve this basic understanding and revise their attitudes towards supporting mathematical research.

- Mathematicians and nonmathematicians principally interact through education. This provides the major interface for clarifying the role of mathematics. The research community must continually expand its involvement in precollege mathematics and science education.

- The mathematical sciences community has always seemed fragmented to the rest of the world. It has not been effective in making its needs known. Factions in all fields are a sign of vigor. But mathematical scientists must seek the common ground unique to mathematical pursuits. Mathematicians are moving that way. They should continue to revamp the consortia through which their professional societies act together for mutual benefit.

As for the role of the research community in remedying the deplorable funding situation we have described, we asked Dr. Brockway McMillan—recently retired from Bell Laboratories, a member of our Committee, and an old hand in the worlds of mathematics, government, and industry—what advice he would give to the mathematical sciences community. After recalling the general appearance of disarray mathematicians presented in national affairs some years ago, he proffered this advice:

> "Get your act together. Determine what it is that you believe mathematics is all about in our society. Define the needs and means for doing it. Then present your case in its proper context and to your whole constituency. It is in fact a good case, but it must be presented with breadth and clarity and maturity of judgment."

We believe the community is doing that now.

APPENDIX A. THE MATHEMATICAL SCIENCES RESEARCH COMMUNITY

We shall describe what we take to be the scope of the mathematical sciences and bring out several characteristics of the associated research community:

- it is a large and varied scientific community;
- it is based primarily at academic institutions;
- it is broadly spread throughout the country;
- it is deeply intertwined with the nation's efforts in mathematical education.

Comparisons with other fields will help emphasize some of these characteristics.

The discussion is divided into sections as follows:

I. Description of the Mathematical Sciences
   A. Relationship to Computer Science
   B. Agreement on Terminology
II. Size and Location
III. Professional Organizations
IV. Commitment to Education

I. DESCRIPTION OF THE MATHEMATICAL SCIENCES

A century ago, the field we are discussing probably would have been called "mathematics" and the associated research community would have been known as "the mathematicians." Growth and specialization have created major subdisciplines, defined either by subject matter or motivation and intellectual style. We found that most people in the field today would accept "pure mathematics, applied mathematics, and statistics" as a description of "the mathematical sciences" after a fair amount of explanation. This is basically the terminology we will use—after an explanation, of course.

The mathematical sciences research community includes the pure mathematicians, who concentrate on the development of the discipline of mathematics in its own right; the applied mathematicians, who develop mathematical tools, techniques, and models for the purpose of describing scientific phenomena, in physics for instance, or solving basic problems in technology; as well as specialists in numerical analysis and scientific computing.[1] The community includes a wide range of statisticians, who combine mathematical techniques with practicality to analyze and interpret data for use in inference, prediction, and decision-making. We include mathematicians from applied areas such as operations research, which grew out of logistical problems in World War II and develops and applies its optimization techniques to management and decision-making; from areas of application sometimes identified with engineering, such as communication theory and control theory; as well as from mathematical biology, mathematical economics, etc.

A few of the areas and subareas of significant activity in 1984 are:

- algebra and number theory, analysis, geometry-topology, and logic (the major subdivisions of pure mathematics);
- solid mechanics, fluid mechanics, dynamical systems, mathematical physics, astrophysics, mathematical biology, numerical analysis, scientific computation;
- probability theory, discrete optimization, combinatorial analysis, game theory, mathematical economics;
- mathematical statistics, biostatistics, applied statistics;
- operations research, control theory, cryptology;
- decision theory, reliability theory, filtering theory, allocation theory, management science.

This is by no means an all-inclusive list.

Near the boundaries of areas of application, questions inevitably arise as to where one leaves the "mathematical sciences" and passes into another field. Where, for example, is the boundary between applied mathematics and theoretical physics, or meteorology, or aeronautics? At a categorical level, it is virtually impossible to give precise answers to such questions. At the individual level, one can almost always do

[1] See the following section, in which we discuss the relationship of the mathematical sciences to computer science.

better. The pattern over time of an individual's work usually reveals whether the focus is on mathematical understanding and techniques or on a particular area of science or engineering.

This distinction is used in fields such as control theory and communications theory to distinguish between mathematical scientists and engineers among the practitioners. It is used in theoretical physics, economics, biology, and psychology to identify the small groups of individuals whose work is consistently of a highly mathematical nature—the people who truly have one foot in mathematics and one in a related science. The convention is to label these individuals "mathematical physicists," "mathematical economists," etc. What these terms are intended to describe is what someone outside the field might call very mathematical physicists, very mathematical economists, etc. This report includes such individuals among the mathematical scientists.

The distinction just described is not an adequate one in areas where affiliation exists by tradition or natural extension of the scope of a subfield, and is retained more for practical than for intellectual reasons. Statistics is an important example. There are many applied statisticians whose work is not primarily mathematical in nature, yet who are called mathematical scientists because we take the field of statistics to be part of the mathematical sciences. Statistics is an identifiable discipline in its own right as can be seen from the fact that its academic home in a major university is usually in a separate department of statistics. A strong affiliation with the mathematical sciences remains, however. This is partly because statistics has an intellectual base in mathematics, but primarily because of two related facts:

(i) its primary sources of students, especially graduate students, are in mathematics;

(ii) a significant amount of federal funding for academic research which develops fundamental statistical concepts and methods comes from the "mathematical sciences" units of federal agencies.

The more applied areas, which deal primarily with applications of statistical methods in other fields, have separate sources of funding, e.g., the National Institutes of Health and the Department of Agriculture. To avoid confusion, we do not include these areas when we discuss support for the mathematical sciences.

A. Relationship to Computer Science

Computer science has developed in the period since World War II, from roots in electrical engineering and mathematics. It became a separate discipline over a decade ago, as can be seen in academic organizations, where separate departments of computer science continue to be established even at the best universities. The field has also developed its own sources of students and its own sources of federal funding.[2]

A number of years ago, when computer science was new and developing rapidly, both academic institutions and federal funding agencies housed the directly machine-related parts of the field (e.g., computer architecture and systems) with engineering, and the more mathematical parts (e.g., complexity or algorithm theory) with mathematics. The combined theoretical units bore titles such as "mathematics and information sciences," "mathematics and computer science," or "mathematical/computer sciences." Occasionally, the terms "mathematics" or "mathematical sciences" were taken to include theoretical computer science. Residues of these practices remain. They are rare now, but care must be exercised, in reading older reports about the mathematical sciences, and in reviewing historical data, not to confuse computer science with the mathematical sciences.

A few more things need to be said about the relationship between the two fields. It is a close relationship and will remain so. Unlike other sciences, computer science has had a strong mathematical base from its beginning. Many of its founders were mathematicians or at least highly mathematical scientists or engineers. Their influence remains, as can be seen in the speed with which mathematization follows advances in the branches of computer science in which the scientific paradigms are fundamentally experimental, heuristic, or inferential. In several branches of the field, the basic problems have important mathematical components in their formulations. We see this even in newer areas such as VLSI and robotics, which pose very challenging mathematical problems.

However, only a small part of computer science could be described as intrinsically mathematical in nature. Nine major subareas are identified in the recent NRC report on *Roles of Industry and the Univer-*

[2] The major federal supporter of computer science research is the Defense Research Projects Agency (DARPA). The more mathematical parts of the field are supported by NSF, ARO, AFOSR and ONR. Both ONR and NSF have recently been reorganized to separate computer science from the mathematical sciences.

sity in Computer Research and Development (National Academy Press, 1982): systems software, integrated circuits, theoretical research, computer writing, artificial intelligence, robotics, scientific computing, data processing, and software. Only two of these—theoretical research and scientific computing—are so mathematical in nature that their relationship to the mathematical sciences is ambiguous. On intellectual grounds, they are part of the mathematical sciences and part of computer science. Even though they are both small, we must be clear about how we deal with these subfields, especially in relation to federal support.

The main associations and interests of theoretical computer scientists are with the larger computer science community. They are supported primarily by "computer science" or "computer research" sections of federal agencies. Clearly the field must be treated as part of computer science.

Scientific computing bears a different relationship to the mathematical sciences. There is a broad spectrum of scientists interested in the development and use of methods for modelling or graphically displaying aspects of scientific and engineering problems or sophisticated methods of extracting hidden information from data, as in tomography. This calls for methods of obtaining highly accurate approximations to solutions of systems of mathematical equations, especially partial differential equations. The greatest interest in the subject is in applied mathematics and the physical sciences, because parts of these fields are being revolutionized by scientific computing; currently, there is very little activity and interest within the computer science community. We believe the appropriate home for scientific computing is in the mathematical sciences, because (i) the mathematical sciences occupy the middle ground between computer science and the scientific applications; (ii) appropriate use of large-scale scientific computation involves the development of mathematical constructs and the use of sophisticated qualitative mathematical analysis; (iii) the approximation methods involved are an extension of numerical analysis, traditionally part of the mathematical sciences.

B. Agreement on Terminology

Our definition of the "mathematical sciences" corresponds closely to that used by the National Science Foundation Division of Mathematical Sciences, although it is slightly broader, because it includes areas where the Division assumes "secondary" rather than "primary" responsibility for evaluation and funding actions (mathematical physics, control theory, mathematical economics, operations research, mathematical biology, mathematical solid and fluid mechanics). It also corresponds closely to the definition used by organizations such as the American Mathematical Society in the collection of data. And it is the definition used by ICEMAP, the Interagency Committee for Extramural Mathematical Programs of the U.S. government.

II. SIZE AND LOCATION

The mathematical sciences research community in the United States has over 10,000 members. About 9,000 of them are faculty members in educational institutions and have research as their primary or secondary activity. They are part of the larger group of 14,000 doctoral mathematical scientists for whom teaching or research is the primary/secondary activity.

There are research groups located at the "nearly academic" research centers: the Institute for Advanced Study at Princeton, which has on its staff some of the greatest mathematicians in the world, and three other research institutes, the Mathematics Research Center at Madison, which long has been important to applied mathematics, and two newer ones being developed with NSF sponsorship, the Mathematical Sciences Research Institute at Berkeley, and the Institute for Mathematics and its Applications at Minneapolis. There are several unique and important research groups in industry, the most prominent at Bell Laboratories and IBM, with smaller ones in the petroleum, aerospace, and defense industries. In government, important work is being conducted in Argonne, Los Alamos, Oak Ridge, Sandia, and Lawrence Livermore National Laboratories; and at the Institute for Defense Analyses in Princeton, the National Bureau of Standards, and the National Security Agency. There are mathematicians at numbers of other organizations. The output of the research institutes and the research groups in government and industry is extremely important. Without detracting from their significant qualitative impact, the point we wish to make here is that collectively they house less than 10% of the mathematical sciences research community.

The heavy concentration of the active researchers in colleges and universities makes the academic research community in the field about the same size as the ones in chemistry and physics, although these other fields are larger if one includes their nonacademic components. For example, the total of 14,000 doctoral mathematical scientists in academia compares with 15,000 in chemistry and 12,000 in physics; the 9,000 of these mathematical scientists primarily or secondarily in R&D compares with 10,000 in chemistry and 9,000 in physics.

About three-quarters of the mathematical scientists in academia are in departments of mathematics, applied mathematics, or mathematical sciences. The remainder are in statistics departments, engineering or operations research departments (or centers), or in departments of management, psychology, etc.

The geographical distribution of mathematical scientists has two interesting features, as we heard repeatedly from mathematicians and scientists from neighboring fields:

- outstanding mathematical scientists can be found in a very large number of academic institutions around the country;
- the number of academic research centers which are of major importance to the field is also larger than in many fields of science.

The second point is reinforced by the fact that for the 50 universities with largest federal research support, the total mathematical science faculty is 1.5 what it is for physics or chemistry. Evidence and analysis support these conclusions, although the second one is difficult to quantify.

Through a literature search, we identified the 4,000 most productive mathematical scientists in the country over the five years 1977–81.[3] The academic component of this group (3,700 of them) represented some 365 academic institutions. Over 100 institutions had concentrations of 10 or more such mathematical scientists; there were 50 schools with concentrations of 20 or more.

The Grants Report of the Mathematical Sciences Division of the National Science Foundation gives another indication of the distribution of talent, because awards are made solely through use of a peer review

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3 Productivity measured by numbers of publications in standard journals. This search captures about 300 theoretical computer scientists, but misses a comparable number of applied mathematicians who publish regularly in journals in other fields.

system. In FY 1982, there were 32 universities with 10 or more grants, 58 with 5 or more grants, 80 with 3 or more grants. These represent concentrations of only the highest quality talent, because only about one-fourth of active research mathematicians are currently supported.

The geographical spread of first-rate mathematicians happens in part because the most creative people are not forced to congregate around special facilities and equipment. They have further dispersed in recent decades because such a high percentage of talent in the field went into academia, and the capacities of the traditionally powerful departments to absorb people were limited. This same phenomenon has increased the number of major concentrations of talent.

III. PROFESSIONAL ORGANIZATIONS

The diversity of the mathematical sciences research community is reflected in the range of professional organizations to which its members belong. A list of the major organizations follows:

American Mathematical Society	(AMS)
American Statistical Association	(ASA)
Association for Symbolic Logic	(ASL)
Association for Women in Mathematics	(AWM)
Institute of Mathematical Statistics	(IMS)
Mathematical Association of America	(MAA)
National Association of Mathematicians	(NAM)
Operations Research Society of America	(ORSA)
Society for Industrial and Applied Mathematics	(SIAM)
Society of Actuaries	
The Institute of Management Sciences	(TIMS)

The combined membership is about 35,000.
There is some overlap of membership with

Association for Computing Machinery	(ACM)
Institute for Electrical and Electronic Engineers	(IEEE)
IEEE Computer Society	

IEEE Control Systems Society
IEEE Information Theory Group

Most of the mathematical science organizations listed, plus the Association for Computing Machinery and the National Council of Teachers of Mathematics, cooperate on matters of common concern through the Conference Board of Mathematical Sciences. There is a Joint Policy Board for Mathematics, formed of representatives of AMS, MAA, and SIAM. The Board on Mathematical Sciences[4] of the National Research Council also plays a vital role.

IV. COMMITMENT TO EDUCATION

The quality of mathematics teaching and education at all levels is a matter of continuing concern to the mathematical sciences research community. Experience over many years has shown that quality teaching of science and mathematics at all levels—including pre-high school, high school, and college—is critical to America's continued strength in science.

Recognition of this has long been present outside of mathematics. When Dr. Vannevar Bush sent his report, "Science: the Endless Frontier" to President Truman on July 5, 1945, he spoke about the importance of quality in science and mathematics teaching, and stated:

> "Students of scientific capability are particularly vulnerable to bad or inadequate mathematical and scientific teaching in secondary school which fails to awaken their interest in science or to give them adequate instruction. Improvement in the teaching of science all along the line is imperative. To become a first-rate scientist it is necessary to get a good start early, and a good start early means good secondary school science training."

The recent report of the National Science Board Commission on precollege mathematics and science teaching reemphasized these points. The input of the mathematical scientists, through the Conference Board of Mathematical Sciences, was influential. Today, there is heightened concern in the research community about improving mathematical education at all levels.

[4] Formerly the Office of Mathematical Sciences.

APPENDIX B. FEDERAL SUPPORT: TRENDS, ANALYSES, AND DATA SOURCES

Descriptions of the basic mathematical sciences research programs at the National Science Foundation (NSF), the Department of Defense (DOD), and the Department of Energy (DOE) will include levels of support, analyses of historical trends and structural budgeting problems which developed in parallel with 1968–82 losses in support, and detailed comments on recent budgetary changes at NSF and DOD. Discussion of the importance of the Interagency Committee for Extramural Mathematics Programs (ICEMAP) for coordinated support of the field and for obtaining accurate data on funding provides a background for analysis of the critical years 1968–73.

Organization is as follows:

I. Major Supporting Agencies

II. NSF Support: Beginnings of Renewal

III. DOD Support: A Continuing Dialogue

IV. DOE Support: Mathematics of Computation

V. Current Support Levels

VI. ICEMAP

VII. Masking by Published Aggregate Data

VIII. History of the Period 1968–82

I. MAJOR SUPPORTING AGENCIES

Federal support of basic academic research in the mathematical sciences is concentrated largely in NSF and three offices of DOD: Office of Naval Research (ONR), Army Research Office (ARO), Air Force Office of Scientific Research (AFOSR). NSF supports 97% of pure mathematics. DOD accounts for 60% of support for applied mathematics and statistics. DOE plays a significant role in the support of computational mathematics. The three agencies together account for about 97.5% of the total support. The remainder comes from the National Aeronautics and Space

Administration (NASA), the National Institutes of Health (NIH), and the National Bureau of Standards (NBS).

We shall not discuss details of the programs at NASA, NIH, and NSB. These programs and budgets are summarized for FY 1982–84 in *Preliminary Analyses of R&D in the FY 1984 Budget* (American Association for the Advancement of Science).

II. NSF SUPPORT: BEGINNINGS OF RENEWAL

In the fall of 1983, the Division of Mathematical and Computer Sciences at NSF was reorganized so that its two sections became separate divisions of the Mathematical and Physical Sciences Directorate. Table B-1 shows the FY 1982–84 budgets of NSF's Mathematical Sciences Division, by program element.[1]

About $3 million of support of academic research in the mathematical sciences is located in NSF units other than the Mathematical Sciences Division. The Division of Electrical Engineering, Computer, and Systems Engineering supports work in mathematical control theory and operations research at an estimated level of $1.6 million. The

TABLE B-1. NSF--Mathematical Sciences Division Budget Authority by Program Element ($ Thousands)

Program Element	Actual FY 1982	Current FY 1983	Estimate FY 1984	% Increase FY 1984/83
Classical Analysis	$ 3,165	$ 3,320	$ 4,100	23.5
Modern Analysis	3,258	3,430	4,150	21.8
Geometric Analysis	2,927	3,120	3,850	23.4
Topology and Foundations	3,980	4,190	5,150	22.9
Algebra and Number Theory	5,048	5,330	6,600	23.8
Applied Mathematics	3,768	4,050	5,300	30.9
Statistics and Probability	3,432	3,560	4,700	32.0
Special Projects	4,911	7,706	8,325	8.0
Total	$30,489	$34,706	$42,175	21.5

[1] The substantial increase for FY 1984 was a first response to the report of the COSEPUP Mathematics Briefing Panel (Attachment 1), which outlined some of the problems with mathematical sciences support which we have described.

Division of Mechanical Engineering and Applied Mechanics supports about $600,000 of work in mechanics. In the Division of Social and Economic Science, the program in Decision and Management Science funds about $400,000 of operations research. From $300,000 to $400,000 are allotted for mathematical/statistical research in economics, and perhaps $100,000 in mathematical biology.

A. Brief Historical Review

The events of 1968–82 brought about imbalances in the utilization of resources at NSF. After the DOD reductions of the mid-to-late 1960s, the mathematical research community saw nowhere to turn except to the National Science Foundation. There were no significant sources of industrial support. No other federal agency (except DOD) would invest substantially in the mathematical sciences. The field remained fundamentally supported by two agencies (four, if one wishes to separate out the three services), but with the balance shifted toward NSF. We described some of the first response at NSF: no budgetary growth was provided, either to support young people or senior investigators dropped by DOD. Policy decisions about how to use the resources which did exist gave summer research time the highest priority. Under the pressure to pick up numbers of the outstanding people dropped by DOD, grants were thinned so that the field as a whole would not have to absorb too large an immediate reduction in numbers of established researchers supported. In the ensuing years, the field grew rapidly,[2] creating pressures similar to those caused by the DOD reductions. Quality standards (cut-off levels) for grants went up, but the priorities stayed basically the same.

Table B-2 shows what an average NSF grant in the mathematical sciences looked like in FY 1978. Senior personnel salaries were used largely (87%) to support research time in the summer. The minuscule amount for remaining direct costs speaks for itself.

Scientists in other fields would look at these numbers and ask how on earth research was getting done. Who was paying for materials, publication costs, support staff? The university, to some extent, or no one. Where did graduate student support come from? Primarily teaching assistantships, augmented by some university and private fellowships, and scarce research assistantships, plus the graduate students themselves.

[2] As did other fields. There were a lot of young scientists in the pipeline when federal fellowship support was cut back in 1971.

TABLE B-2. National Science Foundation Distribution of Funds on Average Annual Mathematics Grant, FY 1978

		Dollar Amount	
A. Salaries & Wages			
1. Senior Personnel		$9,361	
2. Nonfaculty Personnel			
a. Research Assoc., Postdocs, Nonfaculty Professionals	$366		
b. Graduate Students	868		
c. Secretary, Other	212		
Salary Subtotal		1,446	
		10,807	
B. Fringe		1,420	
Total Personnel Costs (A and B)		12,227	
C. Materials	222		
D. Travel	1,098		
E. Publication Costs	379		
F. Computer Costs	121		
G. Other	719		
Other Direct Costs		2,539	
Total Direct Costs			14,766
Indirect Costs			6,491
Total Costs			$21,257

Source: National Science Foundation, MS 80-857, 3-7-80

The grant pattern at the other major supporter, DOD, did not follow the course we just described. Service agency grants in applied mathematics and statistics have continued to support more reasonable fractions of graduate students, postdoctorals, etc., per senior investigator. But the NSF pattern set the tone for what was happening in the nation's university departments of mathematics, applied mathematics, and statistics. By the early 1980s, NSF supported five times as many senior investigators as DOD in these core fields.

B. Recent Trends

When university support for postdoctoral positions, research associates, support staff, travel, etc., weakened in the mid-1970s, a new look at priorities began. In 1976 the newly formed Committee on Science

Policy of the American Mathematical Society wrote a report calling for increased postdoctoral support and a program of "mini-institutes." In the National Science Board and in the Mathematical Sciences Section (MSS)[3] of NSF, discussions about postdoctoral support were also going on, and by 1978 a new postdoctoral program in MSS was started. During the latter part of the period, the idea of a mathematical sciences research institute was examined. This institute was intended to increase available resources and direct more resources toward the support of young people. After a sometimes-heated debate in the research community, the NSF solicited proposals for institutes and alternative ways to bolster research in mathematics. On the recommendations of several review panels, a plan was adopted to package resources under the banner of "coherent modes" of support, phased in during FY 1981 and consisting of:

- a Mathematical Sciences Research Institute at Berkeley;
- an Institute for Mathematics and Its Applications at the University of Minnesota;
- a program of intensive summer conferences;
- increased support for recent Ph.D.'s at the Institute for Advanced Study, the Courant Institute (NYU), and the Mathematics Research Center (Madison).

In FY 1982, NSF also initiated an instrumention (computer equipment) program.

Table B-2 details the growth of support for these items. Note: (i) a growth in graduate student support; (ii) a 14% increase for mathematical scientists in 1983, the first above inflation in many years; (iii) a larger increase in FY 1984 (22%). Figure B-1 relates the trends in numbers of graduate students and postdoctorals supported on NSF grants to the number of senior investigators. Figures B-2 and B-3 show similar data for chemistry and physics support at NSF.

The heavy concentration of mathematical sciences resources on senior scientists (summer research time) is evident in Figure B-1. Equally apparent is the steady (except for FY 1981) decline in the number of senior investigators supported, as support for graduate students and postdoctorals goes up.

[3] Now the Mathematical Sciences Division.

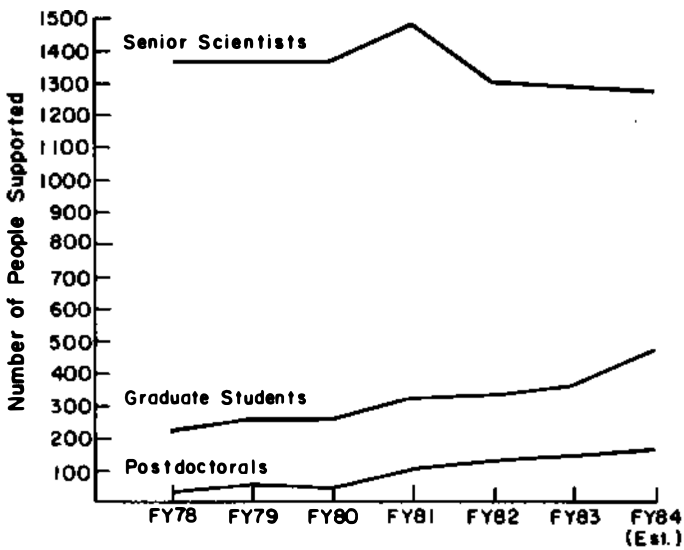
TABLE B-3. Some Trends in NSF Mathematical Sciences Support
($ Thousands)

	FY 1980	FY 1981	FY 1982	FY 1983	FY 1984[4]
Postdoctoral Fellowships	600	700	1,000	1,200	1,850
Postdoctoral Res. Associates on Grants	100	100	100	150	300
Coherent Modes[5]	700	1,300	3,400	3,650	4,000
Equipment Initiatives	---	---	690	840	1,100
Graduate Student Support	1,250	1,500	1,500	1,700	3,500
Total MSD Budget	25,500	28,300	30,500	34,700	42,200

Source: National Science Foundation

Figure B-1

Personnel
Mathematical Sciences – NSF



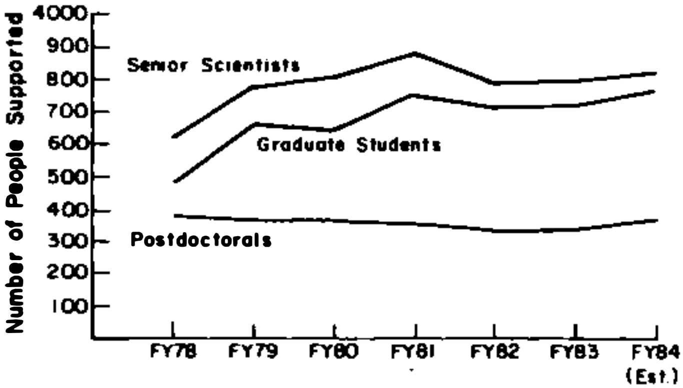
Source: National Science Foundation

[4] These were the FY 1984 figures as per budget. The $42.2 million available to be used for direct support of the mathematical research enterprise has been reduced to about $39.2 million because of internal shifts within the Foundation; presumably the various programs of MSD will be reduced proportionately.

[5] Of the coherent modes money awarded to the research institutes, about 60% supports young people in the postdoctoral mode.

Figure B-2

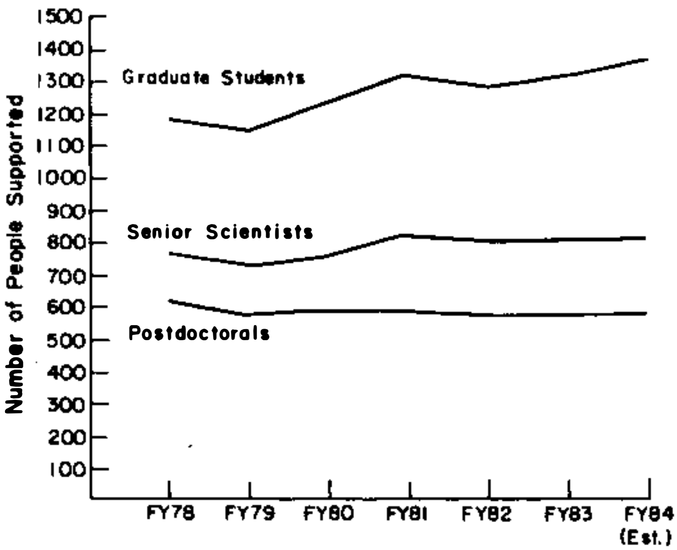
Personnel
Physics — NSF



Source: National Science Foundation

Figure B-3

Personnel
Chemistry —NSF



Source: National Science Foundation

The comparison of Figure B-1 with Figures B-2 and B-3 shows the very different balance in allocation of resources in the mathematical sciences. Obviously, laboratory sciences more directly use graduate students and postdoctorals in the research of the principal investigator. We note, however, that in theoretical physics NSF supports an average of one-half graduate student and one-third postdoctoral per senior investigator. The absolute number of senior mathematical scientists supported by NSF is larger because 75% of all mathematical scientists with federal support are funded by the Foundation, whereas numerous chemists and physicists are supported by other agencies.

Recommendations to increase graduate student support in the mathematical sciences have come from several sources. One of these was the Mathematics Briefing Panel (Browder Panel) of the NAS's Committee on Science, Engineering, and Public Policy (COSEPUP), which was chaired by Professor William Browder of Princeton. This panel reported to COSEPUP in the fall of 1982, then briefed the President's Science Advisor, Dr. George A. Keyworth II, and the Director of the National Science Foundation, Dr. Edward A. Knapp, on potentials and future needs of the mathematical sciences.[6] These briefings brought the mathematical sciences special attention in the President's FY 1984 budget, which included a sizeable increase for the National Science Foundation. The panel's recommendations have been reinforced by the priorities set by the Advisory Committee to the Mathematical Sciences Division at NSF.

The significant growth in the Mathematical Sciences Division's FY 1984 budget, as Table B-3 partly indicates, was targeted to increase the level of support for (i) graduate students; (ii) young investigators and postdoctorals (both as postdoctoral fellows and postdoctoral research associates[7]); (iii) mid-level and senior investigators for short-term visits at centers of excellence; and (iv) research in computational equipment, and computer time. A very modest rise, at inflationary level, is planned for expected increased costs outside of these categories. The number of senior investigators to be supported is not expected to increase in FY 1984; indeed, as we saw in Figure B-1, a further small decrease seems inevitable.

[6] The panel's report is Attachment 1 to this report.

[7] The support level for young investigators at the research institutes was held at a constant level.

C. An Overview

We believe the changes going on at NSF to be moves in the right direction, in the sense that any sensible long-range plan for mathematics funding must provide balanced support for the several basic research needs which mathematicians have. Since 1981, however, these moves have taxed another part of the research program. Whereas steps in 1981 began to increase graduate student and postdoctoral support in tandem with modest growth in the number of senior investigators supported, the three succeeding years saw their numbers drop by 15%.[8] Reductions also occurred at DOD, because of changes which we shall discuss in the next subsection. Overall, the number of established investigators with federal support dropped in these three years from over 2,100 to just under 1,800. We do not believe that the mathematical sciences research effort can adequately sustain itself unless the number of investigators supported is increased.

III. DOD SUPPORT: A CONTINUING DIALOGUE

DOD support of mathematical sciences R&D is provided in two ways:

organizations (primarily academic), and (ii) through operations conducted in-house, in their own laboratories and other installations. We are concerned with basic research, which is virtually all extramural and conducted at academic institutions or research centers. The funding is concentrated in the Army Research Office (ARO), the Air Force Office of Scientific Research (AFOSR), and the Office of Naval Research (ONR). These entities vary in size and organization. We will discuss very briefly FY 1982–84 trends in budgets of the mathematical sciences divisions of these agencies, then pull together coherent data on trends in total DOD support, and finally discuss structural budget issues at DOD.

A. Army Research Office

Table B-4 lists the programs in the ARO Mathematical Sciences Division, together with Computer Science, which is a program of the Electronics Division.

[8] Many factors contributed to this reduction: the increases in support of postdoctorals, research institutes, etc.; inflation; and overhead rate increases by universities, negotiated because of double-digit inflation a few years ago.

TABLE B-4. ARO--Mathematical and Computer Sciences
($ Thousands)

Subactivity	Actual FY 1982	Current FY 1983	Estimate FY 1984	% Increase FY 1984/83
Nonlinear Analysis[9]	$ 1,900	$ 1,670	$ 1,940	16.2
Computational Methods and Mathematical Software	1,800	1,700	1,900	11.8
Statistical Methods and Operations Research	1,300	1,200	1,400	16.7
Systems Theory, Control, and Modelling	900	900	1,200	33.3
Conferences and Special Projects[10]	100	160	160	.0
Computer Science	1,000	1,000	1,000	.0
Total	$ 7,000	$ 6,630	$ 7,600	14.6

Army laboratories such as Aberdeen, Picatinny, and Watervliet conduct a certain amount of mathematical R&D. In FY 1982 and FY 1983 that amounted to $1 million and is not included in Table B-4.

B. Air Force Office of Scientific Research

Table B-5 displays six major program areas at AFOSR in mathematical and information sciences. The seventh area, Information Electronics, concerns mathematics related to electronic communications.

The footnotes indicate special funds dedicated to providing research emphasis, or, as the Air Force puts it, "initiative" in certain years. Numbers in parentheses are core program funds, i.e., program funds without the initiative money.

The Air Force also supports a substantial amount of R&D intramurally at several Air Force installations. In FY 1984, support is planned as follows: Eglin Air Force Base, $550,000; Rome Air Development Center, $525,000; Wright-Patterson Air Force Base, $500,000; and Kirtland Air Force Base, $40,000. This work is quite mission-directed, with about a 33-67 split between computer science and mathematically-oriented research.

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[9] The FY 1984 numbers for Special Projects include $80,000 for Special Graduate Fellowships.

[10] The support of the Mathematics Research Center, University of Wisconsin, is embedded in the first three subactivities listed. These amounts are FY 1982—$2,121,000; FY 1983—$2,200,000; FY 1984—$2,200,000.

TABLE B-5. AFOSR--Mathematical and Information Sciences
($ Thousands)

Topic	Actual FY 1982	Current FY 1983	Estimate FY 1984	% Increase FY 1984/83
Control Theory	$ 853	$ 1,051	$ 1,298	23.5
Computer Science	3,346	4,370	5,180	18.5
	(1,800)	(1,970)	(1,980)	(0.5)
Computational Mathematics	1,397	1,900	1,975	3.9
Physical Mathematics	1,033	867	1,000	13.3
Probability and Statistics	1,798	1,860	3,320	78.5
			(1,820)	(-2.1)
Systems Science	1,659	1,355	1,400	3.3
Information Electronics	580	605	637	5.3
Total	$10,666 [a]	$12,008 [b]	$14,810 [c, d]	23.3
	(9,120)	(9,608)	(10,110)	(5.2)

[a] Includes Systems Automation Initiative of $1,546 thousand.

[b] Includes Systems Automation Initiative of $1,200 thousand and Image Understanding Initiative of $1,200 thousand.

[c] Includes Systems Automation Initiative of $1,200 thousand and Image Understanding Initiative of $2,000 thousand.

[d] Includes Reliability for Real Systems Initiative of $1,500 thousand.

C. Office of Naval Research

Recent reorganization of the Office of Naval Research has split the former Division of Mathematical and Information Sciences into a Division of Mathematical Sciences and a Division of Information Sciences, with the Field Dynamics Program being folded into the Division of Mechanics.

The Mathematical Sciences Division is composed of two groups: 1) a Mathematics Group and 2) a Statistics and Probability Group. The budget is in Table B-6.

Normally, about 30–40 percent of the ONR Program's funds are devoted to "special focus" programs, analogous to the AFOSR "initiatives."

These figures do not contain monies which ONR handles for R&D work at the Naval Research Laboratory (NRL), Naval Air Systems Command, Naval Sea Systems Command, and the Naval Electronic Systems Command. Funds for each of these run about $1 million, though NRL funds are somewhat larger.

TABLE B-6. ONR--Mathematical Sciences Division
($ Thousands)

Group or Program	Actual FY 1982	Current FY 1983	Estimate FY 1984	% Increase FY 1984/83
Mathematics				
Applied Mathematics	$ 1,923	$ 2,873	$ 2,725	-5.2
Numerical Analysis	1,685	2,813	3,300	17.3
Operations Research (Math.)	1,540	1,609	1,534	-4.7
Statistics and Probability				
Statistics	3,931	3,826	3,400	-11.1
Operations Research (Stoch.)	640	1,054	1,005	-4.6
Systems Science	838	525	531	1.1
Total	$10,557	$12,700	$12,495	-1.6

TABLE B-7. DOD Funding Levels FY 1982-84 for Basic Academic Research in Applied Mathematics, Probability, and Statistics
($ Millions)

	FY 1982	FY 1983	FY 1984
ARO	6.0	6.5	6.3
AFOSR	6.7	7.1	8.5
ONR	10.6	12.7	12.0
DOD TOTAL	23.3	26.3	26.8

D. Total DOD Support

The descriptions of the programs in the three service agencies reveal that the total of the budgets of the three "mathematics" divisions would include support for things outside the mathematical sciences. The heads of the three divisions have provided us with the data relevant to this report. Table B-7 shows FY 1982–84 trends in DOD support of basic academic research in applied mathematics, probability, and statistics. Unlike NSF, basic DOD support of the mathematical sciences decreased in constant dollars from FY 1983 to FY 1984.

Over the historical period we have been discussing, these changes occurred in DOD support:

- total DOD support of the mathematical sciences decreased by 25% in constant dollars.

- a shift in emphasis toward direct mission relevance phased out virtually all support of pure mathematics and limited the support of fundamental applied mathematics as well.

We are concerned about the level of total DOD support and about what is supported within the framework of DOD policy. Most of the issues we will raise are discussed in the special briefing report of the COSEPUP Mathematics Briefing Panel, prepared for the Office of the Undersecretary of Defense for Research and Engineering by a group from the mathematical community headed by Dr. Hirsh Cohen of IBM's T. J. Watson Research Center. The group briefed the Undersecretary, Dr. Richard DeLauer, and subsequently a DOD-University Forum subcommittee, chaired by Professor Ivan Bennett of Rockefeller University. The report of the Cohen Panel, included as Attachment 2 to our report, lays solid groundwork for discussion and should be read as background for our next points.

E. Initiatives

For a number of years there has been concern at DOD about the level of its funding of basic scientific research. DOD calls such funds 6.1. At least part of DOD management thinks that Congress has not supported 6.1 programs at AFOSR, AOR, and ONR. The approved resources are not providing for any growth. An effort began several years ago to reformulate portions of the basic science programs as "special research opportunities" and "special focus programs" at ONR, "thrusts" at ARO, and "initiatives" at AFOSR. These are three- to five-year projects of the $0.5–$3.0 million per year size focused on a set of problems directly relevant to the DOD mission. The case is made that the 6.1 budget situation has improved somewhat as a result of these efforts. But we feel that some problems have been created as well; these are likely to get worse if the special programs should become too large a fraction of the 6.1 budgets:

1. Use of "thrusts" limits growth of the "core" program, i.e., those parts of the 6.1 efforts which support fundamental research in the disciplines.
2. The inevitable drift is toward the more immediately applicable.

3. Since mathematical and computer sciences are so closely allied at DOD, mathematical initiatives tend to go into computer science areas; the resulting overall budget figures will then (once again) mask the fact that support of mathematics is weakening.

Other serious questions arise, such as (a) whether there will not be a drift away from highest quality, because only occasionally will there be a good match between what the best people in basic research are doing and this sort of short-term focused effort, and (b) whether there may not be grave wrenching effects when initiatives are terminated. We are also concerned about the consumption of program officer time in preparing for the annual competitions to determine which of the many proposed initiatives will be funded by management. Decisions on which parts of science are to be supported should be made at the program officer level, where the greatest scientific understanding is.

Of the three basic concerns cited, we focus primarily on the first two, because we take the third to be self-evident.

Our concerns do not stem from the fact that the mathematical science units of the service agencies have been faring poorly in the competition for special focus resources—quite the contrary. Nor, with the exception of our point about computer science, do they have to do with the fact that money is moving out of the mathematical sciences. When one raises concerns about the fate of the core programs and the drift toward immediate applicability, the points are often made in response that (i) the resources are still going into research in the universities; (ii) the research supported through the specially focused programs is just as "basic" as the research in the core program; (iii) it is the responsibility of the program officers to see to it that the initiative proposals brought forward are in sound basic science. These are all true statements, even if (ii) is unlikely to hold over time, but they do not address the primary concerns we have.

The present mode of support for the mathematical sciences in DOD is headed toward heavy concentration on work of a short-range nature. Initiatives and special focus programs produce this result because the mission-oriented problems call for specific formulations and need the relatively quick application of approaches that are understood and have been tried. A certain amount of this is healthy for all branches of mathematics, but it will not meet the future needs of DOD.

The history of DOD's support of mathematics tells a lot about this.[11] Some methods and techniques were developed out of particular problem stimulation, but the research climate allowed for their long-range development. It also allowed, most importantly, for DOD support of topics that were felt to be of use in the long run. In many cases, these judgments of the research community and DOD mathematics program officers produced results of great value.[12]

We conclude that:

- DOD policy changes after the mid-1960s significantly narrowed the scope of what service agencies support in the mathematical sciences;
- emphasis on initiatives is shifting programs toward immediate applicability;
- since 60% of existing support for applied mathematics and statistics is at DOD, this is a matter of serious concern.

DOD is a mission agency, but the scope of its dependence on the scientific/technological effort of the country is enormously broad. Virtually every part of technology bears on the long-term mission of DOD, as does virtually every part of mathematics. DOD's success over the long run depends in part on the health and vitality of the mathematical sciences; hence, appropriate ways must be found to strengthen DOD support of the field.

F. Proposals for Discussion

DOD and the mathematical community must continue constructive dialogue, as exemplified by the efforts of the Cohen Panel. The DOD-University Forum is supporting ongoing discussion. We would like to propose that:

- Mathematical program officers, who are closest to the work of greatest potential value, should have resources for new core programs that allow them wider latitude in what they support.
- The mathematical sciences should be made a technical objective of DOD. Mathematical tools typically are applied in a number of

11 See Attachment 2.
12 Ibid.

areas, making it difficult to justify work on them in terms of one DOD functional objective.

- A high-level mathematical advisory committee such as recommended by the Cohen Panel should be established and utilized for several years to coordinate DOD programs and the effort to rebuild federal support for the mathematical sciences.
- The mathematical community should help in promoting understanding in Congress of DOD's role in the support of basic scientific research, and of the importance of this research to the country.

IV. DOE SUPPORT: MATHEMATICS OF COMPUTATION

R&D activities in the mathematical and computer sciences at the Department of Energy are funded prinarily through the program in Applied Mathematical Sciences within the Division of Engineering, Mathematical, and Geosciences. This Division is in turn a subactivity of a program in Basic Energy Sciences, the principal program in the DOE category of Supporting Research and Technical Analysis.

The AMS program funds basic research at many of the national laboratories, universities, and private research institutions in three major categories: analytical and numerical methods, information analysis techniques, and advanced computing concepts. Table B-8 displays the budget for the AMS program.

The Department of Energy program in the Applied Mathematical Sciences has been and continues to be the leading federal agency program in support of research at the interface between the mathematical and the

TABLE B-8. DOE--Applied Mathematical Sciences
Budget Authority by Component ($ Thousands)

Component	Actual FY 1982	Current FY 1983	Estimate FY 1984	% Increase FY 1984/83
Analytical and Numerical Methods	$ 6,000	$ 6,800	$ 7,000	2.9
Information Analysis Techniques	1,900	2,000	2,000	.0
Advanced Computer Concepts	3,200	4,350	4,970	14.3
Special Projects	500	700	700	.0
Total	$11,600	$13,850	$14,670	5.9

computer sciences. The program supports research on numerical analysis, scientific computing, software engineering, database structures, and computer architecture, both at the National Laboratories (where approximately 50% of the funds go) and at the universities. Academically-based mathematical sciences, although a relatively small part of the total activity, constitute leading-edge research. Many researchers supported by the program also have intimate and highly productive associations with centers of research at the National Laboratories.

This program, following recommendations of the Lax Panel Report on Large-Scale Computing in Science and Engineering, is taking a leading role in providing access to advanced computers (Class VI) for the entire scientific community supported by DOE, including the mathematical scientists.

The impact of the DOE program is significant for the numerical analysis and scientific computing research communities. However, its effect on the entire mathematical sciences community is limited by its small budget, which is effectively focused on the mission of the department and its laboratories.

V. CURRENT SUPPORT LEVELS

Table B-9 shows federal support of basic academic research by agency for the fiscal years 1982–84.

The only figures in Tables B-9 and B-10 which appear in published federal budget data are those for NSF's Mathematical Sciences Division. The next section explains how the other figures are obtained.

TABLE B-9. Federal Support of Basic Academic Research in the Mathematical Sciences, Recent Years ($ Millions)

	FY 1982	FY 1983	FY 1984
NSF Mathematical Sciences Division	31.2	34.7	42.2
DOD Applied Math, Probability, Statistics	23.3	25.3	28.1
Other NSF Support (estimate)	3.0	3.0	3.0
DOE (estimate)	2.3	2.8	2.9
NASA, NIH, NBS (estimate)	2.0	2.0	2.0
Total	61.8	67.8	78.2

TABLE B-10. FY 1982 Funding Levels for Basic Academic Research in the Mathematical Sciences, by Major Subfield ($ Millions)

Agency	Pure Mathematics	Applied Mathematics	Probability & Statistics	TOTAL
AFOSR	0. 0	4.9	1.8	6.7
ARO	0.6	4.1	1.3	6.0
ONR	0.05	5.15	5.4	10.6
DOD Subtotal	0.65	14.15	8.5	23.3
NSF--Math. Sci. Sect.	21.00	5.70	4.5	31.2
NSF--Other (est.)	0.0	2.8	0.2	3.0
DOE (est.)	0.0	2.1	0.2	2.3
Other (NASA, NIH)	0.0	1.5	0.5	2.0
Total	21.65	26.25	13.9	61.8

VI. ICEMAP

In 1979, NSF took the initiative in reactivating the Interagency Committee for Extramural Mathematics Programs (ICEMAP). On December 18 of that year, the Committee convened with representatives from the Air Force Office of Scientific Research, Army Research Office, Office of Naval Research, Department of Energy, Department of Transportation, National Security Agency, Department of Justice, National Bureau of Standards, NIH, and NSF in attendance. The purpose was to permit the representatives from the various agencies to obtain valid, timely information on each others' programs with a view to strengthening coordination and improving understanding of the true content of agency programs and their significance in advancing the mathematical sciences. Generally, the Committee meets twice a year to discuss the current status of extramural support and to address current issues. The representatives of NSF, AFOSR, ARO, ONR, and DOE meet more frequently.

Part of the difficulty in determining what is taking place in extramural support of the mathematical sciences is that nearly all the mission agencies (with the exception of ONR and AFOSR) budget on a project basis. Someone must estimate the percentages of project funds that go to mathematics, physics, chemistry, etc. DOD does not classify any of its 6.1 basic research as engineering and, consequently, even the Air Force and Navy numbers for the science disciplines and mathematics must be adjusted to show how much of each goes to support engineering research of various types.

It is through ICEMAP, particularly the representatives from AFOSR, ARO, DOE, NSF, and ONR, that meaningful data on support of the field have been accumulated. Their help is necessary principally because (i) support for the mathematical sciences at most mission agencies is scattered throughout many programs; (ii) even where there is a mathematics program or a unified mathematical sciences budget, the taxonomies of the "mathematical sciences" vary, as do the meanings of "research." If an aggregated budget is to be meaningful, consistent definitions must be employed.

Two problems complicate data collection: much R&D in mission agencies is not basic research, and "mathematical sciences" is not defined consistently.

When ICEMAP was reactivated five years ago, Dr. Ettore F. Infante, the program manager for Applied Mathematics at NSF, undertook a study called *Other Agency Support of Research in the Mathematical Sciences FY* 1981.[13] The following year his successor, Dr. James Greenberg, repeated the exercise. Using the taxonomy of the mathematical sciences employed by NSF's Division of Mathematical Sciences,[14] they reviewed extramural grant proposals at the mission agencies to identify which supported basic academic research in the field. From these reviews emerged: (i) accurate data on basic research, using NSF taxonomy; (ii) good estimates of total federal support, using the broader definition of the mathematical sciences which is employed in this report; (iii) understanding among the major agency representatives of the common terminology.

This understanding has deepened over the past four years, as close cooperation in support of the field has increased. As a result, data can be reliably derived without the necessity of repeating each year the time-consuming grant-by-grant reviews of 1980–81.

The role of ICEMAP is crucial in accumulating sound data on the support of the field and in balancing it to ensure continued health of the mathematical sciences.

VII. MASKING BY PUBLISHED AGGREGATE DATA

The deterioration of federal funding for basic research in the mathematical sciences has been obscured in regularly published data on federal

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13 Internal NSF document.

14 At that time, the Mathematical Sciences Section in the Division of Mathematical and Computer Science.

science support by the two practices to which we have referred: (i) failure to distinguish between basic research and R&D; (ii) inconsistent use of terminology. Probably the most dramatic instance of this "masking" is the past practice of lumping together mathematics and computer science. We shall give other examples later on.

Our purpose in this section is not to write a definitive essay or analysis, but to show by specific examples the care which must be exercised in dealing with data on research support, especially in the mathematical sciences.

A. University-Supplied R&D Data

A regular NSF publication, *Academic Science/Engineering: R&D Funds*, gives data solicited from universities. The 1973–81 data for Mathematics and Mathematical/Computer Sciences are in Table B-11.

One must not use data of this type to identify either levels of support for basic mathematical sciences research or trends in such support. Let us illustrate why.

A cursory examination of Table B-11 reveals that federally supported "mathematics" R&D at academic institutions increased by 24% from 1976 to 1977 and by 34% from 1978 to 1979. These changes are plainly misleading insofar as they relate to support trends for basic science over the last decade, since they suggest that "mathematics" support increased 82% from 1976 to 1979![15]

TABLE B-11. Federally Financed R&D Expenditures at Universities and Colleges FY 1974-81 Mathematical and Computer Sciences ($ Thousands)

	Mathematics	Computer Sciences	Mathematical/Computer Sciences
1973	28,756	24,929	53,685
1974	29,396	28,711	58,107
1975	31,224	33,875	65,099
1976	32,882	32,925	65,807
1977	40,638	37,546	78,184
1978	44,130	41,214	85,344
1979	60,431	69,192	129,623
1980	61,036	76,917	137,953
1981	67,574	93,374	160,948

Source: Academic Science/Engineering R&D Funds, Fiscal Year 1981 (NSF 83-308) Table B-4
1973 data from NSF 81-301

15 The budget of NSF's Mathematical Sciences Section increased 32% from 1976 to 1979.

There are less obvious problems. Take FY 1980 as a sample year. On page 99, Table B-50 of NSF 83-308 shows that Johns Hopkins University received $12.4 million in federal R&D funds in "mathematics" in FY 1980; that is, 20% of the total of $61 million for the whole country. There are excellent mathematical scientists at Hopkins, to be sure. Research goes on in the Departments of Mathematics, Mathematical Sciences, and Biostatistics, as well as in the Applied Physics Laboratory (APL). The NSF supported $160,000 of their research and several hundred thousand were supplied by other agencies (most of it going to APL, where three to five full-time people were involved). We wondered where the additional $12 million might have been spent. Thinking that it might have gone into a huge classified project, we checked with the (then) head of the Applied Mathematics Group at the APL Research Center. He could shed no light on the mystery.

This large sum of "mathematics" R&D suddenly appeared in 1979. It accounted for most of the 34% increase from 1978 to 1979. A large increase occurred that year in computer science R&D also because of the sudden appearance of money at Hopkins.[16]

That was a big "distortion" from the basic research point of view, but in FY 1980 there are many smaller ones. For instance, Table B-50 (NSF 83-308) shows that the University of Dayton received $1.5 million in federal R&D money in "mathematics"; that St. John's University received more than MIT; and that the University of North Carolina at Chapel Hill received $0.3 million, whereas in FY 1979 it received $4.3 million.

Such anomalies might not matter in a field with large basic academic research budgets, but in the mathematical sciences they cause wide fluctuations. If R&D data, as in Table B-11, are confused with basic research data, they create the illusion that support for the field is going up, when in fact in constant dollars it is flat.

B. Agency-Supplied Data

Research support data from the federal agencies can be more reliable, although difficulties with the published aggregate data abound. Consider the publication *Federal Funds for R&D FY* 1980, '81, '82 (NSF 81-325). Its Table C-85 on page 124 relates to the FY 1980 data we

[16] Rather clearly there was a bookkeeping change at Johns Hopkins.

TABLE B-12. Federal Support ($ Millions)

	Chemistry	Physics	Math/Comp. Sci.	Comp. Sci.	Math
NSF-MPS	139[17]	199[17]	82	47	36
NSF-81-325	136	194	80	31	46

just discussed. For that year, the table shows a total of $7.8 million for basic research in "mathematics and computer sciences" at universities and colleges, about 57% of it in mathematics. Let us examine the data more closely.

Table B-12 compares support for chemistry, physics, mathematical sciences, and computer science from NSF 81-325 with those of an unpublished study by NSF's Mathematical and Physical Science Directorate (MPS). Note how closely the results match for chemistry and physics. There is close agreement on the sum of mathematics and computer science, but not on the two components separately. The "mathematics" numbers in NSF 81-325 appear 28% too high, the "computer sciences" numbers correspondingly too low. In fact, the situation is more complicated. The MPS figure of $36 million for mathematics employed NSF taxonomy of the mathematical sciences. In the taxomomy of this report, support in FY 1980 was about $48 million. The MPS computer science figure is $16 million higher because it includes support of basic research by the Defense Research Projects Agency (DARPA) which is funded by 6.2 money at DOD and therefore not reported as "basic research" in the survey. Thus the total for mathematical and computer sciences should have been about $95 million, half in each field.

Since the $48 million for mathematical sciences is quite close to the $46 million in NSF 81-325, one is tempted to think that data published regularly in *Federal Funds for R&D* might be used to gauge funding levels and trends for basic research in the mathematical sciences. But things are not that simple.

Table B-13 shows FY 1976–84 data from *Federal Funds.*

[17] Includes estimates of research in Materials Science Division. The Chemistry/Physics data could vary a few percent because of this.

TABLE B-13. Federal Support of Basic Academic Research in the Mathematical Sciences, FY 1976-84 ($ Millions)

1976	1977	1978	1979	1980	1981	1982	1983 (est.)	1984 (est.)
27.1	34.0	36.0	37.1	46.1	54.0	60.9	68.9	81.4

Source: Federal Funds for R&D, NSF

The FY 1982 figure of $60.9 million compares well with our estimate, which is $61.8 million. The FY 1983–84 estimates are too high by $2–3 million, but they are only preliminary. It is striking that the FY 1976–79 numbers are much too low. We estimate support in FY 1976 to have been over $35 million, 30% higher than the figure shown. A constant dollar graph of the figures in Table B-13 would show real growth, whereas the reality is that support in constant dollars was flat.

Close examination of the data behind Table B-13 reveals a number of discrepancies in the agency reporting of data, presumably related to the different judgments made as to what constitutes the mathematical sciences. The publication *Federal Funds* regularly warns the reader about this sort of difficulty.

The soundest data on federal support are those collected by the people in the agencies at the scientific discipline level who understand and fund the research going on. In mathematical sciences, the soundest data, the ones the Committee used, have been developed by ICEMAP.

C. General Descriptions of Trends

To illustrate how the growth of computer science has masked the funding problems in mathematics, we quote from *Federal Funds for Research and Development, Fiscal Years* 1981, 1982, *and* 1983, NSF 83-320:

> "*Mathematics and computer sciences* represented 3 percent of the 1983 Federal research total, even though the average annual rate of growth of 13.4 percent between 1973 and 1982 was the highest of all the major fields of science."

A casual observer reading that statement would think that mathematics funding had fared well over the nine-year period. In fact, support for basic research in the field grew at the average rate of 9%, just at the inflation rate. Computer science grew at an average annual rate of 17% or more.

The two fields are not now joined because of any failure to identify them separately, but because "mathematics and computer sciences" has become a standard major category like "physical sciences," "biological sciences," etc. Perhaps future statements will point out the discrepancy in the rates of growth.

D. A Final Comment

The data in publications such as *Federal Funds for Research and Development* are gathered with great care. This publication defines its terminology very carefully and, as we indicated, warns its readers about variable judgments exercised by agency representatives. It is important that our comments about the data not be construed as criticism of the *Surveys of Science Resources.* We present them only to illustrate the great care which must be exercised when relating the data to the mathematical sciences.

VIII. HISTORY OF THE PERIOD 1968–82

The Office of Naval Research was created by an act of Congress in 1945. This agency was the earliest supporter of mathematical research. During the late 1940s to early 1950s, it was joined by the other services (the entities now called AFOSR and ARO) and by the National Science Foundation, which awarded its first mathematics grant in 1952. DOD and NSF continued as the major supporters, with the balance shifting more toward NSF.

We want to bring out several features of the history of mathematical science funding. Some of the precise historical data are virtually impossible to find. From years gone by, we have accurate data for NSF and AFOSR, but only estimated data for other agencies. Nonetheless, we have enough to tell the story of funding over the last decade-and-a-half.

Table B-14 shows the history of three aspects of Air Force support of basic scientific research, from FY 1966 through FY 1979: total 6.1 funds (basic research budget), total contract and grant research support

TABLE B-14. Air Force 6.1 Program and AFOSR Contract/Grant and the Mathematics and Information Science Subelement ($ Thousands)

FY	6.1 Funds	AFOSR C/G	AFOSR C/G Math & Info. Sciences
66	94,455	37,986	5,077
67	96,542	40,190	4,913
68	92,000	38,839	4,515
69	95,817	39,409	4,258
70	85,274	31,235	3,763
71	85,215	29,480	4,077
72	87,160	30,203	4,138
73	80,000	23,961	3,300
74	77,840	23,728	3,282
75	78,022	24,658	3,634
76	83,724	29,515	4,235
77	84,788	42,146	5,977
78	95,084	47,536	6,454
79	105,016	51,809	6,734

Source: Air Force Office of Scientific Research

at AFOSR, and the portion of it in the mathematical and information sciences.

These dramatic decreases were paralleled in other DOD agencies. They had begun before 1965, as a result of DOD policy changes emphasizing mission-oriented research more directly.

During the FY 1969–74 period, Congress and the President reduced the NASA and DOD budgets substantially. The FY 1971 federal budget included a 12% cut for NASA. DOD's outlays were reduced $5.8 million. Space and defense research budgets were hard-hit by these moves. Thousands of technically trained people were affected by this change in federal priorities. An early result was an apparent oversupply of Ph.D. scientists, which motivated OMB and Congress to terminate NSF Institutional Science programs and to approve cutbacks and phase-outs of graduate student support programs. The number of mathematics students receiving NSF Fellowships or Traineeships was reduced from 1,179 in FY 1969 to 116 in FY 1974, a 90.2% in just five years. Decreases in National Defense Education Act fellowships were just as dramatic. In 1969, the number in mathematics was about 400. By 1973, there were no new NDEA Fellows, NSF Predoctoral Fellows, or NSF Predoctoral Trainees.

A. The Mansfield Amendment

The Mansfield Amendment insured that basis research projects were terminated first, as federal priorities shifted from defense and space towards "civilian" programs.

President Nixon signed the Military Procurement Authorization for FY 1970 into law on November 19, 1969. Section 203 of that Act was the "Mansfield Amendment." In a December 5, 1969 letter to Dr. William D. McElroy, then-Director of NSP, Senator Mansfield said: "In essence, it [the amendment] emphasizes the responsibility of the civilian agencies for the long-term, basic research. It limits the research sponsored by the Defense Department to studies and projects that directly and apparently relate to defense needs."

This amendment had particular significance for mathematical research. Before its passage, DOD and other mission agencies were supporting a large number of basic research projects in mathematics. NSF identified more than $2.2 million in DOD and other agency mathematics research projects that would "fall out" as a result of the Mansfield Amendment.[18] Dropouts in chemistry totalled $4.8 million, those in physics $4.0 million.

Although not legally required to do so, other agencies applied the Mansfield Amendment's mission relevance criteria to their own programs, to terminate or cut back support for basic research. In all, NSF identified 89 principal investigators working on mission agency-funded mathematical research who would have to either seek support elsewhere or revise their research to "directly and apparently" relate to defense needs. Funding for the 89 PIs totaled more than $2.2 million in FY 1969.

Congress appropriated $10 million in FY 1971 and $40 million in FY 1972 specifically to help NSF compensate for these cutbacks. The Foundation had estimated that about $100 million in mission agency research would be affected by the Mansfield Amendment and that the Foundation would receive at least $50 million worth of research proposals as a consequence of the amendment.

The NSF Mathematics program did not benefit from these increased appropriations. Table B-15 shows what happened to NSF funding in two

[18] This was direct fallout only because of the Mansfield Amendment. Much had been lost before it came along.

TABLE B-15. Comparison of Funding Levels for Chemistry, Physics and Mathematics During Critical Period of Mansfield Amendment ($ Millions)

Year	Mathematics Oblig.	Mathematics % ±	Chemistry Oblig.	Chemistry % ±	Physics[19] Oblig.	Physics[19] % ±
1970	$12.6	---	$17.4	---	$23.8	---
1971	12.9	2.4	19.6	12.6	26.5	11.3
1972	13.8	7.0	24.5	25.0	33.3	25.6
Diff. 1970/1972	$ 1.2	9.5%	$ 7.1	40.8%	$ 9.5	39.9%
Avg. Inc. per Year	$ .6	4.7%	$ 3.5	20.4%	$ 4.7	20.0%

science disciplines and mathematics during the years most affected by the Mansfield Amendment.

Problems created by the Mansfield Amendment were only part of the picture. The shift toward "civilian" programs, and the presidential interest in what was termed "Science for the People," emphasized areas connected historically to industrial development, such as chemistry and materials research. Although the NSF and National Research Centers basic research budgets increased 80.8% between FY 1970 and FY 1975, the real push concentrated on industry-related fields.

B. A Caveat Concerning FY 1968

Total federal obligations for research and development grew in the 1960s, peaked in FY 1967 and FY 1968, then declined, picking up again in the mid-1970s. The peak was due in part to the Apollo program. FY 1967 and FY 1968 should not be used as benchmark years in general discussions of federal science support.

We use FY 1968 as a reference point for mathematical sciences funding only because it was the year in the 1966–70 period for which we have the most accurate support data. The peak funding year for the mathematical sciences was probably FY 1966.

C. The Period 1968–82

We want to discuss:

- the loss in federal support for the mathematical sciences, 1968–73.
- the flatness of it in the ensuing decade, in constant dollars.

[19] Solid state and low-temperature physics was moved from physics into materials research in FY 1971. The amount for FY 1970 has been adjusted to exclude $4.4 million in solid state and low-temperature physics to make it comparable with the amounts show for FY 1971 and FY 1972.

TABLE B-16. Estimated FY 1968 Funding Levels for Basic Academic Research in the Mathematical Sciences ($ Millions)

Agency	Level in NSF Taxonomy	Level in Report Taxonomy
AFOSR	3.2	4.0
ARO	1.5	2.6
ONR	1.1	4.4
DOD subtotal	5.8	11.0
AEC	1.0	1.5
NASA	0.6	0.7
NSF-MSS	12.9	12.9
NSF other	0.0	1.1
Other DOD, NASA, NIH	??	1.0
TOTAL	20.3	28.2

Determining funding levels for the field was more difficult in 1968 than it is today. On September 25 of that year, Dr. William Pell, Head of NSF's Mathematical Sciences Section, wrote a memorandum to Dr. William E. Wright, Division Director for Mathematical and Physical Sciences, in which he presented his best analysis of total federal support for basic mathematical research. He also described the difficulty of the task at some length and expressed the hope that some day it would be possible for the Interagency Committee (ICEMAP) to undertake a grant-by-grant analysis, as has been done in recent years. Table B-16 shows Dr. Pell's estimate of FY 1968 support, in the column headed "NSF Taxonomy." Also included is our best estimate of support in the broader taxonomy of this report, which uses the Pell memorandum extensively since it describes items not included in the first estimate. DOD archival data also helped. The difference between the two ARO numbers is $1.1 million for the Mathematics Research Center at Madison, which Pell stated that he omitted. Most of the ONR difference relates to the program in Statistics and Logistics, which he noted to be large.

Table B-17 shows the ICEMAP FY 1982 estimates, analogous to those for FY 1968.

The FY 1982 budget of $61.8 million is equivalent to $22.7 million in 1969 (FY 1968) dollars, or 81% of the FY 1968 budget. We know NSF and AFOSR budgets precisely over the years. They declined steadily, bottoming out in FY 1973. We do not have accurate yearly data for ARO and ONR, but it seems quite safe to conclude that most of the 20% loss occurred between 1968 and 1973.

TABLE B-17. FY 1982 Funding Levels for Basic Academic Research in the Mathematical Sciences ($ Millions)

Agency	Level in NSF Taxonomy	Level in Report Taxonomy
AFOSR	4.0	6.8
ARO	5.0	6.0
ONR	3.5	10.5
DOD Subtotal	12.5	23.3
DOE	1.8	2.3
NASA, NIH, NBS	1.2	2.0
NSF-MSS	31.2	31.2
NSF Other	0.0	3.0
TOTAL	46.7	61.8

This does not, however, represent all the loss in federal support of research in the mathematical sciences over that period. The federal fellowship reductions also occurred during 1968–73. It is quite easy to put a dollar figure on the value of the lost graduate student support. There were just under 1,200 NSF Predoctoral Fellowships and Traineeships in mathematics in 1968 and about 400 NDEA fellowships. By 1973, there remained just over 100. The average cost of one fellow was $5,100 in 1968, according to the Education Directorate of NSF; hence, $7 million in graduate student support was withdrawn.

Doctoral students are an important part of the research enterprise in the mathematical sciences. Virtually all of the $7 million plus of federal support for them disappeared in 1968–73. Therefore federal support of the research enterprise was reduced by $7 million. Support size had been $28.1+$7.5=$35.6 million, and it dropped to $22.7+$0.5=$23.2 million, a 33% reduction.

Our conclusions:

- federal support for mathematical sciences research enterprise in FY 1982 stood at less than two-thirds its FY 1968 level, in constant dollars;
- the principal reduction occurred in the period 1968–73;
- support was essentially flat in constant dollars for the ensuing decade.

APPENDIX C. ORDERING THE UNIVERSE: THE ROLE OF MATHEMATICS

By Arthur Jaffe

1. MATHEMATICS

Mathematics is an ancient art, and from the outset it has been both the most highly esoteric and the most intensely practical of human endeavors. As long ago as 1800 BC, the Babylonians investigated the abstract properties of numbers, and in Athenian Greece, geometry attained the highest intellectual status. Alongside this theoretical understanding, mathematics blossomed as a day-to-day tool for surveying lands, for navigation, and for the engineering of public works. The practical problems and theoretical pursuits stimulated one another; it would be impossible to disentangle these two strands.

Much the same is true today. In the 20th century, mathematics has burgeoned in scope and in diversity and has deepened in its complexity and abstraction. So profound has this explosion of research been that entire areas of mathematics may seem unintelligible to laymen—and frequently even to mathematicians working in other subfields! Despite this trend towards specialization—indeed because of it—mathematics has become more concrete and vital than ever before.

In the past quarter century, mathematics and mathematical techniques have become an integral, pervasive, and essential component of science, technology, and business. In our technically oriented society, "innumeracy" has replaced illiteracy as our principal educational gap. One could compare the contribution of mathematics to our society with needing air and food for life. In fact, we could say that we live in the age of mathematics—that our culture has been "mathematicized." No reflection of mathematics about us is more striking than the omnipresent computer; consider a few examples of how computers influence us:

Flight. Commercial airliners can now land without a pilot's even touching the controls. Data about speed and position are relayed automatically to a device called a Kalman-Bucy filter, which flies the plane by continually finding a "least-squares best fit" to a first-order approximation of the laws of Newtonian physics. Similar "state filters" guide rockets and space probes and trace satellites. These satellites and rockets transmit back to earth important pictures, which are "spectrally analyzed" by computer to sharpen and enhance the images.

Medicine. Large-scale sampling of data allows medical researchers to correlate disease with patterns of life style and nutrition; hence data analysis makes a general study of epidemiology possible. Computers are revolutionizing diagnosis by providing automatic blood and urine analyses as well as computer-assisted tomography (CT scans) of internal organs. Computers may soon be able to forecast medical dangers ten to twenty years in advance by running simple, noninvasive tests on a patient.

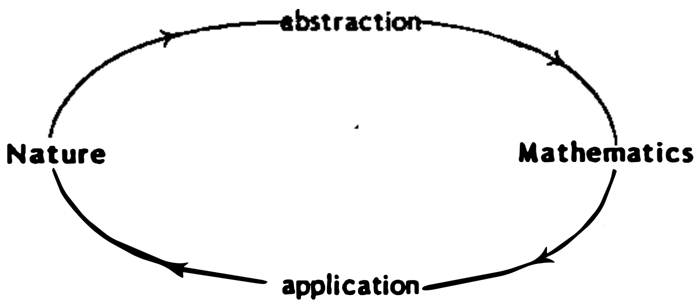
Business. The simplex method of linear programming has altered the planning of industrial production, manufacturing, inventory control, and distribution, by making it easy to compute the most efficient allocation of resources. The capacity to handle and store large blocks of data has revolutionized record keeping, billing, accounting, etc.

What do these widely different computer applications—Kalman-Bucy filters, image sharpening by spectral analyses, medical statistics, CT scanners, and linear programming analyses—have in common? Each is primarily based on linear algebra, a field of mathematics that was worked out in the late 19th century with none of these applications in mind: motivation to develop this algebra came rather from an attempt to understand the geometry of n-dimensional space.

The partial implementation of these ideas occurred during this century—by people with exceptional mathematical talent. Furthermore, each of these applications involves so much data that even the fastest computers could not obtain answers by simple brute force. They also required the development and use of sophisticated mathematical techniques.

We could write several volumes to document the utilitarian value of mathematical research to our society and to show how specific mathematical ideas have influenced our world. Instead, we have chosen a few cases to illustrate the breadth and the depth of the many spinoffs from mathematics. We have a second goal, perhaps more important than simply reporting about some developments at the forefront of mathematics and science. We want to emphasize two themes that occur over and over in the history we relate.

(1) Excellent mathematics, however abstract, leads to practical applications in nature. Hard problems in nature stimulate the invention of new mathematics.



One can enter this vigorous cycle of abstraction and application from either side. The time from mathematics to applications varies enormously. Sometimes it is immediate; sometimes it takes a century before abstract theory causes a revolution through its application. In most cases, the time scale is somewhere in between.

(2) It is impossible to predict just where an area of mathematics will be useful. Even the originators of many mathematical ideas are often surprised by their application. The only thing we can state with certainty is that time plays tricks on anyone who claims, "There will never be any practical use for ______." The great British mathematician G. H. Hardy, for example, wrote in his autobiography, *A Mathematician's Apology*, that he practiced mathematics for its beauty, not for its practical value. He stated confidently that he saw no application whatsoever for number theory or for relativity. Only forty years later, abstract number theory has implications for national security; the properties of prime numbers are the basis for a new scheme for making secret codes. As for relativity, Hardy was disproved within just a few years—by the invention of fission and fusion devices.

It may be surprising that the most abstract subfields of mathematics—geometry, number theory, logic—have great practical importance. Computer scientist D. E. Knuth reports, "Every bit of mathematics I know has helped in some application one way or another."

Physicist Eugene Wigner marvelled at "the unreasonable effectiveness of mathematics, in the natural sciences." Surely it has something to do with the mathematician's penchant for distilling away all but a crucial aspect of a problem, for finding the common point of view from which two seemingly different problems turn out to be closely related. But this does not adequately explain why, time after time, abstract mathematics developed for its own beauty turns out, decades later, to describe nature perfectly.

Harvard mathematician Andrew Gleason has his own answer: "Mathematics is the science of order—its object is to find, describe and understand the order that underlies apparently complex situations. The principal tools of mathematics are concepts which enable us to describe this order. Precisely because mathematicians have been searching for centuries for the most efficient concepts for describing obscure instances of order, their tools are applicable to the outside world; for the real world is the very epitome of a complex situation in which there is a great deal of order."

We propose an additional reason. Mathematical ideas do not spring full-grown from the minds of researchers. History illustrates that mathematics often takes its inspiration from patterns in nature. Lessons distilled from one encounter with nature continue to serve us well when we explore other natural phenomena.

Whatever the reasons for the importance of mathematics to society, understanding how mathematics develops has crucial implications. One must assess how best to nurture excellent mathematics in this country and how to retain the world leadership gained over the past forty years. We believe in two basic principles:

Mathematical research should be as broad and as original as possible, with very long-range goals. We expect history to repeat itself; we expect that the most profound and useful future applications of mathematics cannot be predicted today, since they will arise from mathematics yet to be discovered.

While most mathematical research will be directed towards understanding known problems, we must remember that the direction of mathematics itself is constantly changing. Talented mathematicians should be encouraged to pursue research whose relevance we understand only partially or not at all, but which may ultimately result in new points of view, or in the invention of new areas of mathematics.

We have experienced a golden age of mathematics during the past forty years. Practically every subfield of mathematics has turned out, as if by magic, to be related to every other subfield, and to many applications in the natural sciences and engineering. This seamless web is not only breath-taking, it makes it impossible to be encyclopedic in describing recent mathematical research and application, confounding any overly simple organizational scheme.

Our choice of examples below is necessarily idiosyncratic, governed by our own familiarity and taste. We have organized them loosely into four areas—computation, physics, communication, and engineering—although the topics spill freely over these neat boundaries. We are aware that we are neglecting many important areas and developments. In spite of these omissions, we trust that our examples adequately reveal the nature of the mathematics as a whole.

Before we turn to these applications, we want to tell the story of a single topic in mathematics—Fourier analysis—and how it has developed in 170 years. The story illustrates how mathematics often turns out to be vastly more important than the particular problem it is invented to solve.

Fourier Analysis

In the early 1800s, Jean Baptiste Joseph Fourier, newly returned from his post as civil governor of Napoleonic Egypt, set out to understand the problem of heat conduction. Given the initial temperature at all points of a region, he asked, how will heat diffuse over the course of time? It was curiosity about such phenomena as atmospheric temperature and climate that led Fourier to pose the abstract question.

In order to solve the heat diffusion equation, Fourier devised a simple—but brilliant—mathematical technique. This equation turned out to be easy to solve if the initial heat distribution were oscillatory—that is, essentially a sine wave. To take advantage of this, Fourier proposed decomposing any initial heat distribution into a (possibly infinite) sum of sine waves and then solving each of these simpler problems. The solution to the general problem could then be obtained by adding up the solutions for each of the oscillatory components, called harmonics.

French mathematicians, such as Lagrange, sharply rejected the idea, doubting that these simple harmonics could adequately express all possible functions, and casting aspersions upon Fourier's rigor. These attacks dogged Fourier for two decades, during which he carried his research forward with remarkable insight. Today we owe an enormous debt to his remarkable tenacity, his stubbornness, and his ability to proceed in spite of formidable doubts in the minds of the leaders of the scientific establishment. Fourier found it difficult to publish his work even after he received the 1811 grand prize in mathematics from the Académie des Sciences for his essay on the problem of heat conduction, because the academy's announcement of the award expressed grave reservations concerning the generality and rigor of Fourier's method. Fourier persevered and finally his work won general acceptance with the publication of his now-classic *The Analytic Theory of Heat*, in 1822.

The method of harmonic analysis, or Fourier analysis, has turned out to be tremendously important in virtually every area of mathematics and physical science—much more important than the solution of the problem of heat diffusion. In mathematics, it has become a subject by itself. But in addition the theories of differential equations, of group theory, of probability, of statistics, of geometry, of number theory, to mention a few, all use Fourier's technique for decomposing functions into their fundamental frequencies.

In physics, engineering, and computer science the effect has been no less profound. Fourier himself presaged the impact of his technique in his introduction to *The Analytic Theory of Heat*: "Profound study of nature is the most fertile course of mathematical discoveries... . It is a sure method of...discovering...the fundamental elements which are reproduced in all natural effects." In effect, Fourier provided one of the most powerful tools for mathematical physics. Once Maxwell described electromagnetic waves with his famous equations in 1873, Fourier analysis became one of the key methods for studying these waves and their harmonic components—X-rays, visible light, microwaves, radiowaves, etc. Many electrical and electronic devices are now based on Fourier analysis, including such recent ones as nuclear magnetic resonance spectrometers and X-ray crystallographic spectrometers. In this century, Fourier analysis has provided the basic understanding of quantum theory—and hence of all modern chemistry and physics.

The idea of decomposing data into periodic components has also been central in engineering. It led to the Laplace transform, taught to every engineering student as the standard method of studying linear differential equations. Fourier analysis also led to time-series analysis, which is used in oil exploration for interpreting seismic waves shot through rocks suspected of bearing petroleum.

The advent of the computer has more recently made it possible to perform Fourier analysis numerically as a routine part of data analysis. The ability to decompose sound into its harmonic components has allowed computers to generate and recognize human speech. Performing similar operations on photographs—for example, satellite pictures of regions of the Earth—allows a computer to filter out "noise" and thus sharpen or enhance the image.

Even mundane business, like the multiplication of two numbers, can be accomplished much faster by using Fourier transforms rather than the time-honored method taught in grade school. The idea is to consider the digits of the numbers as a function, which can then be expanded into a Fourier series. For 1000-digit numbers, the Fourier method may be as much as 50 times faster than the more familiar algorithm, and of course it is used in computer design.

The yeoman's duty performed by the Fourier transform is possible only because of clever methods that mathematicians discovered in order to compute the Fourier transform of a sequence of numbers—algorithms which are collectively called Fast Fourier Transforms (FTT). The idea for these is found in the work of Runge and König in 1924, although the germ of the method probably dates to Gauss's work a century earlier. The FFT became widely known and used after Cooley and Tukey's paper in 1965, and various modifications have been proposed by Garwin, Rudnick, Good, Winograd, and others.

The direct computation of the Fourier transform of n numbers requires about n^2 operations. The FFT makes it possible to find the answer in approximately $n \log n$ steps—a tremendous improvement for large values of n. Without this improvement, computers could never analyze many problems in "real time"—that is, produce answers at the same rate the data are flowing in and hence avoid bottlenecks. (Determining the exact amount of time it takes to perform the FFT turns out to be a difficult problem, which hinges on some profound theorems from analytic number theory about the distribution of prime numbers.)

At least as important as the numerous applications to science and engineering has been the application of Fourier analysis to mathematics itself. Like other scientists, mathematicians are constantly searching for new tools to solve their theoretical problems. Frequently it happens that techniques discovered to solve one abstract problem later apply to a wide variety of others.

If you need to be convinced of this, look under "Fourier" in the card catalogue of a university science library. At Harvard's, for example, there are 212 entries, of which the first ten are Fourier analysis in probability theory, Fourier analysis in several complex variables, Fourier analysis of time series, Fourier analysis of unbounded measures on locally compact abelian groups, Fourier analysis on groups and partial wave analysis, Fourier analysis of local fields, Fourier analysis of matrix spaces, Fourier coefficients of automorphic forms, the fourier integral and its applications, and fourier integral operators and partial differential equations.

Dirichlet and Riemann series in the last century were inspired by Fourier series, and they eventually led to the L-series studied today. These ideas have unified number theory with the theory of group representations. Fourier analysis has led to the definition of function spaces (such as Sobolev spaces, Schwartz spaces, distribution spaces, and Hardy spaces) which form the basis of modern functional analysis. In this framework we can analyze differential equations (both linear and nonlinear) and their modern generalization—pseudodifferential equations—and Fourier integral operators. One can study the nature and propagation of singularities by these methods.

Although Fourier realized that his method was important—so important that he perservered for two decades in the face of intense criticism—he never realized just how fruitful his invention would be. While not every new development in mathematics has had the spectacular influence of Fourier analysis, the basic pattern has been much the same: the impact of good mathematical ideas spreads far, and in unexpected directions.

2. COMPUTATION

Perhaps the most striking mathematical application of this century has been the development of the electronic computer. It has become a fixture in offices, schools, and factories and is fast becoming commonplace in the home as well. The point of this section is to show why the computer revolution is not simply an engineering revolution. It is a mathematical revolution, for the ideas central to the invention and everyday use of the computer are sophisticated mathematics.

Computers suffer from two fundamental limitations. Although the fastest computers can execute millions of operations in one second, they are always too slow. This may seem like a paradox, but the heart of the matter is: the bigger and better computers become, the larger are the problems scientists and engineers want to solve. The reach exceeds the grasp. When you double the amount of data in a problem, the number of steps needed to compute the solution often increases four-fold, or eight-fold, or sixteen-fold. In most applications, computing time is the most serious limitation on the frontiers of the possible. Doubling computer speeds every few years only means increasing their ability to solve larger problems by 10%–20%, often not even that. It takes an entirely new mathematical approach to enable a computer to get close to doing the needed arithmetic.

The second limitation of computers stems from their digital nature, since much of the mathematics that underlies science is continuous. Approximating the solution to a continuous problem with a discrete machine takes great skill. Most scientific calculations depend on the answers to such questions as: what mathematical methods lie behind a proposed numerical solution? Will errors produced because the computer deals with numbers of a fixed length (rounding-off errors) compound themselves to swamp the answer? Will other approximations yield such errors? If not, how long does a digital computation take to obtain a desired degree of accuracy? Again the mathematician must continually develop new points of view in order to improve the way the computer handles a computation.

Mathematics is thus at the very heart of computation. But let us tell the story of mathematics and the computer from the beginning.

The Computer Itself

Nowadays it is easy to forget that the notion of an all-purpose computer is a very recent one. Until the last fifty years, a computing machine meant a machine designed to perform some particular piece of arithmetic. The Arabic mathematician Al-Kashi built one in the 15th century for computing lunar eclipses, and he built another for figuring the position of the planets. William Schikarf, Blaise Pascal, and William Leibniz all fabricated machines for automatic addition and subtraction; Charles Babbage was famous for his Analytical Engine. To compute the area under a plane curve, J. H. Hermann, James Clerk Maxwell, and James Thompson each devised planimeters, another sort of analog computer. However, the notion of a single, universal machine suitable for all problems and all calculations came from seemingly the least likely source—abstruse mathematical logic.

Logic and the Computer

The foundations of mathematics rest on the foundations of logic. For centuries, mathematicians believed that deductive reasoning could never lead to inconsistent results. This conventional wisdom was called into doubt in 1903 by the famous paradoxes of Bertrand Russell, and Alfred North Whitehead. For example, let S be the set of all sets which do not contain themselves. Does S contain itself?

About 1915, David Hilbert also embarked on a program to restore the foundations of mathematics. In 1927, John von Neumann, a young co-worker of Hilbert, published a famous paper conjecturing that mathematical logic would soon be proved free from possible contradiction. Yet only three years later, Kurt Gödel proved that even simple arithmetic contains "undecidable propositions," sentences whose truth or falsity cannot be proven. His method also demonstrates that a proof of the logical consistency of mathematics is impossible. The answers to this seemingly esoteric question turned out to have tremendous practical ramifications.

In 1936, Alan Turing and Emil Post realized independently that this question is equivalent to asking which sorts of sequences of 1's and 0's can be recognized by an abstract machine with a finite set of instructions; they envisaged such an automaton as a simple black box with a single long tape for writing and reading a single symbol. Turing and Post proved a surprising theorem about automata: in principle, there must exist a "universal automaton" capable of recognizing any sequence recognizable by any other automaton. That is, this universal machine could—with a finite sequence of instructions—imitate any other special purpose machine.

This was really the birth of the universal computer. The logical ideas were pursued further by Church, Kleene, and others. But it was the great mathematician John von Neumann who realized how to implement the universal automaton as an electronic computer with stored instructions—a "program" which the machine itself could alter in the course of calculation. Von Neumann and his colleagues then undertook the monumental technical task necessary to make the theoretical a reality. Within a decade, devices like von Neumann's ENIAC, built at the Institute for Advanced Study in Princeton, were operating. At no point in the early years of the century would anyone have guessed where the esoteric debate on the foundations of mathematical logic would eventually lead.

Algorithms and Computational Complexity

We have already alluded to one of the central problems of computing: as the size of computational problems grows, the time and space needed to solve them grows far more rapidly. In the earliest days of computing, a mathematician would check the correctness and speed of a program by testing it on various different inputs, noting the time and space required. The drawbacks to this rough-and-ready method are obvious. To avoid them, mathematicians built upon the work of Turing and Post in devising theoretical models of computation to test how many operations an algorithm required. They realized that rather than trying to solve each new problem with specific, ad hoc tricks, it would be better to devise a collection of basic mathematical methods which could act as the building blocks for many algorithms.

Consider, as an example, a common task which a computer might perform many times: given n numbers $a_1, a_2, \ldots, a_n$, write them in ascending order. The simplest procedure is

(1) Write down a_1.

(2) Check if a_2 is less than a_1; if so, write it to the left of a_1. Otherwise, write a_2 to the right of a_1.

(3) Check if a_3 is less than the smallest number written down so far. If so, write it to the left. Otherwise compare it with the next number,

writing it on the left of this number if it is smaller and on the right if it is bigger.

(4) Continue for $a_4, a_5, \ldots, a_n$.

A good measure of the time it takes to perform this algorithm is the number of comparisons which must be done. It might take as many as $\frac{1}{2}n^2$ if the numbers had originally been in descending order. This is called the worst-case complexity of the algorithm. Starting from a randomly chosen order, one would expect acout $\frac{1}{4}n^2$ comparisons to be necessary, which is the average-case complexity. That the time needed grows with n^2 is the real limitation on how large a list can be effectively sorted. The particular coefficient $\frac{1}{2}$ or $\frac{1}{4}$ is usually neglected and computer scientists write that this algorithm takes time $O(n^2)$ to indicate that the time is on the order of n^2 steps.

We should remark that worst-case and average-case complexity of an algorithm can differ markedly. The well-known simplex algorithm for linear programming can require time exponential in the size of the problems in the worst case. However, these worst cases are few and far between; Borgwardt and Smale proved in 1982, for a variant of this problem, that on average, the algorithm requires only time quadratic in the size.

In fact, there is a much faster way to sort numbers, based on the principle of recursion: Divide the numbers into two equal groups; sort each group; then combine these two sorted lists of $\frac{1}{2}n$ numbers. To sort each of the groups of $\frac{1}{2}n$ numbers, use the same procedure: sort them into two groups of $\frac{1}{4}n$ numbers, sort them and merge the lists. Each of the groups of size $\frac{1}{4}n$ is sorted by dividing it into groups of size $\frac{1}{8}n$ and so on. This recursive process takes time $O(n \log n)$, and so runs 64 times faster than the previous method when sorting 256 numbers.

The principle of recursion applies to many others problems at well. Computers are constantly multiplying large matrices—for example, in performing statistical data analysis. The standard high-school method for multiplying $n \times n$ matrices requires time $O(n^3)$. However, there is a trick for multiplying 2×2 matrices which takes only 7 multiplications instead of 8. By recursively breaking large matrices into smaller and smaller pieces, this advantage applies to all the little problems and leads to a time $O(n^{2.83})$ algorithm for matrix multiplication.

In addition to recursion, there are many other useful methods to organize computation, such as "data structures." For instance, all the examples we mentioned in the introduction to this article—the Kalman-Bucy filter, image sharpening, CT scanners, linear programming, and medical statistics—depend on using a computer to solve a system of n linear equations in n variables. Consequently, a great deal of attention has been given to such algorithms. The classical method of Gaussian elimination requires time $O(n^3)$. However, in many important problems—most prominently the finite element methods for solving differential equations or certain eigenvalue problems—necessary in computer simulations of weather, space flight, industrial design, etc., the coefficients in the equations include many zeroes, distributed in a regular pattern. Mathematicians have exploited this structure to obtain a faster algorithm. The pattern can be turned into a graph (a structure consisting of points and edges connecting them), and the graph can in turn be used to devise a very efficient order for performing Gaussian elimination. The result is an algorithm that needs only time $O(n^{3/2})$—a major savings in a problem that must be performed thousands of times. Recently, it was shown that the method also generalizes to yield a time $O(n^{3/2})$ algorithm for matrices whose graphs can be drawn in a plane.

While many algorithms rely on rather elementary mathematical concepts, three recent algorithms exploit deep theorems from very different branches of mathematics to crack difficult computational problems. Some rather esoteric results have turned out to have quite practical computational consequences in testing for primes, graph recognition, and integer programming.

Large prime numbers form the basis for a new encryption scheme. But until recently, testing whether or not a 60-digit number is prime has been beyond the scope of even the fastest computer. The most straightforward test—checking all integers up to the square root of the number in question to see whether they are divisors—requires checking 10^{30} numbers. (Here 10^{30} is scientific notation for 1 followed by thirty 0's.) Number theorists, however, have long been studying the properties of prime numbers. Many of the laws they discovered—such as the so-called higher reciprocity laws—have recently been combined in a new algorithm for primality testing which makes it practical to check even 100-digit numbers.

Another frequent problem in computing is to decide whether two seemingly different n-point graphs are in fact isomorphic—that is, have the same pattern of connections. Until recently, we could only determine this through trial and error. However, a series of algorithms aims to discover symmetries between the two graphs, using a recent triumph of algebra, the classification of finite simple groups. The new algorithms based on symmetry run much faster than trial and error, although still better ways are being sought.

Another computer problem important in industry, integer programming, allows a company to optimize scheduling or use of materials. In the past few years, 19th-century techniques for studying lattices in algebraic number fields have been applied to integer programming and have led to new algorithms. (In addition, these same lattice techniques have provided the fastest algorithms for factoring polynomials.)

Aside from seeking faster algorithms for solving particular problems, mathematicians have begun to ask deep questions such as, "What are the absolute lower limits to how fast a problem may be solved?" and "Are some problems inherently intractable?" By building models of computation based upon Turing machines, they have begun to obtain preliminary answers. One topic of interest shows that certain questions, called NP-complete problems, cannot be solved in a polynomial amount of time. It may seem a rather negative aim to work to prove that a class of problems will remain computationally intractable. But such a proof would elucidate precisely what makes a calculation tractable, making it easier to find algorithms for the tractable problems.

Randomness in Calculation

One of the most stunning mathematical discoveries about computation is that relying on chance—playing the odds, so to speak—can be far more effective than following any known predetermined algorithm.

The classical example is the Monte Carlo method, developed in the 1940s. For instance, to compute the area of a dart board mounted on a 100 square foot wall, throw 500 darts at random toward the wall. Assuming 15 darts land on the board, its area is roughly 15/500 the area of the wall, or 3 square feet. More generally, to compute the volume of a region R inside a box B, pick n points at random in B. A good estimate for the ratio of the volume of R to that of B is the fraction of the n points which lie in R. In fact the error in the method will tend to zero as more points are taken, and the rate of convergence is proportional to $n^{-1/2}$.

For complicated shapes and/or high dimensions the Monte Carlo method is extremely efficient. It has become a conventional numerical method to evaluate multidimensional integrals and standardizes the integration of functions that would otherwise be impossible. Even with the Monte Carlo method, some desired calculations by far exceed the capabilities of existing computers. Computer architects are investigating how to effectively link many parallel processing units, either for a general purpose computer or dedicated to one particular calculation.

Randomness in Algorithms

The examples above illustrate an application of randomness in a continuum setting. Recently, randomness has also proven very useful in studying algebraic problems. Here a random method yields exactly the right answer—except occasionally, when it gives the wrong one. Every such method depends on the fundamental structural properties of abstract algebraic objects such as polynomial rings, number fields, and permutation groups.

How would such an algorithm be useful? An algorithm that is correct 100% of the time might require n^6 steps to execute, whereas an algorithm that produced the right answer only 99.9999999999% of the time might need a mere n steps to perform. The tremendous savings in time would more than offset the small change of error. In the last eight years, dozens of examples of such methods have been found.

One randomizing algorithm, which will vastly improve computer security at a tiny cost will soon be hard-wired into silicon chips. This algorithm makes it possible to "fingerprint" a computer file in order to prevent anyone from tampering with it. Suppose that a computer has 40 megabytes of data stored on a disk. Consider the bytes as the coefficients of a 40,000,000th degree polynomial. Divide this polynomial by a randomly chosen 13th degree polynomial and save the remainder of the division. Now write down the coefficients of your randomly chosen polynomial and of the remainder and put them in your wallet. They are a "fingerprint" for the file. The chance that an interloper could change the data without altering this random fingerprint is less than 1 in 100,000,000,000,000,000,000, or 10^{-20} in scientific notation.

Primality testing, a problem we mentioned above, turns out to be very easy—if a tiny chance of error is allowed. If an integer n is not prime, then at least 3/4 of the numbers between 1 and n have a particular property S which can be checked very quickly. If n is prime, no such numbers have property S. The test is simple: pick fifty numbers and chcek whether they have property S. If any do, then n cannot be prime. If not, then n is almost certainly prime—for, if n were not prime, the chance of picking fifty numbers at random without property S would be at most about $(1/4)^{50}$ (or 10^{-30}). If those odds are not good enough, try another fifty numbers; it takes only a second or two, and the chance of error goes down ever faster.

Computer Assisted Proofs

Mathematicians have used the computer as a scientific laboratory to test ideas and to develop precise conjectures, based on numerical evidence. This has lately been fruitful in the study of maps of the interval and more generally in the field of dynamical systems. Similar use occurs in number theory, algebraic geometry, topology, complex analysis, and the study of quasiperiodic potentials. Patterns emerge from extremely accurate computation. In some cases, detailed calculations even indicate when a function may have cusps or discontinuities and hence provide the basis for mathematical conjectures.

However, a fascinating new possibility has recently arisen: in certain cases the computer can act as a partner to the mathematician, rather than as his laboratory, in establishing a traditional mathematical proof. The computer can perform either an algebraic, combinatoric, or analytic task. For the former, as in the analysis of the four color problem, the computer checks that a certain finite number of cases of a combinatoric statement hold—by checking them one after another. The reduction of the theorem to the combinatoric statement remains the job of the mathematician.

The computer can also verify inequalities necessary for the proof of a theorem. This can be done with 100% accuracy. The mathematicians might have the computer check whether a particular number x lies with certainty within the interval $[a, b]$. Establishing the inequality $x < y$ reduces to establishing that the upper bound for the x-interval is less than the lower bound for the y-interval. The interval arithmetic can be done with certainty by reducing each calculation to integer arithmetic.

This technique has been used with success in studying iterations of maps of the interval and is an important ingredient in the recent proof of the existence of a fixed point for iteration of a quadratic map. The computer can check a large number of estimates, each of which can be established by hand, but which, as a whole, pose a problem of scale.

Both these examples are a departure from tradition which may well become increasingly important. At this stage we do not predict that computers will replace the thinking mathematician in outlining the architecture of a proof. But in some cases they will surely provide an aid to the mathematics which goes beyond the experimental laboratory or mathematical model; they will complement the mathematician's ability by establishing a large number of identities or inequalities within a mathematical proof.

Numerical Analysis and Mathematical Modeling

Early numerical analysis can be traced to Newton (17th century) and Euler (18th century). However, discrete mathematics developed rapidly with the advent of the computer. In the period after 1950, the inversion of sparse matrices and numerical integration were widely studied, as were methods to integrate ordinary and partial differential equations. These advances made possible engineering design in a broad range of problems. Today most advanced technological design and development—from cars and aircraft to petroleum engineering and satellites—is based on computer simulation. In addition, real-time calculations led to the triumphs of space exploration and automatic rocket control.

Computations, however, do not run on automatic pilot. The importance of the intelligent mind cannot be overestimated. At one side of numerical modeling lie the relevant physical laws and mathematical equations which describe a particular engineering process. At the other side are numerical algorithms and codes (programs) used to instruct the computer. Connecting these two domains involves the mathematics of discrete approximations, and a mathematical understanding of the structure of the equations and of the nonlinear phenomena which they describe. Here finite difference and finite element methods have played a central role. With the advent of vector and parallel computing, problems long thought inaccessible are becoming more tractable. Despite this recent progress, numerical analysis of nonlinear partial differential equations in three dimensions, as well as many other frontier questions, still await new mathematical methods.

New mathematics can also make the difference in two important questions of speed. Can calculations be performed fast enough to be useful? In many engineering questions, an overnight turn-around is essential. Secondly, new mathematics can resolve whether a calculation is only feasible, or whether it can be performed in the real time necessary for a practical purpose like landing an aircraft.

Numerical mathematics plays a critical role in three remarkable developments: the replacement of experiments by computer simulation, decision science, and signal and data processing. Computing can be cheaper than experimenting. It is much easier to modify experimental design in a computer study then in an actual physical experiment. In some projects experimentation is dangerous or impossible.

In aerodynamics, the design of aircraft, of turbines, and of compressors is done with computer assistance. The ability to calculate aerodynamics forces on the space shuttle was an absolute necessity for the operation of the flight simulator in which the pilots of the space shuttle were trained.

Some other applications of numerical fluid dynamics are: the design of naval vessels, the calculation of combustion patterns, the flow of a mixture of oil and water (or other chemicals) in enhanced oil recovery, multiphase flow in reactors under transient conditions, the flow of ground water through crushed rock, and the propagation of sound signals through geological layers, etc. Nuclear fusion reactor design relies on mathematical modeling: the plasmas at the densities that we wish to create are not yet available on earth; they exist only as mathematical models. The same is true of the development of laser fusion.

Operations research, an area of decision science relying on mathematical manipulation of stored data, has helped enormously to streamline large scale operations and insure the optimal use of resources. From its first industrial applications to the scheduling of petroleum refineries in the early 1950s, linear programming has resulted in substantial gains in the efficiency of the operations it was used to analyze. Topology, convex analysis, combinatorics, and geometry all contributed to the development of the mathematical model.

Digitally stored information can be manipulated mathematically to extract particular details hidden in a mass of data. Here the mathematics of statistics enters to point up hidden trends and correlations. In medical science, physiological modeling opens up possibilities for understanding the biological functions of organs as well as the design of effective prosthetic devices. Perhaps the most spectacular success of medical computing is its use in computerized X-ray tomography, as well as other advances in noninvasive diagnosis which make use of ultrasonics, nuclear magnetic resonance, and positron emission tomography.

The full impact of the computer will develop over coming decades. As with the invention of the steam engine, the computer has an enormous potential to enhance our lives. Initiatives currently being discussed for supercomputers and fifth generation computers (aimed at artificial intelligence, pattern recognition, knowledge processing, etc., as well as scientific computing) can only partially succeed unless such hardware is complemented by new mathematical points of view.

The architecture of the present computer is sequential; it is called the von Neumann machine after the man who showed how to implement this idea of programming. In the coming revolution of computing hardware, organization will be parallel rather than sequential. It will be designed much more like the slower but far more complex parallel structure of the brain and will certainly require a new conceptual approach to harness and to program. For all its uses, the computer depends critically on ideas, insights, and methods from mathematics.

3. MATHEMATICAL PHYSICS

Modern theoretical physics provides the most striking example of the way mathematics and science complement one another. Whole fields of mathematics have been born from attempts to understand the laws of physics. Reciprocally, mathematics has provided the language of physics. In overview one sees parallel developments in these two subjects. We describe here a few of the triumphs of mathematical physics.

Group Theory and Quantum Mechanics

The theory of group representations has had far-reaching impact on quantum theory and more generally on understanding the symmetries of nature. An object is said to have a symmetry if it is essentially unchanged by some transformation. An equilateral triangle, for example, is unchanged if it is rotated about its center by 120 degrees. Group theory is the study of transformations preserving an object, rather the object itself.

In the late 18th century, Legendre observed the significance of (the group of) root-interchanges for understanding third and fourth degree equations. However, many years passed before 1832, when Galois perceived the importance of studying the general structure of root interchanges for all polynomial equations. The discovery of the Galois group is generally taken as the birth of group theory.

Once the abstract notion of a group had been formalized, several major developments occurred. Felix Klein recognized that groups were a natural tool in the study of geometric symmetry. About the same time, Sophus Lie discovered the connection between groups and the theory of differential equations. Meanwhile the theory of group representations began with work of Frobenius, E. Cartan, and others who related the abstract theory of groups to concrete matrix groups; the matrices were said to "represent" the group.

Remember, however, that over the period of 100 years during which the theory of groups was invented and developed, physicists and experimental scientists virtually ignored it. Yet the theory of groups was marvelously suited to physics, both to classical physics and even more centrally to quantum theory.

In classical mechanics Noether developed the general connection between symmetry groups and conservations laws of classical mechanics, such as conservation of energy or conservation of angular momentum. In quantum theory the developments were even more striking. Only a short time passed between the discovery of electron spin in 1925 and the work of E. Wigner and H. Weyl to interpret spin as an aspect of group theory. The question was how to explain the existence of two states of the electron—spin up and spin down—which in turn explained the splitting of spectral lines of light occurring when one placed light-emitting atoms into a magnetic field. The answer lay in understanding a group called $SU(2)$, which is related naturally to the group of rotational symmetries of the three-dimensional space in which we live. The two spin states of the electron are interpreted as elements of a "fundamental representation" of $SU(2)$.

After that discovery, group theory became a central tool in physics and chemistry. For instance, the classification of emission and absorption spectra from atoms and molecules (including the qualitative and quantitative analysis of atomic and molecular spectroscopy) boiled down to the study of representations of the permutation group and the group $SU(2)$. The Coulomb potential is unchanged by rotations in three-dimensional space. The group representations precisely describe how a physical state fails to share the full rotational symmetry of the underlying force. Finite dimensional subgroups of rotational symmetries (point groups) describe crystal symmetries and many other symmetries of condensed matter physics and chemistry.

In 1926 Pauli discovered another symmetry of the Coulomb force, different from the rotational symmetry discussed above. This symmetry arose from an understanding of Lie's theory applied to the "eccentricity" of a classical elliptical orbit. (This eccentricity invariant of motion, studied by Laplace, Runge, and Lenz, describes how much the ellipse differs from a circle.) The properties of this extra symmetry led Pauli to a simple, elegant picture of the quantum mechanical hydrogen atom. More important, this work foreshadowed the idea of studying space-time symmetry in conjunction with other types of symmetry.

A great step foward came in 1939 when Wiguer analyzed the positive energy representations of special relativity, i.e., representations of the group of Einstein, Lorentz, and Poincaré. The mathematical tools of the day required generalization to solve this problem, and Wigner built on the classical work of Frobenius. One consequence of his analysis is that every representation of the relativity group was characterized by two intrinsic numbers, its "mass" and its "spin." In this way, both mass and spin derive from a fundamental symmetry, namely special relativity. After that discovery, a physical particle in quantum theory could be interpreted as a mathematical object, namely as a group representation.

Staggering development ensued in the mathematical theory of Lie groups and their representations. In turn, these purely mathematical developments came to play a central role in modern number theory, geometry, ergodic theory, etc. This work is ongoing, not only for Lie groups but also for infinite dimensional groups, such as groups of diffeomorphisms, the Weyl group, and gauge groups.

Such a theory of representations later played a role in revolutionizing ideas in physics. At first, group theory was applied to describe laws of nature. Later a modern point of view evolved in which groups actually became part of the statement of the laws. It was not long before physicists accepted as fundamental certain symmetries of nature other than space-time symmetries. Nuclear forces do not depend on the electric charges of the particles involved. Heisenberg described this independence as the symmetry of the nuclear force under a symmetry which relates protons and neutrons. He called this "isotopic spin" and regarded the proton and the neutron as two different states of one particle, the nucleon. These two states are characterized by a two-dimensional representation of $SU(2)$.

Not only has this point of view become standard, it has been considerably extended. Physicists believed that the fundamental laws of nature were invariant under certain reflection symmetries. The mirror reflection symmetry in three-dimensional space had been taken for granted until the late fifties, when Lee and Yang pointed out that it should be tested. They proposed experiments which demonstrated that parity was not an exact symmetry of nature! After this breakthrough, the question of which apparent discrete symmetries are found in nature has been in the forefront of physics.

Why would an expected symmetry not occur in nature? Although symmetric equations may have a simple structure, the symmetry may not apply to nature. On the other hand there is a more elegant possibility: the laws themselves have a symmetry, but the particular solution of interest does not possess it. For example, Newton's gravitational potential is rotationally symmetric, but a classical planetary orbit is not necessarily circular—it can be elliptical or hyperbolic. One expects, however, that the lowest energy state of a system would possess all the symmetry of the laws themselves. If it does not, then the symmetry is said to be "broken."

Broken symmetry appeared first in the physics of magnetization and in the chemistry of phase transitions (such as boiling or freezing of water). Broken symmetry occurs in a model introduced by Lenz and Ising and studied in famous work of Peierls in 1936 and Onsager in 1944. It has become the standard mechanism to describe many effects in statistical physics. Physicists do not know whether parity violation can be described as broken symmetry, but in fact broken symmetry has achieved a crucial role in the description of other aspects of particles.

Starting in the 1950s large accelerators produced dozens of new particles. Soon it became necessary, or at least very desirable, to explain them coherently. How could one explain the masses, the spins, and the other intrinsic properties of these particles, as well as their interactions with one another? The physicists again turned to symmetry to simplify the problem.

Mathematicians for reasons internal to their field had been developing the abstract theory of representations of compact groups. As it turned out, this understanding of group representations provided exactly the information required in the search for the laws of nature. In 1961, Gell-Mann and Neeman proposed extending the proton-neutron symmetry of Heisenberg to the larger group $SU(3)$. Besides explaining the charge independence of nuclear force and neatly classifying the many new particles by their properties, they added a startling new observation. The representations corresponding to the familiar observable particles (protons, neutrons, mesons, etc.) could all be constructed from products of two fundamental representations. Each component of the fundamental representations was dubbed a "quark."

This mathematical picture of fundamental particles composed of quarks predicted the existence and mass of a new particle called the omega, which also had to possess a quality called "strangeness." After the omega particle was found in 1964, the $SU(3)$ symmetry became accepted, as well as the notion that the unseen quark was a fundamental component of the laws of nature.

These twenty-five years made it clear that groups and their representations were as essential to modern particle physics as the more traditional tools of complex analysis and partial differential equations. It is tempting to speculate on the relation of some of the newest advances of group theory to physics. Two beautiful mathematical theories are the classification of finite groups (related to the recent discovery of a "monster" group), and supersymmetry. Only time will reveal whether nature embraces these mathematical notions in some subtle way.

Differential Geometry and Physics

The early history of differential geometry dates to the 17th-century work of Fermat on curves, and to Gauss who studied the curvature of surfaces in the 19th century. Gauss's point of view could be called the concrete approach, since he studied surfaces embedded in a Euclidean space of higher dimension. Riemann formulated the geometry of surfaces as entities in their own right in 1854. Eventually geometry incorporated the algebraic notions of symmetry and groups. What emerged was tensor analysis, a subject founded by Bianchi, Levi-Civita, Christoffel, Ricci, and others.

Einstein embraced this intellectual framework to explain his fundamental ideas about gravity, proposing his General Theory of Relativity in 1915. Einstein's basic equation sets the curvature of space proportional to the density of energy; the fundamental constant of proportionality is defined to be the gravitational constant. From this point of view, gravitational force results from the curvature of space. Relativity theory yields Newton's force law for gravitation as the limiting case of a space-time with small curvature.

The second fundamental force of classical physics is electromagnetism. In 1918 mathematician H. Weyl observed that the electromagnetic forces could necessarily be inferred from the geometry of space. He based his study on scale transformations of space; for this reason he called electromagnetism a "gauge theory."

This conceptual advance was not fully appreciated at the time, but the gauge picture ultimately led to our modern effort to unify the four fundamental forces: gravity, electromagnetism, strong forces, and weak forces. What startled physicists nearly forty years later was a simple but profound generalization of electrodynamics (as described by the basic equations of Maxwell dating from 1873, reinterpreted by Weyl, and also incorporating the equations of Dirac). In 1954, Yang and Mills suggested that the basic symmetry group of electromagnetism be enlarged to include a group describing the symmetry of strong forces. They considered the simplest equations which were compatible with this invariance, and which reduced to Maxwell's equations for purely electromagnetic forces. Today this subject is known as "nonabelian gauge theory," since the basic symmetry group is a noncommutative group. Here the choice of the particular group of symmetries is crucial for physics; it is an explicit example of the philosophy that discovering the symmetry group is a part of finding the laws of nature.

The notion of a Yang-Mills gauge theory was not at all new. Some years earlier, mathematicians had introduced the global geometric notion of a fibre bundle and had recast Riemann's geometry into fibre-bundle theory. A fibre bundle is a space consisting of many similar spaces pasted together. For example, a torus (doughnut) can be assembed by pasting together successive circular cross sections. Mathematicians introduced the notion of a "connection" as an object to measure the local twisting due to the curvature of such a space. An enormous theory was laid out, including the study of the topology (global properties) of abstract spaces with curvature. Many algebraic and geometric invariants, such as Chern numbers, Steifel-Whitney classes, index invariants of Atiyah, Singer, Hirzebruch, Weil, Bott, and others were discovered as part of the general theory. What the physicists had added to this picture was the notion to find such structures as solutions to a set of variational equations. These nonlinear differential equations which the connection satisfies are the natural generalization of Laplace's equation within the framework of differential geometry.

Let us take a brief technical excursion. In the original equations of Maxwell, the electric field $E(x,t)$ and the magnetic field $B(x,t)$ each are vectors—three component objects. Each component of these vectors is a real valued function on space-time. The modern view of a gauge theory is to consider the component functions $E_i(x,t)$ as elements of the Lie algebra of the gauge group. In the case of electromagnetism the group is $U(1)$ whose Lie algebra is just the real line, yielding ordinary real-valued functions $E_i(x,t)$ as in Maxwell's theory. Yang and Mills's generalization was to replace $U(1)$ by a larger compact matrix group whose Lie algebra consists of noncommuting matrices. Thus each component of the electric and magnetic field is a matrix, rather than a number. This matrix varies from point to point as one moves in space and time. While physicists are still not certain which group is the most fundamental one to choose, a typical candidate would be $SU(3)$ or $SU(2)$ or $U(1)$, which characterize, respectively, known symmetries of strong forces, weak forces, and electromagnetic forces.

Analysis and Quantum Fields

Physicists have believed for over sixty years that quantum theory provides the correct framework to describe fundamental, or submicroscopic, particles. Thus modern physics must find a mathematical theory that encompasses gauge theories, as well as quantum mechanics and special relativity. Such a combination is known as a quantum field theory.

Other examples of field equations are the Dirac equation for the electron, nonlinear scalar wave equations, and the Einstein equations for gravitation.

The challenging and elusive search for the mathematical foundation of quantum physics has inspired new mathematics and succeeded in yielding important insights into physics. At present it provides an exciting opportunity to unify these fundamental sciences.

As early as the 1920s when modern quantum mechanics was born, many of the world's greatest mathematicians, such as Hilbert, von Neumann, and Weyl, felt strongly attracted to this new physics. The mathematics of wave propagation, of integral equations, of differential equations, of eigenvalue problems, of linear analysis, of probability theory and of group theory, all contributed to the understanding of nonrelativistic quantum theory. These areas of mathematics also profoundly influence every area of modern physics and engineering.

Let us now focus attention on a puzzle in mathematical physics which arose in the 1930s with the attempt to incorporate into quantum theory the effects which arise because particles can affect themselves, indirectly, through their effects on other particles. For example, a typical experiment might measure the frequency (color spectrum) of light emitted by an excited atom. In the process the light interacts with the atom, while the atom in turn interacts with the light. Because of this circle of effects, the light can affect itself, and the phenomenon is said to be nonlinear. However, every attempt to derive observable frequency shifts from this nonlinear system resulted in infinite answers!

In 1947, a conference was held on Shelter Island to focus attention on the major open problems in theoretical physics and to reorient the researchers in the postwar period. This conference became famous because it stimulated the formulation and application of a set of rules to carry out the mystifying calculations; these rules systematically ignored meaningless quantities such as infinity or division by zero, i.e., 1/0. Yet they yielded definite answers.

The attention of physicists was directed toward understanding a small, recently observed correction to the spectrum of light emitted by hydrogen, today called the Lamb shift. A second effect of the nonlinear interaction concerned the magnitude of the energy of a single electron in a magnetic field. According to Dirac's theory, the value of this magnetic energy, or "magnetic moment," would exactly equal 2 in standard, dimensionless units. In fact the rules of quantum field theory predicted the number 2.002, and the deviation .002 from 2 was dubbed the "analogous" magnetic moment.

Over the last thirty-five years, countless man-years of labor have permitted the application of these rules to the first few terms of a power series in the electric charge. The computations involve the exact evaluation of thousands of integrals. The program is so immense that large computers were called upon just to carry out algebra. The result accurately predicts the magnetic moment of an electron. On the side of experiment, improvements over the years have yielded the present observational value of 2.002319304, one of the most precisely measured quantities in physics. The calculated number and the observation agree down to the last decimal place. Because of this extraordinary check between experiment and prediction, the rules underlying the calculation are taken seriously. In time, these rules for ignoring infinities became known in physics as the "theory of quantum electrodynamics."

Quantum theory's ability to explain the Lamb shift and the anomalous magnetic moment set the stage for a new era in quantum physics. The development of these ideas eventually led to the Yang-Mills theories described in the previous section. With twenty years' additional work, physicists came to choose the $SU(2) \times U(1)$ symmetry group to describe and unify two fundamental forces of nature: electromagnetic forces and weak forces. Glashow, Salam, and Weinberg received the 1979 Nobel prize for this work. In a more speculative proposal, an $SU(5)$ gauge theory also unifies the strong forces. This "grand unified" theory predicts that the proton is unstable—i.e., that it will eventually decay. So far this phenomenon has not been observed in the large experiments currently searching for such a decay. In any case, quantum field theory has become the accepted basis for quantum physics.

However, the work "theory" is not used here in the traditional sense, at least not according to the standards of scientific explanation common in physics before the era of quantum fields. Because of the infinite (or otherwise ill-defined) quantities that physicists initially ignored in formulating their rules, we must ask whether this method actually has a mathematically consistent formulation. In other words, "Can relativity in combination with quantum theory be incorporated as part of traditional mathematics?"

The rules for dealing with the infinite quantities described above are known in physics as "renormalization." Today, some forty years later, this problem is only partially understood. But mathematical progress has helped make quantum field theory approachable and has led to a formulation we believe will succeed. This formulation involves generalization of both the differential calculus and the integral calculus to the case in which the unknown functions depend on an infinite number of variables.

In the usual calculus, one differentiates and integrates functions f(x), where x is a point in a finite dimensional space. The generalization is to consider the mathematics of functions f(x) where the variable x has infinitely many coordinate directions. The subject in which one differentiates or integrates functions of an infinite number of coordinate directions is called the "functional calculus."

The differential functional calculus can be traced to the work of the famous Italian mathematician Vito Volterra, in his study early this century of general partial differential equations. Extensive developments of these ideas have continued ever since, and the field of functional analysis remains central for understanding physics.

In recent years the integral functional calculus has also played a major role. The original ideas in this field appeared in the theory of probability, as developed by Norbert Wiener. He abstracted the notion of integrals of functionals in an attempt to understand diffusion and Brownian motion. In this way, Wiener could represent the solution to the heat diffusion equation as an integral over classical particle trajectories. In physics a related point of view emerged from work of P. A. M. Dirac and R. Feynman in the 1940s, and today it is known in physics as the "sum over histories" approach to quantum theory. The connection between these two ideas has led to an understanding of how Wiener's integral fits into modern physics. It also opened up the mathematical development of "functional integrals," starting in the 1950s, by M. Kac, I. Gelfand, and many others.

The functional calculus eventually has been developed in recent times to the point that it could be used to tackle quantum field theory and the infinities of renormalization. In its modern form, this relatively new area of mathematics and physics has become known as "constructive field theory.".

Reunification of Mathematics with Physics

The above discussion clearly points toward an exciting development taking place right now, reunification of mathematics with theoretical physics. After the advent of quantum theory in the 1920s, mathematics and theoretical physics began to move apart. Perhaps physicists believed that it was impossible to give a complete explanation in the traditional framework, and still keep sight of the increasingly complicated set of physical phenomena. Mathematicians, on the other hand, found physics difficult to understand because the foundations were not treated properly, from their point of view. For whatever reason, each subject developed a special vocabulary, hard for the specialist in the other to penetrate. To make matters worse, study of one discipline by workers in the other was generally discouraged.

Past decades have seen internal unification revolutionize both these subjects. Mathematicians discovered deep relations between group theory, topology, algebraic geometry, differential geometry, analysis, and number theory. Meanwhile physicists discovered intimate connections between particle physics, condensed matter physics, and finally astrophysics. Twenty years ago a professor of mathematics and a professor of physics at the same university rarely had scientific contact. Today we sense excitement as the entire disciplines of mathematics and theoretical physics are coming together.

To illustrate this phenomenon, we here mention several examples of current work. Constructive field theory, developed by Glimm, Jaffe, and others, has resolved much of the fifty-year-old mystery of the foundations of field theory. A new area of mathematics has been created that provides a general framework, dictated by physics, within which one can answer the questions. A complete theory, including renormalization, has been constructed for several quantum field examples. The main reason that the present answers are incomplete is that these examples simplify the presumed equations of physics.

From the point of view of the integral functional calculus, quantum field theory can be regarded as the study of a probability distribution for classical fields. A generalization of probability theory emerges, with many new and challenging mathematical aspects. Similar mathematical problems arise in classical statistical physics. The relation between these subjects also explains at a fundamental level why phenomena known in statistical physics—phase transitions and symmetry breaking—should appear in quantum physics.

The heat kernel $exp(-tH)$ generates a random process labelled by time. Such random processes arise both in pure mathematics (geometry and topology as well as analysis), and in many fields of application, such as electrical engineering, stochastic control theory, and presumably econometrics and population biology. The random field (labelled by space as well as time) has the infinite dimensional Laplacian H as its generator. We suppose that its abstract theory and applications would be as rich as those of the random process indexed by time alone. For example, the functional integral methods—and related problems in stochastic differential equations—appear intimately related to representation theory for infinite dimensional groups, such as "loop groups."

Two ideas useful in mathematical study of renormalization are phase cell localization and the renormalization group; they have also had very fruitful applications to understanding phase transitions in physics. In mathematics, related notions appear in the theory of microlocal analysis and in harmonic analysis, such as Fefferman's study of the spectrum of the Laplacian using the Heisenberg "uncertainty principle." We expect that such relations will become clearer in time.

We have also mentioned the focus on geometric questions in physics. One recent puzzle in classical relativity theory was how to define the total "energy" of the universe. Physicists have proposed a definition of energy for spaces in which the curvature vanishes rapidly at infinity. The important positivity property of the energy (which in the quantum theory is also at the heart of the work on constructive field theory) was established two decades later, about 1980, by the geometers R. Schoen and St.-T. Yau. Their method to solve this physics problem developed the mathematical theory of minimal surfaces in a manner important to the ongoing study of singular harmonic maps and nonlinear differential equations.

In the last ten or fifteen years mathematicians and physicists realized that modern geometry is in fact the natural mathematical framework for gauge theory. The gauge potential of physics is the connection of mathematics. The gauge field is the mathematical curvature defined by the connection; certain "charges" in physics are the topological invariants studied by mathematicians.

While the mathematicians and physicists worked separately on similar ideas, they did not just duplicate each other's efforts. The mathematicians produced general, far-reaching theories and investigated their ramifications. Physicists worked out details of certain examples which turned out to describe nature beautifully and elegantly. When the two met again, the results were more powerful than either anticipated.

In mathematics, we now have a new motivation to use specific insights from the examples worked out by physicists. This signals a return to an ancient tradition. In physics, this understanding has focused attention on geometric questions. One aspect of the mathematical theory explains the observed quantization of magnetic flux in superconductors. Another well-studied example in gauge theory is the predicted existence of an elementary magnetic charge, or monopole, i.e., a magnet with a north or south pole, but not both. Some current experiments are searching for such a particle, but have not detected it.

In 1981, M. Freedman established the four-dimensional Poincaré conjecture, a problem unresolved for over sixty years. At that time one suspected that Freedman's topological classification of four-dimensional spaces would also carry over when one required some "smoothness" of the space. At least intuition says so. In fact S. Donaldson's recent theorem says that this expectation is false. The mathematical results on classical gauge theories motivated by physics' interpretation of the solutions, proved to be very important. As a corollary, it turns out that an "exotic" Euclidean 4-space exists. Many topologists are now studying gauge theories; physicists are now studying topology. It appears that rich new insights into the topology of four dimensions will result from this synthesis.

Another interesting development during the past few years is the study of "supersymmetry" algebras and the construction of supermanifolds. Mathematicians have known superalgebras as graded algebras, and one way to realize supersymmetry is using the standard De Rham complex. Physics introduced a new feature by constructing supergroups, supermanifolds, and superfields associated with these algebras. The Laplace operator (Hamiltonian) can be represented as a function of superfields. The Atiyah-Singer index theorem—a highlight of modern mathematics which unified ideas in topology, geometry, and analysis—can be proved using this method. Supersymmetry gives a new point of view on the index theorem and links it to the interactions (Lagrangians) of modern physics.

It is no surprise that this discovery has captured the imagination of both geometers and theoretical physicists. A related discovery is that "anomalies" of quantum physics—classical equations which fail in quantum theory—can be viewed as an aspect of K-theory, an abstract machine in modern topology and geometry. In fact K-theory even appears to be related to the spectrum of Schrödinger operators with quasi-periodic potentials. Such equations arise in describing magnetic properties of materials with random defects.

We are just scratching the surface of a new set of ideas whose natural setting embodies both mathematics and physics. We appear to be entering a new era where the boundaries between mathematics and theoretical physics practically disappear.

4. COMMUNICATION

As high-speed electronic communication becomes commonplace, there is a tremendous need for better transmission schemes—ones that minimize the effect of inevitable transmission errors, ones that protect confidential or secret messages, ones that route messages most efficiently. Many of the best schemes are based on patterns or properties of classical algebraic and geometric objects, originally studied for their intrinsic interest. Mathematically, these are the subjects of information theory, coding, and encryption.

Coding Theory: Protecting against Errors

Consider the difficult task faced by a Mariner spacecraft sending back to Earth intricate images of the Martian surface. The messages it beams back will necessarily be garbled by random noise and, unless some amount of redundancy is built into the messages, scientists at NASA won't know whether the data that they receive are correct. One solution might be to repeat the message, say, five times, allowing the receiver to compare all the versions and make a good guess as to what was intended. This procedure, however, is very wasteful; the spacecraft can transmit at only one-fifth the rate, and soon its memory will overflow with pictures it has taken but not yet transmitted. Closer to home, the same problem arises with static on a telephone line or even with random errors in stored data, such as bank account balances.

In the earliest days of high-speed communications, the task of building in redundancy without too great a loss in transmission rate was very much a hit-or-miss procedure. Soon, however, mathematicians realized that the question could be approached systematically. First, information theory and probability could be used to study the problem of determining what message was likely to have been sent. Second, the codewords in a coding scheme could be chosen to correspond with the elements of some algebraic or combinatorial object (like a vector space or a graph); the mathematical properties of these objects then could be used to estimate the error-correcting power and transmission rate of the code and thus to find efficient codes. Some of the most common algebraic codes today, for example, use properties of the geometry of lattices in n-dimensional space and the automorphic forms associated with them, or finite geometries and their symmetry groups, or the behavior of the roots of polynomials over finite fields.

A stunning example is Goppa's recent suggestion of a novel way to use algebraic geometry to generate codes. (Goppa is a distinguished Soviet expert in the theory of codes.) Specifically, he started with a curve X over a finite field, certain distinguished points $p_0, p_1, \ldots, p_n$ on X and certain meromorphic functions $f_1, \ldots, f_n$, where f_i has a simple pole at p_i and possibly a pole at p_0. The allowable messages, or codewords in the scheme, would be those n-tuples $(c_1, \ldots, c_n)$ with the restriction that $\sum c_i f_i$ has a zero of order w at p_0, for some w chosen in advance.

The point of such a complicated construction is that algebraic geometers have long studied these objects. The famous Riemann-Roch theorem provides an estimate of the transmission rate of such a code. Similarly the error-correcting power of such a code can be determined by estimating of the number of zeroes of such curves of a given genus. This has also been a topic of great interest in the algebraic geometry research of Deligne, Rapoport, Ihara, Langlands, and others.

Recently, Tsfasman, Vladut, and Zink have applied Goppa's method by using Shimura curves with supersingular points, objects long cherished by mathematicians studying number theory, group representation theory, automorphic forms, and algebraic geometry. Some of the codes obtained not only are better than the best previously known, they are better than the Gilbert-Varshamov bound (a particular bound on efficiency which had been assumed by many to be the limit of how efficient a code could be).

We do not know how practical these new codes will be to implement, but their discovery illustrates how coding theorists find unexpected applications of other, often esoteric branches of mathematics. The flow goes in both directions, however; the sort of questions that coding theorists ask about geometry are at times different from those geometers have studied. Geometers estimate zeroes of curves of varying genus over a fixed field. In this case known methods in geometry could be generalized to yield the desired coding bounds.

Encryption: Sending Secret Messages

Encryption is the process of scrambling a message to make decoding impossible. It has been a hot topic of mathematical interest since 1976 when Diffie and Hellman proposed the idea of a public-key crypto system (PKC).

Such a system exploits mathematical "trap-door" operations, i.e., functions much easier to evaluate than to invert. For example, it is much easier to add together a collection of numbers chosen from a set than to inspect the sum to figure out which were the numbers added. Merkle and Hellman used this notion to create the first PKC. Rivest, Shamir, and Adleman created another scheme based on the fact that multiplying two prime numbers together is simple, while determining what the factors were from the product is very difficult. This scheme has received wide attention.

What makes PKCs unique is that the sender and receiver never need to exchange the secret key for the cipher. For example, in the second scheme above a recipient would announce a "public key" consisting of a large number N and an integer r. Anyone wishing to send a message to this individual would scramble his message according to a simple procedure: consider the digital message as an integer module N (breaking it into blocks if necessary) and raise this integer to the rth power modulo N. There is a second integer s such that raising the encrypted message to the sth power unscrambles it. The catch is that the only known way to compute s requires knowing not just N, but the prime factors of N as well—information which the recipient keeps to himself. So, the recipient has a way of decrypting, but anyone else must first factor N.

Factoring an integer N is a surprisingly hard problem; the best known algorithms take a long time. The most straightforward procedure may require testing up to $N^{1/2}$ numbers as potential divisors. Better methods have been divised which take $O(N^{1/4})$ steps; in fact, the number of steps can be brought down to $c(\varepsilon)N^{\varepsilon}$ for any $\varepsilon > 0$. Here $c(\varepsilon)$ is a constant depending on ε, which grows very rapidly for $\varepsilon < 1/4$. These algorithms, however, are far too slow to factor a 100-digit number.

By contrast, since primality testing can be carried out quickly, a recipient in a public key system can easily choose a 100-digit number N by finding two 50-digit primes and multiplying them together.

Whether such an encryption scheme is secure enough for important government and commercial communication depends on just how hard prime factorization really is. If factorization were known to be an essentially intractable problem, the novel and convenient scheme could be used with full confidence. If a very fast algorithm were known, it would have to be abandoned entirely. And so the issue of security hangs upon questions which a decade ago would have been thought of little practical importance.

As PKCs become more widely used, mathematicians will face the increasing challenges of attempting to crack them. Already, the original scheme of Merkle and Hellman has been broken by Shamir, who showed in 1982 that integer programming techniques can detect patterns in the scrambled messages, making unauthorized deciphering possible. By contrast, many researchers believe that prime factorization is an essentially hard problem. Still, if we use the history of mathematics as a guide, a revolutionary method of factoring should not be discounted. All we can say for certain is that in the next decades some very pure mathematics will take on some new and important ramifications.

5. ENGINEERING

Engineering provides an excellent model of the interaction between mathematics and the other sciences. We include here those areas of classical physics concerned with the gross behavior of matter, including the mechanics of solids, fluids, electromagnetism, chemical reactions, etc. Much of the mathematics which arises is nonlinear, and for this reason the questions are especially difficult and challenging. The overall subject is so diverse that we can discuss only a few selected topics in the sections which follow. A recurrent theme is the interplay between asymptotic and numerical analyses. The isolation of leading contributions may require formulation of new mathematical models. Numerical methods require new mathematics as well. Again, we are not attempting to be representative, but rather to provide generic examples.

Differential Equations

One of the most active branches of mathematics is the theory of differential equations. As discussed in the section on Fourier, harmonic analysis led to the classical understanding of heat and light through the study of the diffusion equation and of Maxwell's equations. These are only two examples of linear differential equations central to engineering. The general methods have been highly developed in the case of linear equations. Here detailed information has been established on properties of solutions to equations which govern our every movement. An understanding of characteristics has been essential for engineering insight into wave propagation and fluid flow. Fourier analysis and its generalizations are such standard points of view that one almost takes linear differential equations for granted; yet they are the basis for a huge fraction of mathematics. Linear equations also lie at the foundation of nonrelativistic quantum theory and hence at the understanding of materials.

Nonlinear differential equations date to the time of Newton and his study of the planets. These equations tend to be harder to understand, especially nonlinear partial differential equations. Fewer solutions are known in closed form. (Special solutions to special equations with an interpretation in nature, such as solitons, have achieved widespread use in engineering and physics models.) Furthermore, the methods to understand one equation seem maddeningly inappropriate for another! However, it is these equations which are important in describing the chemical reactions in a combustion engine, fluid flow under most conditions, magnetohydrodynamics, or stresses in solid bodies. Generically, the equations that describe extreme temperatures, forces, or pressures tend to be nonlinear. Hence many of the most important engineering problems center on the understanding of nonlinear effects. Clearly a theoretical understanding of the equations is important for both qualitative and quantitative questions of design.

Nonlinear equations can also have more than one solution for a given set of boundary values or initial conditions. The question of whether and when this happens for a particular equation is the subject of much current research. The bifurcation process which can occur with the onset of nonuniqueness clearly is important for structural stability, chemical processes, and turbulent flow. We touch on many other aspects of differential equations throughout this section.

Complex Function Theory

Complex numbers were introduced in the 16th century to solve quadratic equations. Only some 300 years later did Gauss demonstrate that the roots of every algebraic equation are complex numbers. The theory of functions of a complex variable emerged as a fundamental area of mathematical research due to his influence and that of Cauchy. The famous Cauchy integral theorem was proved in 1825; Cauchy also laid the foundations for the theory of elliptic integrals. Twenty-five years later, Riemann vastly enriched the subject, discovering connections between problems in physics on the one hand, and those in complex function theory on the other. Riemann's results and conjectures inspired a whole succession of further developments, including the unification and clarification of the integral transforms we now associate with the names Fourier, Laplace, Poisson, and Hilbert.

Complex analysis has permeated engineering. A major reason behind the success of this method is that by using complex numbers, two-dimensional problems can be handled the way one-dimensional ones had been previously. While vectors also simplify multidimensional analysis, they obey a different calculus from numbers. Using complex numbers, one can either study problems depending on two variables (for example, three-dimensional problems with a symmetry) or problems involving two real valued functions which could be treated simultaneously as one complex-valued function.

By 1920, scientists at Bell Laboratories were making systematic use of complex function theory in the design of the filters and high gain amplifiers which made long distance telephone communication possible. A notable example of the importance of complex function theory is the Nyquist criterion for the stability of feedback amplifiers—an aspect of the "argument principle" in complex analysis. While mathematically straightforward, the Nyquist diagram became a marvelous tool for understanding and defeating feedback instability; it is now taught to every engineer.

Conformal mapping techniques have been used to solve a host of problems along the lines envisioned by Riemann. As might be expected, the general applicability of complex analysis to two-dimensional problems became legend. For example, Joukowski used complex mapping techniques to specify the shape of an airfoil, and to analyze the flow pattern around it, revolutionizing airplane design. Complex function theory became a central tool in the description of fluid flow, and in the design of cars and ships.

Time Series and Control Theory

Norbert Wiener's scientific career represents an unusual achievement in mathematics, because much of his most abstract and theoretical work had "instant" applicability. His theory of time series analysis which he developed during World War II to aim artillery, became a focal point of modern control theory. In fact, the original version of his classic paper, "Extrapolation, Interpolation and Smoothing of Stationary Time Series," was a classified document. Because of the color of its cover and the impenetrability of its content to engineers, the paper became known affectionately as "The Yellow Peril." This work, however, had profound implications not only for artillery, but throughout engineering. On the theoretical side, Wiener's work, interpreted by Norman Levinson, blended with the pioneering research of Kolmogoroff in the Soviet Union to form the basis for communications theory, as well as strongly influencing modern ergodic theory and statistical mechanics. As explained in another section, its influence spread through physics.

Questions of how to control engineering processes abound. The origins of control theory lie deeply within the variational calculus. Its early formulations relied on methods that came directly from that part of mathematics: the Nyquist stability criterion, the Wiener filter, the Pontryagin maximum principle, the Kalman filter, and probability theory.

Present research includes understanding systems governed by the heat of wave equations, such as power transmissions networks, telephone networks, chemical processing complexes, large systems of coupled electrical or mechanical devices, etc. Questions raised by robotics—including constrained motion, response to signals, etc.— all fall in this domain.

A related scientific problem is understanding the nature of digital messages. Wiener's student Claude Shannon carried out an analysis of transmission in the presence of noise. Today we view his work as the foundation of modern information theory. It provides the theoretical basis for all telephone and data communications, and the background for the work discussed in the section on coding.

The impact of time series analysis was not limited to communications. G. Wadsworth, a colleague of Wiener, happened to carpool with a geologist named Hurley. Their casual discussions around 1950 revealed that time series analysis might be useful in the seismic exploration for oil. Developed by Wadsworth, Bryan, Robinson, and Hurley, this method of Wiener's has become the standard tool for modern oil exploration! At that time they implemented the new method of analyzing sound signals reflected from the earth with the aid of desk top calculators; today naturally it is carried out on large computers. In the industry, conversion to Wiener's method is referred to as the "digital revolution." It is interesting to note that twenty-one oil companies supported the work on applications in the geology department at MIT. However, no industrial support was given to the pure mathematical research in the same university which made the application possible—even with such a short time-scale for so important a payoff. In fact the application was neither envisaged nor dreamt about at the time of the original mathematical advance—an advance oriented toward an entirely different goal.

Solid Mechanics and Elasticity

Solid mechanics is the science which studies the deformation and motion of solid bodies under the action of forces. It describes the behavior of steel springs and aluminum airplane wings, of rubber tires and asphalt pavement, of muscle fiber and nylon fiber.

The mathematical apparatus for describing how a body, solid or fluid, changes shape was developed by Cauchy and refined in recent years. Every part of every body must satisfy the same equations of motion. The crucial ingredient in solid mechanics is the equation which expresses how the force intensity at any point in a body is related to the change of shape near that point. We can distinguish a rubber band from a steel band of the same size by noting that a given force produces a far greater elongation in a rubber band. Other equations distinguish the responses of air, water, paint, and tar. These equations may be inferred from experiment or derived from a fundamental model.

Elasticity treats materials that are springy, such as rubber, heart, muscle, and steel. The linear theory of elasticity describes small deformation of elastic bodies and is the basis for the study of structures, machines, seismic waves, etc. Plasticity treats solids, like paper clips, that do not spring back to their natural state when the forces that have deformed them are removed. It furnishes an effective theory for describing the forming of metals and determining the ultimate strength of metallic structures. Results in the nonlinear theory hold promise for detecting thresholds at which materials have qualitatively different responses to their environments.

It is important to know the strength and reliability of machine parts such as valves regulating the flow of hot radioactive liquids in an atomic energy plant. The linear theory of elasticity describes well the behavior of such bodies, except near edges and corners, where cracks can form. Studies of the singularities of solutions of the equations of solid mechanics near edges and corners, of the role of plasticity and nonlinear elasticity at such singularities, and of criteria for the onset of fracture and the propagation of cracks are being actively pursued.

Dynamical Systems and Fluid Flow

Fluid flow plays a central role in engineering, and has provided the focus of much classical mathematical study. It is generally assumed that the motion of a viscous, incompressible fluid is described by the Navier-Stokes differential equation, and in the limit of zero viscosity by the Euler equations. A typical dimensionless parameter characterizing fluid flow is the Reynolds numbers, which is proportional to the fluid velocity. For small Reynolds numbers (slow speeds or highly viscous flows) the equations of Navier-Stokes lead to smooth streamlines, called laminar motions. But at higher Reynolds numbers (i.e., higher speeds or lower viscosities), these laminar flows no longer persist. While they may exist as solutions of the governing equations, they are not stable. In contrast, they are replaced by time periodic or quasi-periodic perturbations of the basic flow. Bifurcations of the solutions to the equations enter here.

Only in the last twenty years have mathematicians made significant progress on the problem of this transition and the calculation of resulting flows following an instability. At even higher Reynolds numbers, the flow becomes highly irregular and is known as turbulence. Clearly, any understanding of turbulence has important consequences for aircraft design, for understanding chemical reactions, combustion, and flame fronts, etc. From a mathematical point of view, these equations have proved surprisingly difficult. Even a general proof of the existence of solutions to the Navier-Stokes equations has not been found. Understanding fully developed turbulence exceeds our grasp at this time. In spite of this fact, a tremendous amount is known about some special solutions and models.

Various statistical models of turbulence were proposed by Taylor and von Karman in the 1930s. Shortly afterward, Kolmogorov introduced locally isotropic turbulence and derived the asymptotic form $E(k) \simeq k^{-5/3}$ for the dependence of the energy on wave number.

Understanding turbulent solutions to the equations, or understanding the onset of turbulence as the Reynolds number increases, is still at a preliminary stage. One approach has been to obtain a priori estimates which limit the possible singular nature of a solution. Important progress in this direction has been made over the last couple of years, in bounding the Hausdorff dimension of the singular set. Some people conjecture that the Navier-Stokes equations are dominated by the viscosity and therefore have no singularities at all, though the Euler equations for zero-viscosity flow are generally expected to have singular solutions. This is an area of ongoing study, whose mathematical resolution will be of practical note.

Bifurcation Theory

Bifurcation theory began with studies by the mathematician Leonhard Euler in the middle of the 18th century and with Poincaré's work at the end of the 19th century. It includes a body of techniques for studying the solutions to nonlinear equations when their character changes discontinuously as parameters in the equations cross certain thresholds. Often this occurs at particular parameter values when the equations first have nonunique (multiple) solutions. Buckling and fluttering instabilities are examples of bifurcation, as are instabilities in plasmas. This sort of question was intensively cultivated in the Soviet Union and Europe in the 1950s and 1960s. Since then, bifurcation theory has undergone a remarkable renaissance. In this development, methods of point set topology, algebraic topology, and algebraic geometry have been combined with analysis.

One interesting aspect of bifurcations relates to fluid flow. In particular it is the proposal that the onset of turbulence can be described by the mathematics of successive bifurcations, leading to a transition to chaos. Several different pictures have been proposed, some involving a small number of bifurcations, others using infinitely many.

The bifurcations of iterates of quadratic maps of the unit interval $[-1, 1]$ into itself are the basis for one theoretical picture of the onset of turbulence. In certain situations, corroborating experimental evidence supports this picture. Such bifurcation problems were studied by Ulam and von Neumann in the 1940s; they even appear in earlier work of Volterra, who was asked by the Norwegian government to develop a theory of populations of fish. Today, the mathematical properties of such bifurcations are also related to problems in ergodic theory, continued fraction expansions, Kleinian groups, and topology. Surprisingly, they are also related to phase cell localization and the renormalization group in mathematics and physics.

Bifurcations provide a model of chaotic behavior in a deterministic system. New universal constants have been discovered which are associated with the limit of successive bifurcations; these numbers can be measured both in numerical simulations and also in certain actual physics experiments. Numerical evidence for the existence of these universal numbers was discovered around 1976; it has recently been proved as a mathematical result—using both renormalization group ideas and a computer-assisted proof. A large mathematical literature on these problems is developing at the present time, and the interplay between the new mathematical discoveries and related phenomena in such problems as fluid flow, chemical reactions, or stellar dynamos fascinates many people.

We can hope that recent advances in the qualitative theory of differential equations with the ideas of strange attractors, the abstract mathematics of fractals, and the use of super computers will lead to progress. We also look forward to new mathematical ideas to help in understanding the Navier-Stokes equations, both for fundamental reasons and also because of their technological importance.

Transonic Flow and Shock Waves

An example of the profound interaction of mathematics and practical technology can be found in the development of methods, stimulated by the needs of aircraft designers, to calculate transonic flows. In practical terms, one models supersonic flight and shock waves. The approach depends for background on studies of partial differential equations by Tricomi in the 1920s, followed by the theoretical analysis of the numerical solutions to elliptic and hyperbolic differential equations by Courant, Friedrichs, Lewy, and others. Extension of their work produced good understanding of the basic physical ideas of transonic flow, but progress was limited by the inability to calculate.

It had long been known, however, that incompressible flow theory does not explain the phenomena of high speed-gas flow; rather one must use the partial differential equations of compressible gas dynamics. These equations are locally elliptic for subsonic flow and locally hyperbolic for supersonic flow; in both cases they are strikingly nonlinear.

When viscous effects are present, one must study the full nonlinear equations of Navier and Stokes, as well as other, more detailed models. Other viscous effects are confined to thin layers outside of which the fluid can be treated as inviscid. One major advance in modern applied mathematics is the theory of boundary layers, a brilliant invention in 1904 by Prandtl. He simplified the effects of viscosity without sacrificing the essential features of the flow model. The mathematical development of boundary layer theory has had profound effects in many branches of pure and applied science and enables us to come to grips with numerous phenomena in which the effects of viscosity (or other similar phenomena) are essentially restricted to well-defined regions. Indeed the study of transition effects in thin layers influences entire areas of engineering.

The advent of advanced computer calculation made it possible to include discontinuities (shocks) in the numerical algorithms. This, together with the development of new difference schemes, has made possible practical calculations. One can now obtain shock-free airfoil designs and simulate wind tunnel testing. Large computer codes based on these ideas are used by the aircraft companies on a regular basis.

Combustion Theory and Chemical Reactions

The theory of reactive flow, or combustion theory in gases, includes all of fluid mechanics and adds an extra complication as well: the interaction of fluid flow with chemical reactions. Chemical reactions in a fluid flow change its essential characteristics. The release of heat due to exothermic reactions may cause a flow to be unstable; and the types of instability which arise may be of a different nature from ordinary fluid dynamical instabilities.

The problem of slow flames (deflagrations) in a gas, as well as theories of detonations, i.e., fluid mechanical shocks, are two areas of current research. In the theory of nuclear reactors, related issues point to models of critical size and thermal runaway. Equally important are techniques for incorporating the relevant chemistry into the mathematical analysis. More generally, chemical reactor theory accounts for diffusion and reaction, but typically includes no compressible fluid mechanical effects. Chaotic behavior in dynamical systems has found its way into chemical reaction theory. Chaotic regimes, in fact, have been experimentally and computationally found in the Belousov-Zhabotinskii reaction and other oscillatory reactions. Chaos occurs as a limit of successive bifurcations of periodic motions as the chemical concentrations or flow rates vary. The theory is strikingly similar to the mathematics of the bifurcation model for the onset of turbulence above.

Integral Transforms

The Fourier transform is a special case of the general notion of an integral transform, of profound importance in physics and engineering as well as mathematics. The linear transform T relates a function $f(x)$

to its transform $(Tf)(x)$ by the formula

$$(Tf)(x) = \int K(x,y)f(y)\,dy.$$

Here $K(x,y)$ is a function which characterizes the particular transform.

Exactly when such ideas originated is not certain, but L. Euler used such a transform in 1737 to solve a differential equation. The general method was developed in the early 1800s by Gauss, Fourier, Dirichlet, Laplace, and others.

The same transform studied by Euler appeared as a central tool in Laplace's classical book on probability, published in 1812. There he cast probability theory in a form more or less unchanged until the 20th century. The transform has come to be known by his name.

The Laplace transform did not achieve widespread popularity in engineering until it was rediscovered in a somewhat different guise by Heaviside toward the end of the 19th century. Faced with the practical problem of understanding the transmission and attenuation of waves in the trans-Atlantic cable (laid in 1866), Heaviside invented the "operational calculus." This powerful method solved many hitherto intractable problems in electrical engineering. Some years later, it was realized to be an aspect of Laplace transform theory, which every undergraduate engineer and scientist now studies.

A natural development of Fourier, Laplace, and Heaviside transform theory is to extend the class of functions on which they are defined. In physics, Dirac had already used such a notion with his "delta function," but no general mathematical theory existed. This led in the 1950s to the theory of distributions (developed by Schwartz, Gelfand, and others) and provided the basic tool for the modern theory of partial differential equations.

A generalization in another direction is the integral transform, generally attributed to Funk and Radon about 1916–1917. In particular, their transform of a function $f(x)$ defined on a plane is the integral of f over a line l, namely

$$(Tf)(l) = \int_l f(x)\,de.$$

Here de is the element of length on l. The original function f can be reconstructed from its Radon transform. The transform has been generalized to a transform between two homogeneous spaces of a given group, and has provided invaluable insight both in analysis and in geometry.

Roughly 60 years after the original work above, the physicist Cormack wrote a paper entitled, "Representation of a Function by its Line Integrals." His basic problem was to understand how to reconstruct an image from an X-ray (or radioastronomy) measurement. The practical development of this idea led to computer-assisted tomography, or CT scan, and was recognized in the 1979 Nobel prize for medicine. In actually building a CT scanner, one implements in a microprocessor the fastest possible convolution transform algorithm, a problem closely related to the Fourier transform algorithms described in the section on Fourier. One might expect this from the unity of mathematics.

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ATTACHMENT 1
REPORT OF THE RESEARCH BRIEFING PANEL ON MATHEMATICS

Committee on Science, Engineering, and Public Policy
National Academy of Sciences
National Academy of Engineering
Institute of Medicine

Research Briefing Panel on Mathematics

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PREFACE

This is one of seven research briefings in response to a request from Dr. George A. Keyworth, Science Advisor to the President and Director of the White House Office of Science and Technology Policy (OSTP). The effort was directed by the Committee on Science, Engineering, and Public Policy (COSEPUP), a joint committee of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

Topics for the seven research briefings were selected by OSTP. For each topic a balanced panel of 11–13 experts was organized to develop the briefing. The specific charge to each panel was to critically assess its field and to identify those research areas within the field that were likely to return the highest scientific dividends as a result of incremental federal investments in FY 1984. It was also emphasized that these briefings were not to be construed as substitutes for the much more detailed surveys occasionally undertaken in major scientific fields (e.g., the recent report of the National Research Council's Astronomy Survey Committee entitled *Astronomy and Astrophysics for the* 1980*'s,* Volume 1).

Through discussions with OSTP, the seven topics were defined as follows:

1. **MATHEMATICS**: Research covering the following fields of investigation: statistics, pure and applied mathematics, mathematical systems theory, numerical analysis, operations research, computational mathematics, and scientific computing.

2. **ATMOSPHERIC SCIENCES**: The study of the physical, chemical, and dynamic properties of the atmosphere and its interactions with the Earth, the oceans, and the planetary environment with a view to understanding and predicting the atmosphere's changes and behavior as manifested in weather, climate, air quality, and other characteristics relevant to human society, both as a result of natural processes and as influenced by human activities.

3. **ASTRONOMY AND ASTROPHYSICS**: Research with the objective to obtain information about astronomical bodies by remote sensing from the surface of the Earth, from the Earth's atmosphere, and from Earth orbit.

4. **AGRICULTURAL RESEARCH**: Research of greatest promise for increasing the productivity and efficiency of American agriculture, including:

- plant sciences targeted on developing more productive, resistant, tolerant, and energy-efficient crop plants;
- an objective assessment of the realistic expectations of genetic engineering for developing more productive, resistant, tolerant, and energy-efficient crop plants; and
- research on crops and cropping practices that are more resource conservative.

5. **NEUROSCIENCE**: Research directed toward understanding the molecular, cellular, and intercellular processes in the central nervous system (CNS) and the way in which those processes are integrated in CNS functional control systems, with emphasis on research relating CNS functions with behavior.

6. **HUMAN HEALTH EFFECTS OF HAZARDOUS CHEMICAL EXPOSURES**: Research on the responses of organisms to hazardous chemical exposures, including:

- the nature of the steps leading to damage to the organisms;
- the mechanisms and kinetics of metabolism of hazardous substances;
- the nature of protective responses and repair mechanisms; and
- the in vitro, animal bioassay, and epidemiological methods used to characterize hazardous exposures.

7. **MATERIALS SCIENCE**: Research concerned with reaching a clearer understanding of the complex relationships that exist among the atomistic structure, composition, and defects of materials and their behavior in an engineering environment. Specific areas of investigation include those concerned with surface characterizations, defect structure, electronic structure, catalysis, the theory of crystalline solids, and the properties of solids (e.g., electrical, magnetic, optical, thermal, and mechanical).

Each panel met once, for 2 or 3 days, to carry out its charge. Knowledgeable representatives of government and the private sector were invited to provide input to the panels. Rapporteurs, knowledgeable in the field, were present to summarize the discussions and prepare initial drafts of briefing papers. These papers were reviewed and revised by the panel members and served as the bases for the oral briefings presented to federal officials.

The seven one-hour briefings, presented by panel chairmen and, in some cases, 1 or 2 other panel members, were reviewed by COSEPUP in mid-October and presented to Dr. Keyworth and members of his staff between October 26 and November 18, 1982. The same briefings were subsequently presented to Dr. Edward Knapp, Director of the National Science Foundation, and other Foundation officials on three days in December. Briefings for other interested departments and agencies were held separately on the same dates.

None of this would have been possible without the financial support of the National Science Foundation and the cooperation, under difficult time constraints, of the panel members and staffs. We are indebted to both groups.

If judged useful to federal decision makers, the seven initial research briefings developed as an experiment in 1982 could serve as the basis for research briefings on other major fields of science in future years. Such briefings could supplement other inputs and become important new channels for communication between the federal government and the scientific community.

George M. Low, *Chairman*
Committee on Science, Engineering, and Public Policy

REPORT OF THE RESEARCH BRIEFING PANEL ON MATHEMATICS

PREFACE

In response to an invitation from OSTP to present a research briefing on the current state of mathematical research in the United States, a panel was convened on September 25 and 26, 1982. In addition, opinions were solicited from the chairmen of the top 27 research departments of mathematics in the United States.

This document is an account of the deliberations of the panel. Our charge was to identify special opportunities in the mathematical sciences. We could identify many promising areas ripe for development, but it is our belief that the most dramatic mathematical tendency in recent years is the drawing together of mathematics (often the most abstract science) with other sciences and the interplay between them. Some results have already been achieved; with encouragement, the interplay can be made increasingly fruitful.

A healthy mathematical enterprise must have effective means for nurturing new developments. These means involve (1) support of gifted young investigators who will choose from among the new directions and (2) flexibility for scientific leaders allowing them to develop and expose recent breakthroughs. Unfortunately, severe underfunding has limited the mathematical community's capacity to respond to these ways. For this reason, in addition to spelling out the current state of mathematical research, we have tried to specify the additional resources that will be needed to exploit new opportunities.

INTRODUCTION

There is a striking contrast between the importance of mathematical sciences in the United States and the perception of them. On the one hand, mathematics and its applications play an ever increasing role in science, technology, business, and everyday life in this country, and, on the other, mathematical research is almost completely unknown to and poorly understood by the general public and even the scientific public.

The reputation and achievements of the American mathematical community make the United States first in the world in mathematics. Yet at the same time support for mathematical research erodes at a steady rate, and the institutional infrastructure that supports the enterprise exhibits symptoms of decay. In a year in which two out of three of the quadrennial Field's medalists (the mathematical equivalent of the Nobel Prize) are American mathematicians, we find highly ranked departments of mathematics lacking enough research support to send their most productive people to professional meetings or to photocopy important documents. At a time when the development of research in mathematics is making unparalleled progress, when the influence of mathematics is pervasive in the other sciences, when American mathematicians lead the world in most areas, research support for graduate students and young Ph.D.s, the lifeblood of the enterprise, is insufficient to ensure the quality of future generations.

In Section 1, we describe some of the exciting contributions and important developments in mathematical research and the highly promising opportunities that arise from the recent opening of new bridges between mathematics and other sciences. In Section 2, we document the decay of the infrastructure and support of mathematical research, and indicate possible actions to prevent further decay of the mechanisms that have enabled U.S. mathematics to flourish. In Section 3, we recommend steps to be taken to rehabilitate the ailing infrastructure and to provide the flexibility needed for the exploration of new opportunities.

SECTION 1 SOME RECENT DEVELOPMENTS IN MATHEMATICAL RESEARCH

Our exposition will be in two parts, the first giving examples of the pervasive influence of mathematics in other sciences and the second recounting some of the recent significant advances in theoretical mathematics. Some speculations on future possibilities will be interspersed. (For a somewhat fuller review, we refer the reader to the chapter "On Some Recent Developments in Mathematics" in *Outlook for Science and Technology, The Next Five Years*, NAS, W. Freeman & Co., San Francisco, 1981, pages 467–510.)

Influence and Applications of Mathematics

The research area of mathematics most used today in technology is numerical analysis and mathematical modeling. In the area of industrial design, for example, a given process must be described and understood in a mathematical way, and the details of the mathematical description will interact with the design process. Analysis and design become mathematically interdependent. For example, the design of the fuel efficient, weak shock transonic airfoil, currently flying on the Boeing 767, would not have been possible without the mathematical work of Garabedian, Cole, and Jameson. On the speculative side, a project to mathematically model the human circulatory system now under way might eventually have important medical consequences. Among them would be the possibility of indirect ways to measure the heart in a situation where the direct measurement of the heart itself is impractical. Computer-aided design (CAD) is now used in the design of artificial heart valves, using a mathematical model of the left side of the heart.

Mathematical design of efficient compression and turbine blades is a reality today, while the design of efficient combustion chambers is a subject of intense research.

In national defense, the replacement of experimentation by numerical modeling, made possible by advances in computers and dramatic improvements in mathematical algorithms, has resulted in great savings in the cost and improvement in the quality of design. This has been particularly significant in weapons-related research and development, where experimentation is costly, dangerous, and physically impossible in the early stages of a project.

In economics, mathematics is playing an ever-increasing role, as witnessed by three recent Nobel prizes in mathematical economics.

In oil prospecting, mathematical results are used in a fundamental way in the separation of primary signals from multiple reflections. The modern theory of inverse scattering is becoming a basic tool in this area. Mathematical modeling is important in the study of efficient secondary oil recovery.

In electrical engineering, the mathematical work of Weiner has proved fundamental in several areas, and mathematical control theory plays an important role.

In medicine, great advances in diagnostic techniques (tomography—the CAT scanner; and NMR) are strongly related to mathematical research. In the latter, methods from singular integrals, complex function theory and Hilbert space were used. Statistics and statistical methods are crucial in epidemiology, drug testing, and many other areas, and mathematical modeling is an important tool in the development of new drugs. The list could be extended indefinitely with examples drawn from biology, chemistry, neuroscience, and other sciences.

There are many recent examples where mathematical research, driven by the inner dynamic of the subject without reference to practical problems, has been found to be of great significance in other areas. An outstanding illustration of this has been in the development of Gauge Field Theory in physics. Nobel Prize winner C. N. Yang wrote, "I found it amazing that gauge fields are exactly connections on fibre bundles, which the mathematicians developed "*without reference to the physical world.*" Algebraic geometry produced all self-dual solutions for the Yang-Mills equations. But the physical theory led also to important consequences in topology, as we will relate later.

Other new and important mathematical inputs into physics have been the introduction of abstract probability into statistical mechanics and material science with the notion of Gibbs state and the input of the theory of dynamical systems and ergodic theory into the study of turbulence. All these phenomena illustrate the drawing together of abstract and applied mathematics and their fruitful interaction.

As a result of the rise of the computer, the theory of computation has become an area of mathematical research. Solidly based on methods and fields in the mainstream of modern mathematics, such as probability, combinatorics, algebraic geometry, and number theory, it creates important tools for the practicing computer scientist. The main themes in this new area are the study of algorithms and of programming.

Efficient algorithms often have important practical importance. Notable examples are the Fast Fourier Transform with its application to signal processing and the recently developed randomized algorithms in number theory and finite fields with their application to error-correcting codes and cryptography.

Developments in coding and cryptography provide dramatic examples of unexpected applications of "pure" mathematics to applied areas. Number theoretical work of A. Weil in 1948 was applied to coding theory some years ago. Last year a group of Soviet mathematicians showed how to use the latest work of Deligne, Rapoport, Ihara, and Langlands, in the most abstract areas of algebraic geometry, in the design of error-correcting codes of a theoretical efficiency heretofore deemed impossible.

In the field of robotics, the development of automatic industrial processes depends on successful mathematicians or modeling of the processes involved. In many industrial areas, progress is in its infancy, and some of the most simple tasks seem the least likely to yield to automation. It is extremely difficult to design a robot arm with sensors that will enable it to avoid obstructions while picking up a target object, one of the most routine of human abilities. The parameters of this problem can be interpreted as a problem in algebraic geometry, and progress here may have some effect on the solution of other practical problems.

There are current proposals for initiatives in large-scale scientific computing that would have sizable components of mathematical research and important applications to applied mathematics. In pure mathematics, Thurston (one of this year's Field's Medalists) has made a surprising use of the computer as an experimental tool in his work on topology of 3-dimensions, although the solution of the famous four-color map problem a few years ago required the computer essentially in the proof.

Recent advances in computer technology and software are having a deep influence on the nature of work subjected to statistical analysis, on the methods of analysis, and on theoretical questions in statistics. The computer and space technologies provide vast amounts of high-dimensional multivariate data which standard classical methods no longer fit, because the underlying assumptions of normality and linearity are no longer satisfied. Methods justified under those assumptions would lead to serious errors. Novel methods of pattern recognition and robust regression and new methods of graphical representation that allow the comprehension of these data are being developed. The interactive kinematic displays of Friedman and Tukey are examples.

New interactive statistical packages with properly built-in diagnostics will permit naive users to observe phenomena formerly accessible only to trained and ingenious statisticians.

Statistics has aided computer science in that simulations and experimental designs have been used to find good hardware configurations and software designs.

Progress in Theoretical Mathematics

By "theoretical mathematics" we mean research motivated by the inner dynamic of the subject rather than by the needs of research in other sciences. It is remarkable how often much of this research, seemingly irrelevant, turns out to have important practical impact. Who, for example, in the 1920s and 1930s would have predicted that the most abstract work in mathematical logic—recursive functions and "Turing machines"—would provide the philosophical framework for von Neumann's introduction of the stored program computer, in which the instructions to the machine can be manipulated and modified by the machine itself? The development led eventually to a multibillion dollar industry.

Progress has been spectacular in recent years and we cite some notable examples:

The work of Deligne, proving the famous "Weil conjectures" in number theory.

The classification of finite simple groups, as the end of a 20-year effort.

The work of Yau on the Galabi conjecture, with important applications to algebraic geometry.

The work of Thurston, showing how to employ methods of (mostly non-Euclidean) geometry to attack problems in the topology of 3-dimensions.

The work of Kachian on polynomial algorithms in linear programming.

The discovery of solitons and strange attractors.

The work of Connes on operator algebras.

Instead of continuing this list at length, we give in greater detail short accounts of some recent dramatic examples, starting with a startling advance in pure mathematics resulting from interaction with physics.

- Physicists have introduced gauge theories in 4-dimensions (space-time) as a unifying principle in field theory. The study of Yang-Mills equations of motion in this context led S. Donaldson to a remarkable description of certain 4-dimensional spaces. A little earlier, M. Freedman, using purely topological methods, had produced a powerful comprehensive theory of 4-dimensional manifolds. These results of Donaldson and Freedman have combined to give the following result in the topology of 4-dimensional space. In all other dimensions there is essentially one mode of doing calculus in a Euclidean space (R^n has a unique differential structure for $n \neq 4$), but an entirely different situation exists in dimension 4 (there are at least two different structures on R^4). This qualitative difference between dimension 4 and other dimensions is a startling development for topology, and it may also be the reflection of some deeply significant physical principles.

- The unifying role of group symmetry in geometry, so penetratingly expounded by Felix Klein in his 1872 Erlanger Program, has led to a century of progress. A worthy successor to the Erlanger Program seems to be Langlands's program to use infinite dimensional representations of Lie groups to illuminate number theory.

That the possible number fields of degree n are restricted in nature by the irreducible infinite dimensional representations of GL(n) was the visionary conjecture of R. P. Langlands. His far-reaching conjectures present tantalizing problems whose solution will lead us to a better understanding of representation theory, number theory, and algebraic geometry. Impressive progress has already been made, but very much more lies ahead.

Closely related to the Langlands program is the remarkable and mysterious connection between counting points in finite spaces and computing the topological invariants of continuous spaces. First propounded in the Weil conjecture, the connection is being made more accessible by the Goreski-MacPherson-Deligne homology theory. The whole thrust of these developments is to force the next generation of mathematicians to embrace heretofore widely separated areas of mathematics. The expected unifications are awesome.

- In analysis, the old problem of the regularity properties of the Cauchy integral (for Lipschitz curves) was recently solved by the work of Calderón, Coifman, McIntosh, and Meyer. Crucial to the solution of this problem were the techniques of Hardy spaces developed within the last decade, as well as recent methods for dealing with singular integrals with "rough" coefficients. It seems very likely that these ideas will be applicable to a host of important problems in partial differential equations, as is indicated by their role in recent advances in the solution of "Kato's conjecture" (dealing with square roots of Laplacians) and solutions of parabolic equations with minimal smoothness assumptions.

SECTION 2 THE MATHEMATICAL RESEARCH ENTERPRISE

In analyzing the state of mathematical research and its needs, we must keep in mind special features that distinguish mathematics from the other sciences. Among those features are these:

1. Mathematics is the most labor intensive of all sciences. Little equipment is involved, except for computers, which are heavily used in statistics and areas of applied mathematics and are an experimental tool for a few pure mathematicians.

2. The vast majority of research mathematicians are employed in universities as teachers. Industry and national laboratories support only a handful.

3. Very few federal agencies support research in mathematics. NSF supports 60 percent of all research in mathematics and almost 100 percent of pure mathematics; most of the rest is supported by DOD and DOE. This contrasts strongly with other disciplines.

4. The magnitude of the total research support of mathematics by the federal government in comparison with other fields is miniscule, less than $60 million annually.

5. Mathematics is "small science." Though collaboration among 2 or 3 researchers is not uncommon, large projects with many researchers devoted to specific goals are relatively rare. Mathematics thrives on the interaction of independent viewpoints and different approaches.

6. The health of the mathematical enterprise in the United States hinges on the strength and vitality of the departments in the leading research universities.

The following special factors have strongly influenced the pattern of decay in the support of mathematical research that we perceive.

a) It is now generally accepted that the impact of inflation is much greater in labor-intensive enterprises than in the general economy. The impact of declining resources and inflation has, therefore, been most severe in mathematics.

b) The universities as a whole are subject to the same effect, so that the resources available to them as the main supporters of mathematical research have dwindled proportionately.

c) There has been no organization of mathematicians expressing their discipline's support requirements for research. In sciences needing large instruments or projects to achieve their scientific goals, organized support has evolved and served effectively. But in the small-scale individualistic atomosphere of mathematics, no mechanism has evolved for calling attention to the alarming decline in funding.

d) Inflexibility is inevitable when few funding agencies support mathematical research; investigators not supported by NSF, for example, often have no place else to turn, unless their research has clear potential relation to the goals of a mission-oriented agency.

e) The number of top-ranking graduate students seems to be declining, and many of them are from abroad.

f) The strength of some of the leading departments of mathematics is being undermined by the lack of federal funding for research, a lack that the universities cannot replace, especially in states whose economies are suffering.

Yet the small scale of the mathematical enterprise would make it rather inexpensive to alleviate many of the serious shortcomings in the support configuration and to ensure the health and vitality of American mathematical research into the next century.

As the support of mathematical research by federal agencies has eroded over the last decade, the universities have, to some extent, taken up the support, as for example with postdoctoral research instructorships. The support has, however, become more and more difficult for the financially troubled universities to continue.

Support in the Mathematical Sciences

Currently, the United States ranks first in the quality of research in the mathematical sciences. But the vitality conceals a variety of problems that, if left unsolved, will inevitably lead to a substantial deterioration in the nation's mathematical sciences research enterprise. The same can be said substantially of other sciences, but it is our purpose here to document the special strains in mathematics that are reaching the crisis stage.

The consequences of the financial stringency are falling most heavily on the young mathematicians, graduate students, and recent recipients of the Ph.D. Little research support is available to graduate students, though many teaching assistantships are available, particularly at the large state schools. This means that mathematics graduate students, unlike those in other sciences, seldom have the opportunity to work full time in research. The lack of postdoctoral research appointments in mathematics creates a similar problem for the young Ph.D.'s, who, in addition, are finding it increasingly difficult to obtain research grants. *At a time of real opportunities, in the drawing together and mutually fruitful interaction of mathematics and applications, the young innovators who will be needed to exploit the opportunities are not receiving the nourishment they need for full development.*

The chairman of a prestigious mathematics department writes (in a letter to the Panel): "Mathematical research has been flourishing in the past decade, but the institutional structure of mathematical research is in trouble. Recruitment of young talent for the future looks to be in even more serious trouble. The level of research support has been very low in terms of the percentage of active research people supported, and

TABLE 1

	Percentage of Research in Universities Not Sponsored November 1978–October 1979
Engineering	16
Environmental Sciences	16
Life Sciences	13
Mathematical/Computer Sciences	59
Physical Sciences	22

Source: "Activities of Science and Engineering Faculty in Universities and 4-Year Colleges 1978-1979; Surveys of Science Resources Series NSF Final Report," NSF 81-323, page 2.

recent cuts in support have produced signs of a serious deterioration of morale especially among younger mathematicians."

Another chairman writes: "We are some one hundred in number, we are invariably ranked among the top twelve departments in the country, we continue to recruit good graduate students, and I claim with confidence that of the one hundred at least ninety are seriously engaged in research and scholarship. Yet, after two severe years, we are down from one-half to about one-third of the faculty on NSF grants. Moreover, we have sustained these severe losses without any sense of the prevalent quality of work having declined at all; on the contrary, several colleagues have lost grants in the very year when they have done their best work. . . . At the same time, universities are increasingly affected by lack of money. Here, for example, loss of NSF grants has reduced departmental income from overhead just when the university, which in any case had always counted on strong departments like ours to earn much of its research support outside, is quite unable to raise the level of state support. Also, of course, we find little endowment money coming in earmarked for mathematics."

Yet another writes: "Many young mathematicians are discouraged at their prospects for a successful career in mathematics because of decreased research funds, poor salaries and the shortage of openings in universities. Several I know are actively looking for jobs in other fields where they can expect much better treatment economically, and even the top departments are finding it increasingly difficult to attract qualified graduate students."

What is the research support picture in the mathematical sciences? One can glean an indication from Tables 1 and 2.

TABLE 2

	Estimated Federal Obligations for Research* Performed at Universities and Colleges, Fiscal Year 1981
Engineering	$ 350,208
Environmental Sciences	$ 332,063
Life Sciences	$2,088,893
Mathematics	$ 55,906
Physical Sciences	$ 511,638

*"Research" does not include "R&D Plant," which is defined as follows: "R&D plant (R&D facilities and fixed equipment, such as reactors, wind tunnels, and radio telescopes) includes acquisition of, construction of, major repairs to, or alterations in structures, works, equipment, facilities, or land, for use in R&D activities at federal or non-federal installations. Excluded from the R&D plant category are expendable equipment and office furniture and equipment. Obligations for foreign R&D plant are limited to federal funds used in support of foreign research and development." Ibid, page 2.

Source: "Federal Funds for Research and Development, Fiscal Years 1979, 1980, 1981," NSF 80-318, page 117.

Lest the disparity in funding exhibited in Table 2 be totally attributed to differences in the numbers of professionals in the various areas, we note that in January 1980 the numbers of full-time scientists and engineers at doctorate-granting institutions in the various areas were as follows:

Engineering—20,511

Life Sciences—93,309

Physical Sciences—16,845

Environmental Sciences—5,891

Mathematical Science[1]—9,146

In light of these data, it is not surprising that department chairmen speak of discouragement and of deterioration of the morale of young mathematicians. If it is in the national interest to maintain a healthy and vigorous mathematical sciences research enterprise, then it is imperative that conditions contributing to this deterioration be altered. We discuss the more important problem areas, and for each area estimate the dollar cost of alleviating the problem.

[1] Data from "Academic Sciences: Scientists and Engineers," NSF 81-307, Table B-5. These figures include both research and nonresearch scientists and engineers.

Postdoctoral Positions in the Mathematical Sciences

Table 3 (based upon data in "Academic Science: Graduate Enrollment and Support for 1980," NSF 81–330, Table A-30) gives dramatic evidence of the disparity in the numbers of postdoctorates in various sciences and in engineering.

The excellence of science in the United States today derives from postdoctoral opportunities in the past. Clearly, if the current postdoctoral pattern in the mathematical sciences persists, we will jeopardize the quality of the mathematical sciences at our leading universities in the years to come. To be sure, the pool of outstanding candidates for tenure positions at the leading three or four universities will be large enough. However, one can expect a serious drop in the quality of candidates at the next five ranking universities, and an even more serious drop at the next ten and twenty.

There is a clear need to provide a significant number of outstanding recent Ph.D.s in the mathematical sciences with the opportunity to devote full-time to research in association with a major scientific figure of their own choosing. The most creative future mathematicians in the United States will emerge from this group, and it must be nurtured. We are not advocating a large move away from teaching, the traditional mode of mathematical support. Typically, all graduate students will do some teaching while preparing for their doctorate. However, we are suggesting a small shift to allow promising young investigators a few years after their degrees to develop their research talents.

The Panel estimates that there should be an additional 120 postdoctoral appointments each year, each appointment being for two years. At a cost of $25,000 per appointment per year, this amounts to an increment of $6 million per year. It is also essential, in the view of the

TABLE 3

	Number of Postdoctorates in All Graduate Institutions: 1980
Engineering	981
Environmental Sciences	311
Life Sciences	11,715
Mathematics	143
Physical Sciences	4,261

Panel, that there be flexibility in the nature of the postdoctoral support. A variety of modes should be used in offering it: fellowships in NSF, DOE, DOD; institutional support at major centers and research institutes; enhancement of grants by providing support for postdoctoral positions.

Research Grants in the Mathematical Sciences

Table 4 indicates many categories where funding for the mathematical sciences is markedly insufficient. The amount of dollar support available for graduate students in the mathematical sciences is inadequate. We illustrate this with a very particular example. A member of this Panel recently directed two Ph.D. dissertation students, one in the mathematical sciences, the other in another science. The non-mathematics student was federally supported, and his sole task was to devote his time to study and research. The mathematics student, on the other hand, was university supported and, in addition to study and research, had to devote his time to grading papers, teaching, registering students, and holding office hours. The difference in treatment did not go unnoticed by these students nor, assuredly, did others fail to notice it. Mathematics students need the opportunity for a year or two of uninterrupted research during their graduate study to fully develop their research abilities, as students in most other fields of science do.

Along with decreased research support there is a rapid increase in the demand for mathematics courses in universities. Those who enter and remain in the mathematical sciences must necessarily teach more and devote less time to research. This is now characteristic of the mathematical sciences but not of most of the other sciences.

Because of this dearth of support and less than optimal conditions for the acquisition of knowledge in mathematics, many talented students select other areas for study. As a consequence, the quality and excellence of the graduate student body in the mathematical sciences are diminishing, and, as with the postdoctorates, the implications for the future excellence of the discipline in the United States are ominous.

Implicit in Tables 1 and 4 is that a substantial amount of support for research in the mathematical sciences must be contributed by universities in the form of reduced teaching loads, secretarial services, travel costs, and other aids. This support, as we have noted, is crumbling.

TABLE 4 Bugetary Categories of NSF Awards: FY 1981

	Mathematical Sciences		MPS Directorate[a]	
Budgetary Constraints	Amount	Percentage of Total	Amount	Percentage of Total
Personnel:				
Senior Personnel	$11,710	41.6	$ 23,253	10.2
Postdoctoral Associates	1,699	6.0	17,068	7.5
Graduate Students	1,189	4.2	26,611	11.7
Other Personnel Costs[b]	2,538	9.0	29,027	12.7
SUBTOTAL: Wages, Salaries, and Fringe Benefits	17,136	60.8	95,959	42.1
Permanent Equipment	50	.1	38,550	16.9
Other Direct Costs[c]	2,022	7.2	41,141	18.0
Indirect Costs	8,972	31.9	52,370	23.0
TOTAL: All Budgetary Categories	$28,180	100.0	$228,020	100.0

[a]Excludes mathematical sciences section, MCS.
[b]Includes undergraduate students, secretarial-clerical, other professionals, technicians, and fringe benefits.
[c]Includes domestic and foreign travel, materials and supplies, publication costs, consultant costs, and computer costs.

It is the view of the Panel that an additional 300 graduate students should be supported per year. At a cost of $20,000 per student (including indirect costs) this amounts to an increment of $6 million per year. In addition, the amount allocated in federal mathematical sciences research grants for secretaries, travel, and publication should be increased by $3,000 per individual investigator. This amounts to an increment of $5.4 million per year.

Percentage of Active Research People Supported

We saw in Table 1 that, in fiscal year 1979, 59 percent of the mathematical sciences research done in universities was unsponsored. Since then, the situation has deteriorated even further: *approximately* 200 *active researchers doing high-quality work have lost support during the past two years. Moreover, the research of many excellent new Ph.D.s in the mathematical sciences—more than* 86 *percent—goes unsupported.*

We are not capitalizing on the investment made in the development of mathematical scientists.

To stem the decay and to put a measure of vitality into the mathematical sciences research enterprise require the allocation of sufficient funds to support an additional 500 researchers. At a cost of $20,000 per researcher, including indirect costs, this amounts to $10 million per year.

State of the Infrastructure

The stresses addressed thus far pertain primarily to those that affect the individual researcher or graduate student. Severe problems also exist in the area of communication and interaction between researchers. There is a paucity of mechanisms in the mathematical sciences for maintaining the vitality of researchers working at a distance from elite centers, for generating young people's interest in promising and important new subfields, and for informing and educating the research community about new ideas and results—particularly those ideas and results that lie at the boundary of two, or more, scientific disciplines. Those mechanisms that do exist are in disrepair. Support here is especially important because of the new opportunities made possible by the recent liaisons between mathematics and other sciences.

There has been substantial discussion in the mathematical sciences community concerning means of supporting the infrastructure of the research enterprise. In Table 5 we list some of these means and the amount of funds that the Panel feels will be required per year to support them.

Table 6 is a summary of the per annum dollar amounts discussed above (*not* listed in priority order).

SECTION 3 RECOMMENDATIONS

The mathematical sciences lie at the core of science, technology, and the national defense. An excellent mathematical sciences enterprise does not automatically produce excellence in science and technology and strength in the national defense, but one cannot have quality and strength in the latter without quality and excellence in the mathematical sciences.

TABLE 5

Summer schools, special years, mini-institutes		$5.3 million
Mid-level fellowships		3.6 million
Travel grants, senior research associate programs		4.5 million
Computer time and equipment		2.0 million
	TOTAL	$15.4 million

TABLE 6

Postdoctoral positions		$ 6.0 million
Graduate students		$ 6.0 million
Operating expenses in grants		$ 5.4 million
Increase in number of grants		$10.0 million
Infrastructure (total)		$15.4 million
	TOTAL	$42.8 million

The cost to the federal government of ensuring excellence in the mathematical sciences is relatively very small, the leverage of the dollars invested very large, the ratio of benefit to cost enormous.

In times of economic uncertainty or stress, the implementation even of programs with high benefit-to-cost ratios is sometimes delayed. When this ocurrs, one must also look at the "disbenefit" associated with delay. The current exciting opportunities will not be fully exploited in the United States; the erosion of excellence in the mathematical sciences will accelerate; the disbenefit to our nation of not doing something now is too large to allow the erosion to continue.

The Panel makes the following recommendations:

1. *The federal dollar allocation for research in the mathematical sciences should be increased over the next three years by approximately* 80 *percent—i.e., there should be a total increment over the next three years of* $42.8-*million* (*in* 1982 *dollars*).

2. *The increments should be allocated in proportions deemed appropriate to the* NSF, *the* DOD *agencies, and the* DOE.

3. *The managers of mathematical science funding programs should have sufficient flexibility and freedom to choose the areas of research to be supported and the mechanisms for support.*

The Panel recognizes that it has no formal status as representative of the mathematical community. No single group does. Nevertheless, because it realizes that increased support may be slow in coming, despite the emergency, the Panel tried to give priority to its recommendations as follows.

• If there is no increase, we recommend no changes. The mathematical community, after deliberation, has recently reallocated the resources available to it. It shifted monies into an alternative mode of research support and cut down on the number of individual research grants. It will take time to absorb these changes. We must emphasize again, however, that no increase in support will spell disaster for all but a few of the top mathematical research centers.

• If there is a 10 percent increase ($5.7 million), we recommend that the increment be disbursed approximately as follows:

Allocation	Use
$1.5 million	Postdoctoral Positions (in a variety of modes)
$1.2 million	Increasing the Operating Expense Allocations in Grants
$0.5 million for each of the following:	Mini-Institutes
	Senior Research Associate and Visiting Position in Grants
	Mid-level Fellowships
	Graduate Student Support
	Equipment and Computer Time
$0.25 million for each of the following:	Increasing the Number of Grants
	Travel Grants, Special Years, etc.

• If there is a 20 percent increase ($11.5 million), we recommend that the increment be disbursed approximately as follows:

Allocation	Use
$2.0 million	Increasing the Operating Expense Allocations in Grants
$2.0 million	Postdoctoral Positions (in a variety of modes)
$1.75 million	Increasing the Number of Grants
$1.0 million	Senior Research Associate and Visiting Positions in Grants
$1.0 million	Graduate Student Support
$0.75 million for each of the following:	Mini-Institutes
	Mid-level Fellowships
	Research Institute
	Equipment and Computer Time
	Travel Grants, Special Years, etc.

• If there is a 50 percent increase ($28 million), we recommend that the increment be disbursed approximately as follows:

Allocation	Use
$4.8 million	Increasing the Number of Grants
$4.6 million	Postdoctoral Positions (in a variety of modes)
$4.3 million	Increasing the Operating Expense Allocations in Grants
$3.5 million	Senior Research Associate and Visiting Positions in Grants
$3.2 million	Graduate Student Support
$2.2 million	Mini-Institutes
$1.7 million	Mid-level Fellowships
$1.4 million	Equipment and Computer Time
$1.4 million	Research Institute
$0.9 million	Travel Grants, Special Years, etc.

• If there is an 80 percent increase ($42.8 million), we recommend that the increment be disbursed approximately as follows:

Allocation	Use
$6.0 million	Postdoctoral Positions
$6.0 million	Graduate Students
$5.4 million	Operating Expenses in Grants
$10.0 million	Increasing the Number of Grants
$5.3 million	Summer Schools, Special Years, Mini-Institutes
$3.6 million	Mid-level Fellowships
$4.5 million	Travel Grants, Senior Research Associate Programs
$2.0 million	Computer Time and Equipment

ATTACHMENT 2

REPORT

by

THE SUBPANEL ON THE DEPARTMENT OF DEFENSE

of

THE MATHEMATICS BRIEFING PANEL

Committee on Science, Engineering, and Public Policy
National Academy of Sciences

Members of the Subpanel:

Dr. Hirsh Cohen (*Chairman*), T. J. Watson Research Center, IBM Corporation

Professor William Browder, Princeton University

Professor Julian Cole, Rensselaer Polytechnic Institute

Professor Bradley Efron, Stanford University

Professor James Glimm, Courant Institute of Mathematical Sciences

Dr. Ronald Graham, Bell Telephone Laboratories

RESEARCH BRIEFING ON THE MATHEMATICAL SCIENCES OFFICE OF THE UNDER SECRETARY OF DEFENSE FOR RESEARCH AND DEVELOPMENT

JULY 7, 1983

I. Summary and Recommendations

For the Department of Defense, progress in the mathematical sciences is a vital component in achieving the technologies that will produce the strongest defense at a minimum cost; for the mathematical sciences, the Department of Defense is a principal source of funding. Continued contributions to science and technology by mathematics require attentive support.

This report is a supplement to a research briefing prepared for and presented to the Office of Science and Technology Policy in October 1982. Particular attention is given here to the extremely productive relationship that has existed for thirty-five years between the Department of Defense scientific agencies and the mathematical sciences. Examples of significant contributions through new mathematical concepts, methods, and important applications are noted: asymptotic diffraction theory in radar identification and underwater acoustics, mathematical fluid dynamics, reactive flows, control theory in aerodynamics and fire control, signal processing, reliability theory, linear programming. The increasingly important role of computation is described. Many of these contributions were made by a very direct approach to the Department of Defense application, seeking new methods of analysis and computation; others resulted through the unpredicted benefits of mathematics research in other contexts.

Examples of current topics of mathematical research that, among many others, show high potential are in the fields of transonic flow, turbulence, nondestructive evaluation, new approaches in statistics and combinatorial optimization, distributed control, VLSI, data transmission and spline approximations.

In the OSTP report, evidence was cited that the support structure of the mathematical sciences has been decaying. This means that the successful relationship of mathematics to government needs is threatened. The recommendations to the Department of Defense for improving the current deterioration are:

A. Protection of 6.1 core funds and greater flexibility in program direction by the scientific program offices.

B. Increased funding for: (i) training of graduate and postdoctoral fellows, (ii) computing equipment, time, and software, and (iii) research grants.

C. A senior advisory committee on the mathematical sciences, reporting to the Office of the Under Secretary of Defense for Research and Development.

II. Introduction

It is our belief that the overall health of the mathematical sciences, as well as the specific products of its research efforts, are important to the DOD. The creativeness and productivity of mathematical research strongly affect all other scientific disciplines and both through them and independently affect the development of technology.

In this supplement to the briefing document produced at the request of the Office of Science and Technology Policy in October 1982, we will discuss the achievements, the potential and the problems of mathematical research from the perspective of the DOD, its history, structure, and mission.

In part III, we shall first cite examples of mathematical research of the past thirty years that are significant as mathematics and have been particularly important for the DOD. Some of these achievements could not have been perceived at the time to be of immediate relevance to the DOD mission, but much of this research was supported by the DOD. We then give examples in several different areas of current research with both immediate payoffs and long-term potential. As has always been the case in mathematics, there will be unanticipated and seemingly fortuitous contributions from current work to future specific applications problems. There are other areas of research in mathematics in which the initial motivation and formulation lie within one area of application and, as the ideas are worked through, understood and generalized, they become useful far more broadly.

In part IV, we note the recommendations of the OSTP Mathematics Briefing Panel and interpret them in the DOD context. We also give our views on the role of the scientific program officers in the DOD scientific funding agencies and we propose a new advisory committee aimed at further improving the relationship between the mathematical sciences community and the DOD.

Throughout this document, we will discuss research in mathematical sciences, particularly applied mathematics, probability and statistics, and the mathematics of operations research. We do not discuss computer science, but we will discuss the mathematics of computation which, obviously, has important relations to many aspects of computer science and to computational equipment.

It is our firm belief that increased funding and the new approaches we propose will provide the DOD with a close and productive relationship to the mathematical sciences and, in doing so, will provide direct contributions to new technologies, avoid technological surprises, and facilitate break-throughs.

III. Mathematical Research, Past, Present and Future, and the DOD Research Mission

In the OSTP Briefing we have recounted some of the important accomplishments in our times in the mathematical sciences. Ranging from the fundamental work of Wiener and its contributions in signal processing to the conception of the stored program computer and the modes of using it by von Neumann and many others, these accomplishments extend into every aspect of modern science and technology as well as extending the frontiers of man's conception of the universe and the philosophic context in which science takes place.

The role of the DOD in the support of this research has been notable, extending across the whole range of mathematical activities. For some years, the DOD, beginning with ONR, was the first and for some time the only agency sponsoring research of the most deep, basic and significant type, across the whole spectrnm of scientific disciplines. With the founding of the National Science Foundation, the support of the DOD became increasingly focused on applied mathematics and certain closely related areas of pure mathematics. The Mansfield Amendment in 1968 reinforced this focus and produced a demand for a clearly perceived relation of the supported research to specific mission goals of the agency. Although the Mansfield Amendment may no longer be the operative force, these demands continue to deny the DOD program the flexibility and breadth needed to respond to the new opportunities and developments which arise, unanticipated and without plan, from progress in the broadest areas of basic research in mathematics.

In the paragraphs that follow, we will first give some examples of significant contributions of the mathematical sciences, with importance to the DOD missions and supported by the DOD. Then, we will cite some topics of research that are currently exciting and which point the way towards some areas of focus in research in the mathematical sciences in coming years. One should bear in mind, however, that judging by past history it is difficult to predict which topics in mathematics will come to bear on future problems and, for this reason, as we shall discuss later, a very broad view must be maintained.

A. Significant Contributions

Asymptotic diffraction theory has been developed over the past twenty-five years. Because its initial goal was to provide solutions for radar reflections and scattering patterns it was formulated within electromagnetic theory. It has been used extensively, as well, in underwater acoustics where it has been extended to account for the kind of random media encountered in underwater surveillance problems. In the next section we will describe some current uses of the theory.

Other areas of physical mathematics include the major work done in bringing plasticity theory from initial mathematical formulation all the way through to numerically calculable design methods. The theory has application in naval, ground, and aeronautical structures and vehicles of many kinds in understanding heavy loadings beyond the elastic range. Ocean and ship hydrodynamics have been strikingly advanced mathematically, including the behavior of shallow surface waves near beaches, tsunamis, tidal waves and design calculations for ship hulls and hydrodynamic cavitation. Problems of reactive flows which occur in combustion, detonation and flame propagation are at the heart of propulsion systems and of many types of weapons.

Almost all of these problems are governed by nonlinear partial differential equations, the understanding of which has been the subject of extensive research, particularly during the past twenty years. In these problems, the techniques of singular perturbations and the development of bifurcation theory have been important. These techniques, developed and perfected by applied mathematicians and analysts, evolved out of fluid dynamics (boundary layer theory) and elasticity (structural buckling). Continued support of mathematical investigations which combine physical mathematics and research in nonlinear analysis will make it possible to attack a wealth of unsolved applied problems.

Control theory has been extensively used in aerodynamics, space, and fire control. It has had many applications as process control in manufacturing and in many kinds of operational machinery. Its origins lie deeply within the variational calculus, and early formulations of control theory relied on methods that came directly from that part of mathematics. We discuss some current work and future directions in control in the next section.

Signal processing is ubiquitous in military communication, information gathering, and detection. The development of the fast Fourier transform over the past twenty years has been vital to advances in signal processing. Its application is absolutely everywhere from radar and sonar, to modems and laboratory data analyses. Mathematical understanding of the FFT in terms of the complexity of calculation has led to new and more efficient transforms. Some of these have been number-theoretic based. Complexity theory has evolved over the past twenty years, stimulated by applications such as this and in computer design, mathematical programming and, in fact, many years of computation. Number theory has also prominently appeared in cryptography with the use of prime number methods and the diophantine approximations. Another aspect of mathematical complexity theory, the delineation of problems that can be calculated in polynomial time, that is as a power of the number of numerical elements in the calculation, or nonpolynomibreak ally (calculations that grow exponentially) has been exceedingly valuable in understanding what is really meant by hard and long calculations.

In statistics, reliability theory, motivated by both military and industrial applications, has come into constant use in all manner of ways. It has had theoretical development to form a good mathematical base and it is now developing calculational methods of great power. We describe some current work and future directions in statistics in the next section.

Finally, we must mention the impact of mathematical programming. Linear programming was born in the logistics and supply problems of World War II. The invention of the simplex method and its initial, successful applications led first to gradual and then swift increases in the kinds of application and to more efficient modes of calculation. Linear programming is a part of virtually all commercial, manufacturing, and military activities. It has saved millions of dollars in designs and operations. Nonlinear and integer programming have had both theoretical and practical study. In just the past few years new insights into mathematical programming have been generated by the discovery of the ellipsoid method which stimulated, in turn, the exciting proof that an average linear programming calculation grows in calculation time only linearly with the size of the problem.

Throughout this description of some of the successful contributions of mathematics we have mentioned computational mathematics of several kinds. It is obvious that the role of computers and computation in all of scientific research has been enormous. In each of the mathematical areas mentioned above—analysis of differential equations, combinatorics, statistics, signal processing, mathematical programming—numerical calculational techniques have been developed rapidly. In scientific calculations, for example, the use of sparse matrices, finite element methods, splines, multi-grid and adaptive grid techniques, random number (Monte Carlo) and random choice methods have all been supported by the DOD as they have been developed and used extensively on DOD problems and in industry by mathematicians, scientists, and engineers.

Computation will play an increasingly important role in mathematics itself and in all of science and technology. Research into more efficient, cheaper, and faster methods for current and future machines is vital. In a recent National Research Council study on a computational wind tunnel there is a call for a 30% increase in hardware and software capability *and a 30% increase in speed from numerical analysis and algorithmic development.* There is little question that the mathematics of computation must be developed with full support of all the government agencies.

B. Current Topics

1. Physical mathematics and nonlinear analysis

i. Transonic computations and shockwave calculation

An example of profound interaction of mathematics and practical technology is the development of methods of calculating transonic flows with shocks, stimulated by the needs of aircraft designers. Mathematically it depended on very early studies of the numerical solution of elliptic and hyperbolic differential equations (e.g., Courant, Friedrichs, and Lewy in 1928). Early theoretical studies on partial differential equations of mixed type by Tricomi (1923) gave a useful background. The work of Guderley (1953) and related research gave a good understanding of the basic physical ideas of transonic flow, but progress was limited by the inability to calculate.

With the development of large-scale computers it was possible to use new numerical methods and to include discontinuities (shocks) in the numerical algorithms. This, together with the development of type-sensitive difference schemes, made practical calculations possible. At the same time, Garabedian was able to do hodograph calculations and, therefore, to obtain, by calculational inverse methods, shock-free airfoil designs. These calculation methods are increasingly important for understanding the results of modern wind tunnel testing.

The increasing needs of technology, however, have led to a whole new series of mathematical problems, including generation of computational grids for 3-dimensions, fast solution algorithms, and the use of interactive graphics.

While the methods developed thus far are successful for analyzing aircraft in cruise and are widely used to reduce wind tunnel and flight test requirements, the problem of aircraft in combat is more complex.

ii. Turbulence

The understanding and calculation of turbulent flow fields is a long-standing fundamental problem of science. Turbulence affects the design of many defense-related systems, for example, aircraft, missiles, and submarines. Turbulence affects communications and atmospheric and ocean currents. It is fair to say that thus far no basic theory or methodology exists to provide a fundamentally sound method of calculation. Turbulence is a complex problem but hope for progress toward a solution can reasonably come from developments in several areas:

1. New theoretical ideas—Dynamical systems and mathematical chaos theory give "random" solutions to deterministic systems; coherent structures and vortices have been observed experimentally; fractional dimension singular sets have been understood and look promising.
2. Better numerical algorithms for systems such as Navier-Stokes will continue to be developed.
3. Faster computers could be effectively used for the large scale of computation necessary.
   The pay-off in this area is likely to be long range but is of enormous importance for the DOD and for science in general.

iii. Nondestructive evaluation testing

For many years, tests have been available that show the presence of cracks, flaws, or other imperfections in solids. More detailed quantitative information on the nature of imperfections, size, shape, orientation and, therefore, on reliable lifetimes or predictions of time to failure can be obtained if the inverse scattering and source problems can be solved. Scattering of ultrasonic, x-ray, or neutron radiation or passive capture of emitted energies must be analyzed. The mathematical techniques of asymptotic and ray diffraction theory, developed originally, in fact, under ONR support and aimed, as we have noted, at radar target interpretation, are now being applied in nondestructive testing. In this case, elastic waves are studied and new asymptotic formulations are being developed. The applications to aircraft, ships, land vehicles, etc., are enormously broad.

This is one example of an inverse problem and one mathematical mode of analysis. Another inverse problem, computer-aided tomography, is closely related to the Radon inversion formula which is being adapted for the accuracy needed and fast, efficient calculation. The mathematical problem involves constructing a function of two variables from line integrals. Seismic analysis and underwater sound wave propagation and reflection are other examples of important inverse problems.

These three topics are examples of the application of nonlinear analysis, differential equation theory, and the development of new modes of calculation. There is obviously a great deal more to be done in the physical mathematics of nonlinear problems. It has always been a field of most successful contributions from mathematics to all of science and technology and it will continue to be so.

2. Statistics

Following the postwar heyday of decision theory, mathematical statistics is gathering momentum for another move forward. The line of advance involves the electronic computer, which is now not only very fast, but very cheap.

Most of the commonly used statistical methods, analysis of variance, linear regression, maximum likelihood, etc., were developed under the constraint of slow and expensive computation. The dramatic computational improvements of the past 30 years, by several orders of magnitude, are comparable to the transition from naked eye astronomy to the telescope. New statistical theories which can take effective advantage of all this computing power are just starting to emerge. Projection pursuit and the bootstrap are two such methods which have attracted considerable interest among statistical practitioners.

A traditional statistician looking at those new methods is struck both by how powerful and how weak they are; powerful in their freedom from Gaussian assumptions and linear mathematics which dominate the older theory, but weak in their theoretical underpinnings. The developers of classical statistics gave mathematical proofs that their methods were *best*, at least within the narrow framework of Gaussian assumptions. Current research, which is a combination of mathematical and computational exploration, is trying to find and prove optimally in a much broader setting. Both the prospective payoffs and difficulties seem enormous.

An example of the possible enormous payoffs of improved statistical methods is the test firing program for the MX missile. With conventional statistical techniques a minimum acceptable confidence level of 72% would require 36 test firings in Phase I and the total sample size in all phases would have to be greater than twice the planned deployment size. With a new and different statistical approach based on Bayesian techniques in reliability, the Phase I test firing size has been reduced to 25 with an increase in reliability from 72% to 93% and an estimated direct cost saving of $250 million.

New techniques and new approaches mean that statistics will play a very strong role in many aspects of the DOD technologies in the future.

3. Combinatorial optimization

Systems composed of many interconnected and communicating components occur in a wide variety of situations. These can range from voice and data networks for globally distributed locations (such as command posts) to highly complex VLSI devices formed from tens of thousands of components placed on a silicon chip. Among the numerous critical and extremely difficult problems which arise from this area are those of partitioning, routing, and placement of the individual components. These are typical of the large class of combinatorial optimization problems which have been studied for many years. Recently, a synthesis of efforts by mathematicians, computer scientists and physicists has led to much more efficient ways to attack some of these problems and has suggested a deeper connection between these problems and phenomena occuring in statistical mechanics.

A natural technique to try for these optimization problems is that of local improvement, e.g., a sequence of small changes, each of which improves the overall solution. It turns out that the effectiveness of this technique can be strengthened enormously by occasionally allowing the changes to make the solution somewhat worse, but in a careful way. This was the underlying insight in the Kernighan-Lin heuristic algorithm for solving the infamous Traveling Salesman Problem.

It is also a key component in recent network optimization algorithms for large data and voice networks, for example, which have resulted in very significant cost reductions for users of the network.

It turns out that this idea is also the basis of an exciting new development for dealing with combinatorial optimization problems, which goes under the name of "simulated annealing." Using techniques very similar to those coming from statistical mechanics for studying the way in which alloys solidify, it appears to be possible to obtain very good solutions to a variety of routing and placement problems which are both easy to find and, at the same time, nearly optimal. This whole approach clearly demands further investigations in order to understand just what is really going on.

These new concepts and the techniques that derive from them point to high yield in many problem areas of direct concern to the DOD.

4. Large-scale control systems

Large-scale control systems include those arising out of systems modelled by partial differential equations, control problems involving extensive networks, such as occur in power transmission systems, e.g., hierarchial systems, involving many layers of organization with correspondingly delegated responsibility for monitoring and control. Many other types of systems, more or less related to these, could be cited. These systems occur in control applications to extended elastic structures, chemical processing complexes, electrical power and telephone networks, large systems of coupled electrical or mechanical devices, etc.

Over the last two decades, a great deal of progress has been in developing the distributed parameter control theory pertinent to the wave and heat (diffusion) equations. The study of the control theory of hyperbolic and parabolic constant coefficient systems has shed new light on the relevance of earlier studies by Paley and Wiener, Ingham, Levinson, Schwartz and others, in the area of completeness and independence of sequences of complex exponential functions and has spurred significant new contributions to this classical field of study.

The intimate relationship between control and stabilization has led to very significant additions to the body of knowledge concerning asymptotic stability of infinite dimensional systems. This has been particularly evident in extensions of the Liapounov theory, making use of controllability concepts and developing the theory of operator equations of Riccati type arising in the linear quadratic optimal control theory of systems governed by partial differential equations.

Work in this area has also stimulated renewed interest in the modelling of large systems. An exciting and hard problem of present interest is the behavior of large space structures. This is a challenge for distributed control theory and to structural dynamic elasticity theory.

It is quite clear that distributed control of large systems offers a number of mathematical challenges and lies at the heart of many technical requirements of the DOD.

5. Other topics

i. Electronic technology

The DOD has been involved, almost from the beginning, in the support of semiconductor technology. Although there is a large industry moving this technology along rapidly, in recent years the DOD has sponsored the VHSIC program to be sure its own needs will be met. The mathematical aspects of semiconductor technology span a very broad spectrum that begins with the analysis and calculation, from the Schroedinger wave equation, of generation and recombination functions and Fermi energy levels and goes all the way through to the logic and combinatorial calculations required for chip design and wiring. In the latter case, some marvelous relationships (which we have referred to earlier in the comments on combinatorial optimization) between calculational methods in statistical mechanics and the combinatorial optimization required for chip design have recently come into use. The analysis of the nonlinear partial differential equations of electron-hole behavior in the transistors that populate chips is one of the mathematical topics that uses many of the qualitative, asymptotic, and calculational techniques that have been developed in widely different kinds of mathematics research. A major problem in these calculations is to handle regions in the devices in which large changes (in charge or current) occur in very narrow geometries. This requires multi-grid and stiff equation methods numerically and singular perturbation techniques analytically. The very large circuit analysis problems in chip design are the original stimulation for considering sparse matrices. Many hard, poorly defined mathematical problems in logical and physical testing remain in this area as challenges for combinatorialists, analysts, and logicians.

ii. Symbolic dynamical systems and data transmission

Mathematical studies of ergodic theory and dynamical systems have evolved symbolic methods for describing the behavior of the mappings that describe such systems. These methods have themselves become known as symbolic dynamics and play an important role in the abstract mathematical analysis of dynamical systems. The symbolic or coding methods for a special class of symbolic models, the topological Markov shifts, together with an isomorphism theorem for these shifts, have been shown to be exactly the codings required for several very practical technological applications. They are applicable to magnetic recording and storage of data and to data transmission by lasers over fiber optic channels.

In the magnetic recording case, the problem is to code arbitrary bit sequences into new sequences which do not violate physical limits of the magnetic devices; namely, the capability of recording the flux changes and the speed of the electronic clocks. The objective is to decrease interference between the analog signals as they are read from the disk. The coding must be done in a manner that yields new sequences that can be accurately decoded to retrieve the original bit sequence. Symbolic dynamic systems theory ensures the equivalence of the source sequence to the finally decoded sequence through theorems involving topological entropy. The theory actually produces a prescription for constructing the correct codes. The result is that higher information density can be achieved for the magnetic disk storage.

iii. Curves, surfaces, computer graphics and spline functions

The problem of the mathematical representation of curves and surfaces occurs in many situations where shapes of objects have to be manipulated and analyzed. Computer-aided design and manufacture (CAD/CAM), design of aircraft and automobiles, and mapmaking are obvious examples, but the problem is universal.

A basic problem in CAD/CAM where classical methods fail is the construction of a smooth curve or surface to a designer's specification. "Construction" means the development of a mathematical formula that can be evaluated to give any particular point on the curve or surface for plotting purposes, for the composition of complex objects from simple ones, for the machining of the surface with the aid of automatic milling machines, etc. The design takes place interactively. A rough outline of the curve is made, typically just a sequence of points in the plane, and it is left to a computer program to come up with a smooth curve that looks like the sketch. This curve is then modified locally and globally until it fits the designer's original concept. Local flexibility and faithful reproduction of the overall proposed shape are the main requirements of the mathematical curve or surface descriptions used in such programs.

Today's standard method which satisfies these requirements is Schoenberg's variation diminishing spline approximation. A spline is a piecewise polynomial with smoothness properties—named after the flexible strips used by draftsmen to design smooth hulls for ships before the age of computers. The mathematical study of splines was started by I. J. Schoenberg in the 1940s, in his studies of smooth curve fitting specifically to fit data from ballistic tables. It has been strongly supported in its development by the DOD.

The variation diminishing property is based on the use of B-splines and on total positivity, concepts that have come out of different fields of mathematics. These concepts, in turn, have stimulated new and impressive mathematical studies. Present mathematical efforts concentrate on the fitting of smooth functions to scattered data in two and three variables. This whole area is a challenging one and of great practical interest.

IV. DOD Support of Research in the Mathematical Sciences

The DOD is the major federal agency supporter of basic research in applied mathematics (including probability and statistics), in the United States. In fiscal 1983, for example, the extramural DOD budget for this was $26.3 million, as compared with an NSF budget of $7.6 million in these areas. DOD research policies, as a result, have a major effect on the development of the subject as well as contributing to the missions of the DOD. These two roles are not in conflict. For the building of the strongest, most advanced and technologically sophisticated defense, at the lowest cost, the DOD needs a research enterprise of the strongest and most vigorous type in the United States in the broadest areas of science and mathematics, as well as ready access to it.

A. Mode of Research Direction

The evolution of the DOD funding pattern has led to some inherently unhealthy conditions for DOD-sponsored research in the mathematical sciences.

There has been a progressive narrowing of the range of DOD-sponsored research, particularly in the years since the Mansfield amendment. The necessity in many cases to explicitly link sponsored research to a particular mission of the armed services, by its nature, makes it difficult to support more visionary, and in the long run, more significant research. It inhibits the ability of the program managers to respond to the dynamics of research in the subject areas.

"These programs must retain their fundamental, long-term nature and... there must remain sufficient flexibility to program officers, who have the best technical understanding of their fields, to explore new opportunities as well as general disciplinary research issues they judge to be important to the DOD, without special management approval." Dr. Richard D. DeLauer, before the Subcommittee on R&D of the Committee on Armed Services of U.S. House of Representatives, 22 April 1983.

While agreeing strongly with these sentiments, we see little implementation of this approach in the DOD scientific program offices. The ever-increasing constraints imposed by the special initiatives have decreased the flexibility of program managers. Unless the funding for basic, core programs is protected, one may anticipate further declines in the breadth and flexibility of this support, and a decrease in the overall quality and the long-range impact of supported research.

For example, an arbitrary minimum of $1 million for special initiatives seldom makes sense in mathematics. Mathematics is small science, with many individuals working for the most part alone. Such an arbitrary minimum may sometimes create a situation where an important special initiative must either be foregone, or that more money must be spent on it than necessary, at the cost of other important basic research.

We, therefore, recommend that the scientific program officers in the mathematical sciences be given renewed control and flexibility in managing their programs. We also recommend that increases in funding flow through into the core programs and that there be a reduced effect of initiatives. We are confident that the scientific programs officers will generate programs that will produce the mathematics needed for both the short-range and the long-range needs of the DOD.

B. Funding

In the OSTP Briefing we indicated evidence of the general deterioration of the support structure of mathematical research in the United States, and gave recommendations for stopping this deterioration and renewing the foundations of this very successful research enterprise, the strongest in the world. We have recommended, in general, that support be increased in the mathematical sciences in these four areas: postdoctoral grants, graduate fellowships, research grants, and support of the infrastructure (symposia, work shops, computer equipment, etc.). We believe that the DOD should share in this increase in support so that the full promise of the exciting research momentum we have described can be attained.

The general recommendations have begun to the implemented in the NSF budget for 1984, which shows an overall increase for mathematical sciences of 24%, and an increase of 30% in the subareas of applied mathematics and probability and statistics. In contrast, at a time when the DOD research budget is increasing at a very rapid rate, the DOD budget shows only a very small increase in the extramural basic research budget in the applied mathematics, probability and statistics areas.

The goal of a substantial increase in DOD support would fall into *three* general areas of central interest to the DOD:

1. Training

> The DOD has a tremendous interest in enlarging and upgrading the pool of mathematically qualified personnel in the United States at the research and highest technical levels in the mathematics sciences. There will be a growing need for the people to produce the formulation of new technical problems, the development of new techniques for solutions that have their sources in many fields of mathematics itself, and for people to carry out exploratory, design, and operational calculations with a high degree of mathematical sophistication. In spite of the fact that funds have been made available in several of the DOD scientific agencies for predoctoral fellowships, very few of these have gone to the mathematical sciences. The fellowship program has narrow constraints placed upon it so that, for example, pressing needs for more graduate students in the mathematics of computation are not being met. A large increase in postdoctoral associateships in grants, together with enlarged graduate student support, would contribute to increasing the numbers and quality of our pool of this talent, in the way most in keeping with the existing DOD modes of support.

2. Equipment: computers, software, and computing time

The increasing role of computers in conducting applied mathematics has been described in the examples given earlier. To mathematicians, the computer has become the laboratory for both research itself and the training of young people. This is also an area in which DOD has made funds available but, again, the criteria are so restrictive that the needs of the mathematical researchers are being poorly met. The new funding is not usable, for example, for cycle time payments or for needed software. The program needs to be interpreted more broadly and funding needs to be increased so as to gain far greater effectiveness from the research.

3. Increasing grants

A substantial increase in the number and size of grants in the core areas is needed, to broaden and deepen the contact of the DOD with important areas of mathematics. For example, the area of combinatorics in which the DOD support played a historically important role has all but disappeared from its grants, as well as large areas of nonlinear analysis, which are being squeezed more and more in the DOD budget. In the preceding section we have cited promising developments in these topics and in a number of other topics.

We finally believe that investment by the DOD in applied mathematics will have important scientific and technological yields. To catch up with the needed levels of expenditures in these areas, we recommend an overall increase of $24.5 million over three years, divided roughly as follows:

1.	Training	5.8 million
2.	Equipment	3.7 million
3.	Increase in grants	15 million

All of this enlarged scope for support in the mathematical sciences will provide an otherwise unobtainable effect for the DOD: namely, a close relationship to the mathematical sciences community. In particular, it will assure the DOD of attaining its major aims for science—to provide direct contributions to current and future defense technologies, to avoid technological surprises in future years and to facilitate breakthroughs. We recommend that this increase in support take place over the next three years.

C. The Role and Responsibility of the Mathematical Sciences Community

We would like to propose that the mathematical community, for its part, play a more active and responsible role in the DOD program in the mathematical sciences. We believe that it is important for us to understand the DOD scientific aims and goals, the near- and long-range requirements for technology, constraints on funding and the other elements that go to make scientific policy. We believe that mathematical scientists who are supported by the DOD should understand the sources of the problems they work on so that they can be sensitive to their own findings and discoveries in mathematics.

We also believe that the DOD needs as much light as possible shed on new developments in the mathematical sciences. One or another of these may just fit a pressing requirement. And, the DOD needs to be assured that the training of new people in both old and new branches of mathematics is proceeding well. Specifically, to exercise this responsibility we propose the formation of a Mathematical Sciences Advisory Committee reporting to the Office of the Under Secretary of Defense for Research and Development. Senior members of the mathematical sciences community would be asked to serve for a term to regularly advise and consult with those DOD officials and managers who are concerned with scientific policy, management, and funding. These will be mathematicians representative of broad areas of mathematics and of the professional societies.

Among the functions of such a committee would be:

- an understanding of important defense technologies requiring mathematics and computation and suitable transmission of these problems to the community

- advising and reporting on new methods, new topics, and new insights in areas of the mathematical sciences
- reviews of funded programs
- recommendations of mathematicians to fill program officer positions

Typical issues that such a committee might discuss with the DOD management are:

- the role of mathematics in large-scale scientific computation
- the quality of mathematical work in the DOD laboratories
- the role of the DOD in training people for work in the mathematics of computation and other emerging fields

In summary, our recommendations are:

A. An implementation of the DeLauer scientific management policy at the level of the scientific program officer, emphasizing the flexibility to meet the dynamic demands of the fields according to their special characteristics. Beginning with the 1985 budget, increases in mathematical research funding should flow, for the most part, towards the core programs.

B. An increase, over 3 years, of $24.5 million in extramural basic research funds in mathematics (for the support of training, equipment, and research).

C. The appointment of a mathematical sciences advisory committee, reporting to the Office of the Under Secretary of Defense for Research and Development.