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Committee on Opportunities for Research
in the Geological Sciences

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Commission on Physical Sciences,
Mathematics, and Resources

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PREFACE

The Director of the Division of Earth Sciences, National Science Foundation (NSF), requested the Board on Earth Sciences (BES) "to prepare a report on the state of the science and to recommend policy for the decade of the 80's relevant to the academic community and NSF." The BES endorsed this request at the meeting on April 30 and May 1, 1982. The Committee on Opportunities for Research in the Geological Sciences was appointed to accomplish this assignment.

A workshop, which had been held in 1981, at which twenty-six participants covering a broad range of subfields reviewed the geological sciences with a perspective of a decade or more, served as background for the study requested by NSF. A summary of discussions at the workshop is presented in Geology and Our Future: Summary of a Workshop Report.

A statement which solicited comments about the needs and outlook for the geological sciences was sent to twenty-four professional societies for publication nationally. Letters to 140 departments of geological sciences in academic institutions throughout the United States stated the purpose of the present study and requested comments. Similar letters were sent to the Chairmen of Committees and Boards of the National Research Council whose activities are related to the geological sciences, to members of the Sections of Geology and Geophysics of the National Academy of Sciences, to the heads of governmental agencies doing fundamental work in the geological sciences, and to all State Geologists. In sum, more than one hundred individuals contributed to the formulation of this report (see Appendix A).

Scientific importance, timeliness with respect to developing science and technology, potential significance to society, and current budgetary constraints were considered in selecting topics for discussion in this report. It should be noted that because this report was initiated at the request of the National Science Foundation, we have placed primary emphasis on those areas of earth-science research that have typically been supported by the Foundation. Other areas that may have equal societal relevance and scientific excitement, such as radioactive waste disposal and earthquake prediction, are not emphasized in this report because they typically have been supported by other federal agencies as mission-oriented projects. However, fundamental research as described in this report is applicable to many pressing societal problems. The assessment of the field was deliberate, and the priorities and recommendations are made to improve that knowledge of the earth which sustains our society.

William R. Dickinson, Chairman
Board on Earth Sciences

CONTENTS

RECOMMENDATIONS

At no time in history have the geological sciences offered more potential for mankind than today. Developments and breakthroughs are increasing our fundamental understanding of earth processes and structures, affording many practical applications in the search for resources and the use of land and water.

The geological sciences are bristling with excitement, and stimulated by new concepts with related controversy and debate. Previously accepted models of geology are being tested regularly with new measurements and new analytical capabilities. Accepted concepts of the earth's crust are being challenged in ways that have ramifications for resource exploration as well as for the basic understanding of geological processes. Models used to estimate resources and to aid in land-use planning are in need of continuous revision with improved understanding of the dynamics of the earth. The global plate tectonic model, the availability of improved instrumentation, and the importance of understanding the earth to give substance and safety to an increasing number of inhabitants of the earth combine to provide unique challenges and opportunities for geological research in the 1980s.

This report examines those research opportunities that are pertinent to the programs of the National Science Foundation's Division of Earth Sciences. Although the committee wrote most of the report, many of the ideas and statements were provided by a wide circle of earth scientists in the United States. Research priorities for the NSF's Division of Earth Sciences have been developed for the decade of the 1980s using all available information.

The recommendations presented in this chapter are of two types: those related to policy, research requirements, and budget; and those related to research strategy and priorities.

A. PROGRAM AND BUDGET REQUIREMENTS

1. Research Policy

The NSF's Division of Earth Sciences should pursue a philosophy of synergistic programs. A large mix of research is recommended in this document because a narrow approach to priority research topics (Chapter 1, section B) would fail to reach the desired goals. Increased funding for basic research is fully justified by the potential discoveries, and the additional effort should be an essential part of national science policy (p. 81).

1. Priority should be given to the scientific programs that will make the most progress in the light of scientific breakthroughs and technical developments.
2. Planning for research programs should include the realization that advances from a proper mix of efforts can be greater than the sum of the research results from individual grants and projects.
3. Funding should emphasize basic research. The activities of other sectors, including mission agencies, industry, and foreign investigations, should be taken into account. The implicit relationship between scientific research and societal needs must be recognized.

2. Research Goals

The NSF's Division of Earth Sciences research programs should be directed at obtaining a new level of understanding of the structure, composition, energetics, and evolutionary history of the earth with emphasis on the continental lithosphere and its margins. The highest priority research in this report attacks the continental lithosphere (see Chapter 1, sec. B). Improved methods of measurements, interpretation, and synthesis present opportunities for large advances in understanding the earth, including natural hazards and mineral resource distribution.

3. Research Requirements

The recommended research goals will require expanding support for some programs and taking new initiatives for others. Requirements (pp. 79-81) for attacking the research priorities (Chapter 1, section B) are as follows:

1. Standard Research Grant Program. The individual research grant program is the backbone of the Foundation's funding. The individual grants are now inadequately funded and have low graduate student participation. Grant allocations should be more than doubled (p. 77-79). The research program should be biased toward the research priorities given in this report.

2. Major Experimental Projects and Facilities. Medium- and large-scale consortium-type projects have special requirements because they need support exceeding that obtainable by individual research grants and because special management and services are required to sustain their operations (p. 73-74).

2a. New Initiative: A large array of portable digital seismographs is needed to define heterogeneities of the lithosphere by variations in the propagation paths and character of seismic waves from controlled and earthquake sources. This method and the deep reflection seismic method provide the most direct data on variations of geologic features with depth in the earth. The initial program will cost NSF about $1-2 million annually (p. 75-76).

2b. Augmented Initiative: Deep seismic reflection studies are providing new insights about the details of geologic structure at depths to 40 km. This is a major new tool for looking at the deep crust, which is a research frontier. This very successful NSF project is currently supported at $3-4 million per year. The funding for seismic reflection studies should be doubled (p. 73).

2c. New Initiative: Deep continental drilling provides the only means of directly measuring rock properties, establishing geologic structure, and observing earth processes at exceptional depths. It provides the observational data to confirm or deny inferences made from near-surface observations. We recommend that a program of continental drilling be initiated at $4 million per year (p. 74-75).

2d. New Initiative: The global system of permanent seismographs, of which the U.S.-supported Worldwide Standardized Seismograph Network (WWSSN) is a major part, provides data on the internal structure and composition of the earth as well as the characteristics of earthquake sources. The large majority of stations have analog-recording, which constrains data analysis. The development of digital seismograph equipment and the consequent increased capability in processing and analysis of digital data offer the potential for large advances in our knowledge of the earth. Some expansion of seismic research using current facilities is allowed in the recommended budget of Table 1, but no funds were allocated for the development of an advanced global digital seismograph network. Planning for such a network should be initiated (pp. 75-76; p. 45).

3. Laboratory Instrumentation. The inadequacy of laboratory instrumentation in American universities has been documented. For essential widely required instrumentation, such as electron microscopes, high-pressure and temperature equipment, etc., the NSF's Division of Earth Sciences is allocating about $5 million per year. This amount is inadequate and should be increased severalfold (pp. 70-71; p. 42).

3a. New Initiative: A program needs to be established to put expensive equipment such as ion microprobes and accelerator-based mass spectrometers in a few locations that are committed to be available for many users and that have dedicated support staff. A pilot program will require $1-2 million annually (p. 73-74; p. 42).

4. Data Management and Facilities. Rapid advances in digital computers and their extensive use in acquiring, manipulating, and storing data have made it possible to address problems that previously could not be attacked. Data management must be addressed by the scientific community, and facilities to handle large quantities of data must be made available. Many complex problems involving comprehensive data sets are amenable to solution only through using large computers. These investigations are usually at the cutting edge of science, and computer time should be made available for the required analysis of data. Table 1 implicitly allocates funds for work of this nature but does not allow funds for a dedicated facility though one may eventually be needed (p. 45; pp. 72-73).

4a. New Initiative: NSF's Division of Earth Sciences should support the development of dedicated data centers as they are needed. Problems in data handling arise from large volumes of data: need to access, needed storage capacity, need for cataloging, and needed dissemination. Geophysics and to a lesser degree geochemistry have the major needs. Seismologists have assessed the data management problem and are recommending a National Center for Seismological Studies. This is an example in a field where $1-2 million a year will be required from several federal agencies to fund such a center, probably through a consortium (p. 73).

Scientists should be strongly encouraged to invest time in data management. NSF's Division of Earth Sciences should allow a small portion of research grant funds to be so directed and should request that original data with informative headings be stored in a retrievable manner. A catalog of multiuse data should give the specifics of the data so that other users have easy access to information (pp. 72-73).

4. Research Budget

Progress toward the solution of the high-priority research problems discussed in Section B of this chapter requires a strategy using various modes of operation and a mix of techniques, with financial support greater than that now provided. The requested budget for FY 1984, $42 million, is used here as a base on which to project anticipated additional costs. It is assumed that the base budget will continue to support individual grants, but the focus of those grants should shift toward the research priorities in the next section. The funding table that follows allocates increments to this base budget among the eight research priorities presented in the table. The funding increments recommended for FY 1985 represent estimates of the amounts needed to take full advantage of immediate opportunities for fruitful research in each priority area. The funding recommendations for FY 1990 are naturally more speculative, but they represent projections for anticipated funding requirements. Work by individual investigators supported by grants awarded in response to unsolicited proposals will remain as the chief mechanism for carrying out the priority research. Examples include isotope and trace-element geochemistry, structural geology, laboratory measurements of physical and chemical properties of rocks, biostratigraphy and climatology, paleontology, volcanology and magma genesis, and mantle structure and plate tectonic theory. All of the funding increments in Table 1 include appropriate increases in the basic grants program. Other costs for research requirements are identified in Table 1 in the research initiative and the footnotes.

B. RESEARCH STRATEGY AND PRIORITIES

1. Research Strategy

We believe an effective strategy for research in the geosciences over the coming decade can be built around the following main thrusts, pursued in a coordinated fashion:

- Coordinated study of the evolution of the continents, including: (a) systematic testing and extension of relevant aspects of plate tectonics as it applies to the continents; (b) probing of phenomena not readily explained by plate tectonic theory in terms of either time or behavior; (c) widespread application of new methods and ideas in established fields such as geologic mapping, isotopic geochemistry, geophysical profiling, geochemical analysis, experimental petrology, geochronology, modern paleontology, and satellite imagery; and (d) investigating the physical and chemical character of the crust by deep-scientific drilling.
- Development, testing, and suitable modification of the plate tectonics model using the best available technology.
- Utilization of existing and improved techniques for ship-based and other investigations of problems pertaining to the evolution of ocean basins and the sea waters they contain.
- Investigation of geological processes in order to better evaluate and predict geological hazards, environmental effects, and resource bases.

Within these four thrusts are many specific approaches that have been discussed in detail in reports prepared for the National Research Council, the NSF, and other groups. These reports were prepared by groups of experts in each of the several areas. In this report we have

TABLE 1 Recommended Funding for Research Priorities and Initiatives

RESEARCH PRIORITIES*	FOOTNOTES	PROPOSED ANNUAL INCREASES ABOVE NSF'S DIVISION OF EARTH SCIENCES 1984 REQUESTED BUDGET (In Millions of 1984 Dollars) FY 1985	FY 1990
Structure and composition of continental lithosphere	1,2	$ 7.0M	$ 22.0M+
Sedimentary basin development	1	2.0	3.5+
Magma generation and and emplacement	1,3	3.5	7.0+
Physical and chemical properties of rocks	3	2.0	6.5+
Physics of tectonic processes	2,3	3.0	5.0+
Convection of earth's interior	2	1.0	2.0
Evolution of life	-	1.0	2.0
Surficial processes	-	1.5	5.0
Proposed total annual increase		$ 21.0	$ 53.0
Total 1984 requested budget		$ 42.0	$ 42.0
Total proposed budget for years shown		$ 63.0	$ 95.0
RESEARCH INITIATIVE			
Continental drilling (including planning, site surveys, and background studies for dedicated drilling and implementing holes of opportunity).		4.0	20.0
	GRAND TOTAL	$67.0	$115.0

* Data management needs to be considered in all priorities.

+ In addition to the figures shown for research priorities, the research initiative for continental drilling will complement and supplement a number of scientific priorities but will be of most value to these five.

1. Includes funding for deep seismic reflection (Research Requirement 2b) and refraction profiling (Research Requirement 2a).
2. Includes funding for limited maintenance and upgrading of existing seismic networks and deployment of some new networks for specific studies (Research Requirement 2d and 4a).
3. Includes funding for replacement or upgrading of existing inadequate instrumentation, as well as development of key frontier instrumentation (Research Requirement 3 and 3a).

attempted to integrate their ideas into a coordinated approach.

2. Research Priorities

Chapter 3 summarizes the most challenging opportunities facing the geological sciences today. Not all of these can be addressed simultaneously or with equal emphasis, and choices must be made. We have based our choices on the following criteria:

- Will research on the subject fill an important gap in the intellectual fabric of the geological sciences?
- Is the work feasible in terms of both technology and the present state of geological knowledge?
- Is there a perceived need for the answers to research questions in basic or applied science?

Following these precepts, we have singled out eight topics or areas as having the most promise for advancing geology in the next decade. Suggestions for promising approaches to the solution of these major problems are offered in Chapter 3. We believe most groups of earth scientists would agree with these topics but that their rank ordering would vary from group to group, depending on the personal preferences of individual members. Therefore, with the exception of the item on continental lithosphere (Topic 1), which we consider of overriding importance and priority, no significance is intended by the ordering of the topics.

1. A MORE DETAILED AND ACCURATE DEFINITION OF THE STRUCTURE AND COMPOSITION OF THE CONTINENTAL LITHOSPHERE, INCLUDING THE CONTINENTAL MARGINS

Our knowledge of the continents at depth is woefully incomplete, yet the continental crust is where mankind lives and from whence we draw our resources. The emphasis in this high priority topic is on the acquisition of precise information on the details of the geometry and composition of the units comprising the continental lithosphere. Technological means are at hand to increase vastly our understanding of nearly all processes operating at or near and beneath the surface to great depths, and to study the results of these processes as they have operated through long reaches of geological time. The starting point for deciphering the history of the earth during the last 3.8 billion years is accurate knowledge of the present state. We therefore seek knowledge of the detailed structure and composition of the continents comparable to that which has been so recently achieved for the ocean basins. The greater complexity of the continents requires application of the most modern techniques, involving workers in nearly all subfields of the science, to acquire this knowledge and understanding. An expanded program of large-scale geological mapping and geochemical sampling in selected areas is a necessary component of these studies. The use of high-resolution deep seismic profiling techniques, reflection and refraction, must be increased. In addition to such surface-based investigations using geophysical and geochemical methods, direct sampling by deep drilling can provide additional required data. Although much of the needed research can be done by individual researchers with basic grant support, consortia should be formed as needed to work on large problems involving major equipment and facilities for multidisciplinary and interdisciplinary investigations (pp. 28-35).

The high priority assigned to this topic is supported by international recognition of its importance. As stated by Dr. D. A. Bekoe, President of the International Council of Scientific Unions,* "ICSU considers the study of the lithosphere to be one of the most important scientific activities for the future of mankind."

2. QUANTITATIVE MODELS FOR SEDIMENTARY BASIN EVOLUTION

The rocks and fluids of sedimentary basins are the results of tectonic, erosional, sedimentary, diagenetic and thermal processes. The main thrust in basin analysis is the development of integrated quantitative models of basin evolution. The resulting understanding of the rates and intensities of the processes can lead to predictive models for basin formation and the origin of the ground water, mineral deposits and hydrocarbons that

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*Dynamics and Evolution of Lithosphere: The Framework for Earth Resources and the Reduction of Hazards. ICL Report No. 1, Inter-Union Commission on the Lithosphere, April, 1981.

they contain. Research can be conducted through small grants that support field and laboratory studies. Some projects may be more amenable to attack by consortia, especially those that combine academia and industry. Seismic surveys, drilling programs, and computer modeling of such processes as water-rock reactions and hydrodynamic flow in the basins will provide supplemental information (pp. 23-24).

3. IMPROVED UNDERSTANDING OF MAGMA GENERATION AND EMPLACEMENT

Investigation of the formation of magmas, their geochemistry, movements in the Earth's interior, and extrusion requires a combination of field and laboratory studies. A global data set must be assembled. A major goal is the formulation of models of magma formation that can account for the variety of magma chemistries. Understanding magma genesis will require replacement of outdated or inadequate equipment, and the purchase of innovative frontier instrumentation. The successful solution of this problem will lead to improved methods for locating mineral deposits and better understanding of volcanism and geothermal processes. Some seismic studies will be required, as well as deep drilling. Approaches for research on the associated tasks are given in Chapter 3 (pp. 37-42).

4. KNOWLEDGE OF THE PHYSICAL AND CHEMICAL PROPERTIES OF ROCKS

Laboratory studies of properties of rocks and rock-forming minerals, under controlled conditions of temperatures and pressure (up to 3000°C and 1.5 megabars) provide basic data for quantitative models of geological processes. Studies of isotopes, trace elements and trace amounts of organic compounds are included. This research requires specially equipped facilities and perhaps can best be implemented through consortia of universities and by use of governmental laboratories. The need for major new instrumentation is great. Facilities are required for studies of the flow of fluids through large samples and the interaction of fluids with rocks. Improved laboratories for investigating fracture and creep processes should be equipped. In addition to laboratory studies, *in situ* measurement of rock properties should be made, especially in deep drill holes (pp.45-48; pp. 23-26).

5. A BETTER UNDERSTANDING OF TECTONIC PROCESSES, THE PHYSICAL AND CHEMICAL STATES THAT PRODUCE THEM, AND THE STRUCTURES THAT RESULT

Study of relations between stress and structures is required to provide models for tectonic processes. Scales of observation must range from mineral grains to continental blocks. Models for tectonic processes can, in turn, foster investigations of the mechanical and chemical interactions at plate boundaries and the energy budget of tectonic processes. Needed research includes field observations, laboratory research, and theoretical investigations. Among the special facilities required are laboratories for studies of rock mechanics and petrophysics, strategically located digital seismograph networks, and adequate computers. Special observations include *in situ* stress measurement, especially in deep drill holes. Field mapping of structures and stratigraphy, as well as seismic profiling, will be needed in key areas (pp. 42-45).

6. A MODEL OF CONVECTION IN THE EARTH'S INTERIOR

Current evidence supports the concept that convection drives the dynamics of the Earth, but the shape, size, and distribution of the postulated convective cells are not known. New data, some provided by satellites, and recent theoretical developments in continuum mechanics make this problem more amenable to attack than heretofore. This fundamental problem calls for a broad interdisciplinary approach in which geophysical and geodetic observations are made using a variety of modern instruments and analyses using large computers. Although much of the needed work can be done by individual investigators, they will require access to global data sets acquired by a great variety of sensors and archived in well-managed data centers (pp. 48-53).

7. EVOLUTION OF LIFE

The origin and evolution of animal and plant life, including the causes of mass extinctions and explosive or punctuated evolution, represent a first-order intellectual challenge. The relationships between organic evolution and the changing compositions of the atmosphere and oceans, paleoclimatic variations, continental rearrangements as they change the pattern of flow of ocean currents, and impact phenomena involving extraterrestrial objects all require interdisciplinary investigation. An adequate paleontological data base coupled with paleoclimatological, paleoatmospheric, and paleo-oceanographic information obtained by geochemists, paleontologists, stratigraphers, and tectonicists is required to address these problems. Modeling of the catastrophic effects of major extraterrestrial impacts and their effects on life forms requires the integration of paleontology, cratering mechanics, cosmochemistry, atmospheric sciences and biology (pp. 21-22; pp. 27-29).

8. SURFICIAL PROCESSES

Knowledge of the dynamic processes that shape and that have shaped the surface of the earth enables geologists to understand earth history better and to predict changes in the landscape, such as those produced by landslides, floods, and volcanic eruptions. It also helps us deal with the stress that man imposes on his environment. Advances in understanding the origins of landforms and rock types require the study of present-day processes and the extrapolation of results to ancient conditions. Such research requires a large data base for statistical modeling applicable to surficial processes of the past and future (pp. 18-22).

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PERSPECTIVES

The earth is a planet limited in size and limited in total resources. For the foreseeable future, it is the only available habitat for mankind. It follows that the more we know about our planet the better we can utilize its space and its resources for our well-being now and in the future. A former Undersecretary of the Interior phrased it as follows*: "As the most intelligent species on earth, man can certainly provide for himself and yet prudently protect the total ecosystem from unnecessary and unacceptable degradation." But to accomplish this we need reliable basic information. Geological science provides such information.

Recently, Dr. M. King Hubbert took note of the important effect of the earth sciences on society, on how we think and how we live. In the first century of modern geology, 1780-1880, the influence of geology was primarily on how we _think_, changing our concept of the earth from one with a short catastrophic history to one that developed gradually over enormous spans of time. In the second century, 1880-1980, the emphasis was on how we _live_, on expanding the knowledge base so that we could more easily find and utilize the earth's resources. Now, in 1980, we should concentrate on both how we think and how we live; how we can better estimate potential global resources and how we can manage these resources for maximum benefit to mankind. Our objective is to understand the earth; our success in achieving this objective will be reflected in our attempts to manage its potential resources wisely.

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*William T. Pecora in _Congressional Record_, 21 March 1972.

The range of the geological sciences is broad, from seismological inferences about the earth's deep interior to geological interpretations from satellite data, from the study of sedimentary basins to the study of the atomic structure of minerals, and from the study of mountains to the study of erosion and accretion of shorelines. Like other sciences, geology builds upon what has already been done. Progress is achieved by using the thoughts, measurements, and observations of past workers, adding to these the insights from new experiments and from new concepts. Geology has its own methodology and approaches; but, more than most sciences, it calls also upon the tools of physics, chemistry, mathematics, engineering, and biology to help solve its problems. The response to these calls has been fruitful, and use of these tools has greatly enhanced our understanding of the earth. At the same time, the cross disciplinary approach creates communication problems, because the different groups, using different tools, speak in their own technical languages. Yet in the past two decades the geosciences have evolved a global tectonic model, called plate tectonics, into which the work of all the subdisciplines can be fit and which can be tested by observations on a global scale.

The importance of plate tectonics to the geological sciences can hardly be overstressed. It has provided earth scientists for the first time with a working model of the earth as a whole, a unifying concept of global structure and composition, a fresh context for viewing earth history, and a framework in which to set detailed local geological studies.

Stated very briefly, plate tectonics holds that new oceanic lithosphere is constantly being generated along mid-ocean ridges called spreading centers. Partly molten material rises from the underlying mantle, cools, and is forced aside by new hot material coming up from the depths. Thus, the ocean floor is in motion, the principal relative movements being at right angles to and away from the spreading centers of the mid-ocean ridges. Ultimately, the oceanic crust moves to areas called subduction zones (at deep sea trenches) where it is drawn down and reabsorbed into the earth's mantle. The earth's crust is broken up into plates that move (1) away from each other at the spreading centers, (2) toward one another at subduction zones, or (3) slide by each other along fractures called transform faults.

It is a compellingly attractive hypothesis--simple, elegant, and potentially able to explain a wide range of diverse observations. And it has brought together such diverse specialists as paleontologists, seismologists, petrologists, marine geologists, geomagnetists and structural geologists, all working in concert to supply crucial evidence and tests.

The evidence for plate tectonics is strongly supportive, but there is much still to be done. The maturing theory must be fully explored, its implications and limits ascertained. For example, it explains large horizontal movements better than more modest vertical ones; it explains earthquakes and volcanoes along plate boundaries better than those in the middle of plates; it explains the location of porphyry copper ores better than that of most deposits of lead and zinc. Most important, although marine magnetic measurements and deep sea drilling have effectively demonstrated the reality of continental drift and sea floor spreading, we still do not fully understand the driving mechanisms. Thermal convection is the most likely candidate, but has yet to be proved. Finally, we are still unsure just when in the earth's history the whole process started. Many years of study have given us considerable insight into the structure of continents as revealed at the surface, but we know little about the detailed geology at depth and the dynamic processes of the earth's interior. Our understanding of the nature, history and evolution of both regions will depend upon an increase in our knowledge of the deep interior of the earth where the forces responsible for deformation orginate.

Several other factors combine with the new theory to make this a decade of great promise for the geological sciences. For one thing, rapid advances in geochemical and geophysical sensing, both direct and remote, have provided new tools for determining the composition, internal structure, form, and history of the earth and its component materials. Other new instruments allow us to study quantitatively the processes that have shaped and are shaping the earth. And the ubiquitous computer adds a new dimension to most of these instruments and techniques. On the more applied side, increasing population, increasing industrialization, and increasing complexity of modern society demand more detailed geological information so as to better use the land, reduce geological hazards, provide an inventory of resources, facilitate waste disposal, and prepare for long-term effects such as changes in climate and sea level. Geology will be required to become a more predictive science, and man's role as a geologic agent must be better recognized. Understanding causes and effects is the first step toward controlling them.

Geology must be concerned with processes that have affected the earth in the past as well as those that affect it today. Some present processes can be monitored or observed directly with existing instruments, arrays, or networks. Understanding these present processes not only gives us insights into the interpretation of past processes, but also helps to look ahead to future processes. Prediction, which in geology is the ability to anticipate the future results of processes and to better estimate the resource potential of the earth, is based upon what present processes are doing and what past processes have done.

The diversity of approaches and locations at which the studies are best made makes it very difficult to set absolute priorities in the geosciences. This very diversity explains why the research grant to an individual scientist is and should remain the backbone of support of earth sciences at NSF. From the wide-ranging activities carried out under the sponsorship of this program come the seminal ideas that move us ahead. Plate tectonics may have been demonstrated by large projects, but the basic ideas came from individuals. We can identify major opportunities and challenges, but we have more difficulty in identifying precisely how to respond to them most effectively.

At the same time, specific, often large-scale programs are especially timely when a problem has been clearly identified that requires larger coordinated efforts, because a technology has become available that makes such a program possible or because further progress demands such a program. Drilling is an outstanding example of such a large-scale program. By the 1960's, interpretation of the marine geophysical data had gone about as far as it could, and actual samples were required to move ahead. Now deep refraction and reflection measurements on the continents are bringing us to the point where actual sampling by the drill will soon be called for if further progress is to be made. The results of the Kola Deep Test Well (12 km deep) in the USSR, where the actual samples negated the geophysical inferences, demonstrate that there are many geophysical, geological and geochemical surprises awaiting us inside the earth.

Many research activites supported by NSF depend on facilities supported or run by other agencies. These include seismic networks, satellites, some computer facilities, drill holes of opportunity and others. Without these facilities, the research cannot be done; without support for the research, full advantage cannot be taken of the facilities. Although this report will focus on the avenues of research appropriate for NSF, this symbiosis should be recognized.

3

MAJOR RESEARCH CHALLENGES IN THE GEOLOGICAL SCIENCES

Specific emphasis in this report is on topics for research that we believe to have potential for major advances in scientific understanding of the earth during the next decade. Many of these have strong interdisciplinary connotations stimulated by the recognition during the past two decades that the history of the earth reflects the interactions of a turbulent interior and shifting crustal plates (plate tectonics) and an evolving atmosphere, hydrosphere, and biosphere. The topics and specific opportunities for research are discussed below. They are arranged in order from those dealing with the outer surface of the earth to those dealing with its interior, ending with a review of topics relating the earth to other units of the solar system and a statement on marine geology.

A. SURFACE AND NEAR-SURFACE PROCESSES AND THE ENVIRONMENT

At and near the surface of the earth, many physical and chemical processes involve complex interactions among (a) the rocks of the solid earth, (b) the soils formed from them by weathering, (c) the gases of the atmosphere, (d) the waters, both surface and subsurface, of the hydrosphere, and (e) the biosphere. These interactions produce some of the most complex natural systems known to science. Designing means to study them in a quantitative way and deciding the most effective approaches to such study present difficult intellectual and technological challenges.

Yet these surface and near-surface processes established many of the key conditions for the support of all life on earth and set environmental constraints for the conduct of human society. This section highlights significant research topics for the coming decade within this area of the geological sciences. Any short summary of such an interdisciplinary field runs the risk of overlooking some important interrelationships. The focus here is on the most fundamental of the outstanding questions and upon research directions that offer the most promise of fruitful results.

From a geological standpoint, a recurrent theme underlying many investigations of surface and near-surface processes is the behavior of water: water in the flowing streams and glaciers that mold the surface of the earth, and water in the ground where it is contained within and flows through the open spaces that exist in porous, near-surface rocks and sediments. The nature of the chemical reactions between these waters and the solid materials of rocks and soils is a growing field of current research. The physical effects of water and other natural fluids include those pertinent to an analysis of potentially damaging phenomena, such as floods and landslides. Understanding the geochemical role of water can help answer basic questions of environmental contamination by various forms of waste products generated in the modern world, as well as the formation of many metallic ore deposits. The influence of fluids on deep-seated processes, such as the genesis of molten magmas, is discussed in a later section, but is also an integral part of the story of water in the earth.

Included in this section are discussions of the porous sediments and sedimentary basins that overlie the so-called basement rocks of the continental blocks. Some of these sedimentary accumulations are many kilometers thick and are thus part of the near-surface realm of the earth only in a relative sense.

1. Surficial Processes and Landform Development

Although most of the earth's land surface has now been explored, the dynamic processes related to rivers, glaciers, soils, hillslopes, and coastlines have been well studied only in a few of the major climatic and terrain environments of the world. The special conditions of tropical, desert, and polar environments are far less well understood than those of temperate regions. The modern surficial processes of the earth serve as analogs to past processes responsible for the rocks and geologic features that comprise the record of earth history. Our concepts of origins of different rock types require the careful study of modern processes and the extrapolation of the results to ancient conditions. Innovative thought is required to explain erosional and sedimentary processes prior to the early Paleozoic colonization of the land by plants or prior to the development of an oxygen-rich atmosphere. The surfaces of Mars, Mercury, and the moon show that surficial processes during the first few hundred million years of history on all the planets were dominated by impact cratering. That the same processes operated on the early surface of the earth has profound implications for the subsequent evolution of the atmosphere, hydrosphere, and biosphere.

Equally challenging to the extrapolation of surficial processes into the past is their extrapolation into the future, given the realization that the future will see the increased role of man himself as a geologic agent. The clearing of forests and cultivation of the land, injection of particulates and gases into the atmosphere, disruption of natural shoreline processes, disposal of wastes, and construction of new habitats and transportation facilities are but a few of the accelerating impacts of human activity on the earth's surface. Geological archeology will become increasingly important in tracing the impact of such activities on the natural environment. It is necessary to understand the basic operation of surficial processes in order to understand man's impact on the earth.

The record of climatic and landscape changes during the Quaternary (most recent 2 million years) serves as a guide to some of the changes that can be expected in the near future. Moreover, the vast environmental changes of the Quaternary provide well-documented experiments on the effects of dramatic climatic change. One task of the surficial geologist is to read the stratigraphic and topographic record of those experiments, and to relate that record to the earth processes responsible. Although scientific observations of great floods, ice advances, sea-level changes and volcanic eruptions are both short and incomplete, the longer time spans represented, for example, by ancient flood, glacial, and coastal deposits provide the missing perspective. The past could be the key to predicting the future for this branch of geology.

2. Paleoclimatology, Paleo-oceanography, and Paleogeography

The history of the earth can be characterized as an ever-changing interplay between meteorological, hydrological, and geological events. The results of these events can be depicted on paleogeographic maps for time intervals reaching as far back into the past as we can read and interpret the rock record. We already have such depictions well back into the Mesozoic era for time slices of several million years each. As our ability to interpret earth history increases and we can identify synchronous events better, our maps will improve. Soon we will be able to display more accurately how the earth looked, and what conditions prevailed in the biosphere, at time intervals back into the Paleozoic and even into the Precambrian. Improvements in geochronology, paleomagnetism, and better interpretations of basic geological data will increase the accuracy of these maps.

Paleoclimatological studies, ranging from isotopic measurements of sediment from deep-sea cores and of ice cores from glaciers (and similar measurements on travertine deposits and ancient trees) to investigations of cave deposits and fossil reefs, will aid us in defining ancient climatic trends. These studies may also lead to better understanding and predictions of future trends. Eventually we may be able to determine whether, in the next millennium, we will be heading toward another glacial age with attendant sea-level drop, or toward a warmer climatic state that could result in a sea-level rise. Determining which of several possible scenarios is more likely to occur, and how rapidly, has obvious large societal implications, especially for coastal areas and for agricultural regions.

The past positions of continents and oceans and the related interplay of atmospheric and oceanic circulation also define some aspects of ancient climates. Along with knowledge of thermal gradients from equator to poles, a paleoclimatological approach defines likely areas of deposition of limestone, phosphate, dune sands, glacial drift and other types of sediments. Some application of this approach has already been made with analysis of positions of ancient reefs and deserts. What remains to be done is a far more complete and comprehensive analysis of paleoclimate and paleooceanography, providing detailed quantitative models of air and ocean circulation. On these models depend predictions of major sites of deposition of carbonates, evaporites, siliciclastics, pelagic sediments of varied types, potential petroleum source rocks, etc.,--i.e., a better understanding of earth history. At this time, we recognize ancient deposits and facies relationships for which we apparently have no modern analogs. Proper explanation of these ancient examples, in terms of paleoclimate and paleo-oceanography, requires modeling based upon the physics of fluid circulation over an earth with a distribution of continents, mountains, shallow seas, and deep seas very different from that of today.

3. Geochemical Cycling and Sedimentary Mass Balance

Until about 20 years ago, global information on the dissolved and suspended burden carried by rivers to the sea was inadequate for quantitative estimates of the exchange rates of these materials. Recently, numerical estimates of the kinds, masses, and ages of sediments and sedimentary rocks have been compiled on a worldwide basis. One of the key relations that has emerged from the integrated data is the fact that the total mass of sediments deposited during earth history is nearly an order of magnitude greater than the mass of sediments and sedimentary rocks now in existence.

Mathematical models have been developed that couple sediments, masses, and fluxes with suspended and dissolved material. Models of global biogeochemical cycles of the elements, especially of carbon, nitrogen, phosphorus, and sulfur, are yielding new insights into the importance of biosystems in earth processes. Of perhaps even greater utility has been modeling of the chemical cycles of the various types of sedimentary rocks, because transfers of materials (such as carbonates, sulfides, silicates, oxides, and organic carbon) among these reservoirs automatically include the coupling of the cycles of the elements.

Models of these relations have been constructed; reservoir sizes and transfer functions have been assigned. Modeling results are constrained by the requirement that observed variations in the stable isotope ratios of sulfur and carbon must be reproduced by fluctuations in the reservoirs of the model through time.

Among the capabilities of such models is a check on sedimentation rates and sediment types with time, determination of the relative importance of various global oxidation-reduction couples (ferrous-ferric, sulfide-sulfate, carbon-carbonate), further understanding of the complex coupling of sedimentary reservoirs and fluxes, and, perhaps, reconstruction of oxygen and carbon dioxide levels of ancient atmospheres.

Considering the importance of the models, determination should be made of: (1) the fine geochemical structure of sedimentary rocks, with special reference to their stable isotopes of C, S, O, and Sr, to their variations in chemical composition, and to all the other parameters necessary for mathematical modeling; (2) global element cycles, global sediment cycles, global oxygen budgets, and the interrelationships of atmospheric and oceanic change to the evolution of organisms.

4. Evolution of Sedimentary Basins

Most sedimentary basins in the United States have been explored to some degree and many have been studied extensively in the search for petroleum. Outstanding questions that remain are discussed at length in the NRC report, Continental Margins (1979), which points out the need for systematic basin studies. Although a good start has been made in relating basin development to plate tectonics, available knowledge is not sufficient to determine fundamental factors that control the location and size of basins, the relationship between the formation and the evolution of basins and the sediments found in them, and the factors that affect the conversion of sediments into rock and of buried organic matter into fluid hydrocarbons.

A more complete understanding of the tectonic, sedimentological, and diagenetic (post-burial) history of sedimentary basins is important not only for finding useful resources within them, but also for understanding earth processes related to fluid movements with the basins. For example, the origin of basins and their compaction history have a direct bearing on quantities and qualities of groundwater in the basins. The disposal of our nation's increasing quantities of waste products requires a much more intensive study of the properties of basin sediments (NRC report, Geological Aspects of Industrial Waste Disposal, 1982).

The origin and development of sedimentary basins on the continents are linked to the nature and action of the underlying continental basement. The subsidence process is complex, involving tectonic thinning of continental crust, thermal cooling of lithosphere, tectonic flexure of lithosphere, and the direct effects of local sediment loading. Understanding of the nature of the basement provides insight into basin development and permits meaningful predictions about basin history and economic significance.

The main thrust in basin analysis is the development of integrated quantitative models of basin evolution. If successful, these specific models can supplant the general qualitative views of the past and bring new benefits to all applications of basin studies. All the following facets of basin analysis are amenable to systematic and vigorous quantitative treatment: (a) the operation of the depositional systems responsible for the sedimentation of the strata within the basins, (b) the diagenetic processes of compaction and cementation that convert loose sediment to coherent rock, (c) thermal maturation of organic matter to hydrocarbon compounds, (d) lateral and vertical migration of contained fluids through porous sediment or rock, and (e) the rate of subsidence of the substratum as it receives a gradually thickening pile of sediment.

These different facets of basin analysis are currently under varying degrees of quantitative theoretical control. A major task for the future is the integration of all the various related quantitative models into a more general theory of basin subsidence and evolution that can be applied to a broad range of practical and theoretical problems in hydrology, resource exploration and development, and waste disposal.

5. Roles of Fluids in Rocks

The amount of fluids in rocks varies from a fraction of one percent in fresh volcanic rock to more than thirty percent in porous sediments. Water with varying amounts of dissolved salts is the principal constituent; hydrocarbons or carbon dioxide can also be present in major amounts, along with lesser amounts of other gases. Fluids in rocks are involved in numerous geological processes.

Water is an essential and high-volume resource commodity for modern civilization. Sound long-term assessments, through computer models, of the quantities and qualities of groundwater in basins are required for intelligent management of this resource. Some hydrological problems are caused by man and are often recognized too late (see Climate, Climate Change, and Water Supply, 1977; Scientific Basis of Water Resource Management, 1982; Geological Aspects of Industrial Waste Disposal, 1982).

One of the most important products of the interaction of water and rock is the formation of soils. Because soils are an essential part of the foundation of the biosphere, understanding of the processes by which soils are formed, transported, and depleted or enriched in particular minerals is important for the proper long-term management of this resource.

Petroleum and natural gas are generated when an organic source within sediments becomes deeply buried, and, as a consequence, is subjected to elevated temperatures and pressures. The rocks of petroleum reservoirs are also changed with burial. Porosity is destroyed and rejuvenated by processes that depend upon fluid flow and physical-chemical interactions that are as yet poorly understood. Basic research on these phenomena is of both fundamental and practical importance. Key questions that need further investigation, and can be addressed with current or developing methodology, include: (a) the role of clay minerals as catalysts in generating hydrocarbons, (b) the reasons for the occurrence of different molecular mixtures of hydrocarbons generated under different subsurface conditions from various source rocks, and (c) the factors that govern reaction rates and the degree of completion of processes affecting the reduction of primary porosity and the development of secondary porosity.

Many mineral accumulations are controlled by fluids on the surface or those moving within the earth's crust. Water plays an essential role in the accumulation of coal, and thick deposits of valuable materials, for example, halite, were formed by evaporation from aqueous solution. The fluids that flow through sedimentary basins change composition by reacting with the sediments. Saturation in valuable minerals may be achieved when one fluid contacts another, as when oxygenated surface waters meet reduced groundwaters, or where fluids from deeper within the basin move to conditions of lower temperature or pressure. Knowledge of the patterns of fluid flow and of ore deposition will permit prediction of favorable sites for resource occurrences. Models of mineral occurrence need to be developed based on both empirical and theoretical considerations. Many deposits will be below the depths of conventional exploration and exploitation. Increased emphasis should be given to in situ techniques such as forcing recoverable solvents through large and deep masses of rock to extract minerals. Difficult problems of bringing leach solutions into contact with mineral concentrations in relatively impermeable rocks must be resolved.

Occurrence models enable the nature of a deposit to be recognized quickly and enable prediction of important parameters such as size, grade, shape and extension of ore bodies with depth. Process models should provide quantitative material and thermal budgets for fluids and ore components derived from magma systems, for adjacent convecting hydrothermal systems, and for precipitation from basin-derived fluids.

Convecting hydrothermal systems are known to produce submarine copper, zinc, and lead sulfide deposits from rocks beneath ocean bottoms, but many details of the processes involved are lacking. Factors governing the location and preservation process for marine sulfides should be understood to assist in exploration for ancient deposits on land.

The role that fluids play during the deformation and fracture of rocks is inadequately known, but rocks are significantly weakened by the presence of fluids. The processes responsible are poorly understood. Mathematical models are needed of the processes involved, including non-linear deformation, fracture and fluid migration.

Fluids originate and circulate in rocks during metamorphism and no doubt control the rates of many reactions. When rock melts, fluids dissolve in the melt and increase its mobility and reactivity. In descending lithospheric plates, plate identity becomes diffuse on heating and reaction with fluids. Magmas may result and drive overlying volcanic systems. The effect of volatile components on such deep-seated processes has been studied experimentally with varying degrees of intensity, but much has been based on experimental data, and also on the study of volatile materials brought to the surface in magmas and lavas. The correlation of the experimental phase equilibria studies with the geochemistry and petrology of the rocks and with the physical processes involved must continue to be a fundamental direction in research.

6. Origin of Life and Evolutionary Paleontology

An intellectual challenge of the first order is to understand the factors involved in the origin and evolution of animal and plant life as we see it today. Many life forms have disappeared during earth history and sometimes in short periods of geologic time, while others have evolved in consonance with new conditions. This is a very broad and basic topic which involves research into environmental conditions early in earth history, past climatic changes, past conditions on the earth's surface, such as the extent and nature of seas and land masses, and the response of biological processes to environmental contraints.

Current paleontological research is being focused on evidence for changes in rates of evolution within and between species and higher taxa; evidence for the nature of earth's atmospheric evolution during the history of the planet; evidence of patterns of climatic change at several time scales during the past billion years; evidence of previous world physical and biological geographies; and evidence that asteroidal or cometary impacts may have played an infrequent but significant role in influencing the direction of evolution.

Perhaps the most exciting and controversial research concerns the study of the geological record of evolutionary rates. This involves: (1) debate within the paleobiological community between those who see evolution as a basically gradual process and those who see evolution occurring in short bursts between long periods of relative evolutionary inactivity and (2) a stimulating controversy involving claims that several of the most dramatic "crises" in evolution had extraterrestrial causes and are thus not amenable to explanation by recourse to traditional evolutionary processes rooted in paleobiology alone.

A new and now rapidly developing area of the science is that of biogeochemistry, the use of organic molecules of biological origin preserved in ancient rocks and of the preserved patterns of distribution of the stable isotopes of such elements as carbon, sulfur and the like to provide evidence of the biochemical and metabolic characteristics of past forms of life. These together with related areas of rapid advance in paleontology are for the first time providing important insight into the timing and nature of major events that shaped the course of biotic evolution during the earliest 85 percent of the history of the planet and are yielding new understanding of the interrelated development of the earth's biosphere, lithosphere, atmosphere and oceans. The study of biogeochemistry is also linked inseparably to the study of how buried organic materials such as kerogen are converted to fossil fuels such as petroleum. Understanding such maturation of surficial biochemicals to their deep-seated products not only has direct economic importance, but also can help us understand the evolution of sediment into sedimentary rock.

Almost all of this research requires an accurate and up-to-date body of information about the time/space distribution of well-identified fossils. Evaluation of rates of evolution and of fossils for both facies analysis and for biogeographic interpretation depends on such information. An adequate paleontological data base results only from careful collection and curation of fossils and competent evaluation of their morphologies by specialists. The systematic paleontologists necessary to create and maintain such a solid data base must have specialized skills and in-depth experience that cannot be acquired overnight. The current community of systematic paleontologists in the United States is aging, and for many key groups of organisms replacement specialists are not being trained. The data base of paleontology needs constant upgrading, and neglect of this sector will affect all interpretations based on it. Stimulation of research in systematic paleontology is essential to future advances in all of those areas of geology that depend on the fossil record for their primary information.

In order to address the problems discussed above, more information is required on detailed dating of the sedimentary record coupled with a greater knowledge of evolutionary changes so that the kinetics of evolution can be addressed. Geochemistry and paleontology are both required to address the problems of climatic and atmospheric changes and their effects on biological evolution; a close cooperation between workers in these fields is needed. Similarly, modeling of the effects of major impacts and how they affect living organisms is required to determine the contribution of extraterrestrial impacts to biological evolution. Biogeochemical studies require greater analytical capability using modern instrumentation and laboratory synthesis to understand the chemistry of the maturation process and the processes that form the various organic compounds, including those containing sulphur. The solution of many paleontological problems requires an interdisciplinary approach.

B. CONTINENTAL BLOCKS

Continents and ocean basins are the two primary subdivisions of the outer part of the earth in a geological sense as well as a geographical sense. The obvious contrast in the average elevation of the earth's solid surface in these two different regions reflects fundamental differences in the nature of the rock masses that underlie them. The insights gained thus far from plate tectonics are much more explicit and clear-cut with respect to the ocean basins than for the continents. A major focus of research in the geological sciences in coming years will be directed toward better understanding of the origins and history of the rocks beneath the continents.

How do continents differ from ocean basins? Most fundamental is the consistent and dramatic difference in the thickness of the outermost shell of the solid earth, called the earth's crust. The base of the crust is marked by the position of a major change in the velocity at which seismic waves propagate through the earth. This seismic discontinuity, termed the Mohorovicic or M-discontinuity, marks a transition from rocks of the crust above to rocks of the earth's mantle below.

Past work has shown that the crust of the earth is approximately 30 to 40 km thick beneath typical continental areas, and locally reaches thicknesses as great as 50 to 75 km beneath some mountain belts, whereas the crustal thickness in typical oceanic regions is only 5 to 10 km. The segments of thicker crust beneath the continents can thus aptly be termed continental blocks.

Thickness is not the only contrast between the crust of the continental blocks and that of the ocean basins. The nature of the crust in the two areas also differs in two other important ways. First, there is a difference in bulk composition. Whereas nearly all the oceanic crust is composed of materials chemically similar to the common rock called basalt, the continental crust also includes other diverse components, the most significant of which resemble the common rock called granite.

Second, there is a major age difference between many continental rocks and most oceanic rocks. Research during recent decades has shown that the oceanic crust is nowhere more than about 200 million years old, whereas rocks of the continental blocks range up to 3.8 billion years old. It follows that the rock record of earth history is mostly contained within the continental blocks.

The continental blocks also possess most of the earth's recoverable resources of minerals and fossil fuels. Considering its importance to mankind, the continental crust is still poorly known and consititutes a major frontier for science. Geochemical and geophysical techniques allow effective exploration of the deep continental crust. New approaches to geological mapping at the surface also allow more thorough appraisal of the visible part of the crust. In a sense, the coming effort in the exploration of the continental crust can be seen as the capstone of an enterprise that began several hundred years ago with the first great geographical expeditions aimed at delimiting the lands and seas of the world.

For the following discussions, it is important to know the distinction between the terms crust and lithosphere in current usage. The term crust, as noted above, denotes the outermost shell of the earth that is chemically distinct from the underlying mantle, although both are complex internally. The term lithosphere includes the crust and part of the uppermost mantle as well. The distinction between the lithosphere and deeper parts of the mantle is one of physical behavior. The lithosphere includes all the relatively cool rocks that form the generally stiff plates whose motions with respect to one another are described as plate tectonics. The hotter mantle beneath the lithosphere is less rigid and more capable of flowage. Estimates of the depth to the base of the lithosphere commonly lie in the range of 75 to 125 km.

1. Structure and Composition of the Continental Lithosphere

Plate tectonics provides a conceptual framework for understanding seafloor spreading and the drift of continental blocks across the face of the globe. The theory accounts well for the development of oceanic lithosphere, with its thin crust, formed at the zones of spreading where hot material wells up from the mantle. The nature of the continental lithosphere presents a more complex problem. Although much of the crustal materials in the continental blocks are generated by processes associated with the consumption of oceanic lithosphere at subduction zones, other processes that are not clearly envisioned may be significant as well. The much longer history of the continental lithosphere also allows extensive internal modification of the continental blocks through time.

Geological scientists have long subdivided the crustal rocks of the continental blocks into grossly different terranes or rock associations within which the fundamental nature and configuration of key rock masses are distinctive. The salient division is one between (a) the tectonically stable parts of the continents, called cratons, which are composed at depth of igneous and metamorphic rocks, and which may be covered by layers of sedimentary rocks near the surface; (b) the tectonically active mountain belts, called orogens, which are comparatively narrow elongate regions where intense crustal deformation, igneous activity, and rock metamorphism are characteristic; and (c) continental margins, which represent interfaces between continental and oceanic lithosphere. This basic subdivision needs to be tempered by the concept of time-dependence, because regions of continental margins and orogenic mountain-building at one stage in their history may later be incorporated into the stable cratonic cores of the continental blocks. The time-honored classification also needs some modification to allow for the processes of crustal stretching and rifting that lead to the sundering of continental blocks, and, in some cases, the formation of intervening ocean basins by seafloor spreading. Thick sedimentary basins of various kinds may develop in all three of the basic subdivisions.

Current experimental tools and potential technological advances offer multidisciplinary opportunities to expand our understanding of crustal structure beneath the continents. A continuing and vigorous program for the geological mapping of surface exposures will remain an essential component of crustal studies. Geochemical studies of the isotopic systems in rocks, and of the distribution of key trace elements in the crust, offer potential clues to the evolution of continental blocks. Paleomagnetic and geochronological studies can form the basis for improved knowledge of the age and deformational history of rock masses. Seismic reflection and refraction profiling, supplemented by deep drilling, afford the means to establish the subsurface configuration of geological structures and the rock masses involved in them. Some investigations will require the assembly of multidisciplinary teams of geoscientists to pursue complicated problems in a coordinated way over a period of years. On the other hand, thoughtful individual investigators, working alone or informally with a few colleagues, may produce conceptual breakthroughs that might elude organized team efforts. Both types of activity, so-called big science and little science, need support for the healthy development of crustal studies.

The dynamics of mountain belts are linked to plate tectonic processes, relatively well understood in some cases, less so in others. Examples of the former are where oceanic lithosphere moves beneath continental lithosphere, as in the Pacific Northwest and Alaska, and where continental lithosphere collides with continental lithosphere, as in the modern Alpine-Himalayan system. Perhaps included in this class are transform boundary systems such as that related to the San Andreas fault in California, where complex tectonic activity occurs for several hundred kilometers away from the fault, along which two plates of lithosphere slide sideways past one another.

Less well understood are regions such as the Basin and Range Province of the western United States. Its crustal profile is atypical of most continental areas, and it appears to be undergoing distension or stretching that is distributed over a wide region. In the Basin and Range Province, the crust is thinner and hotter than normal and is intensely fractured to produce alternating mountains and valleys occupying uplifted and depressed blocks, respectively. Least understood are regions of uplift and subsidence within intraplate areas, such as the central mid-continental portion of the United States. Relations between plate tectonics and intraplate tectonic activity require continued research to determine how much intraplate activity is related to plate motions and how much is related to independent causes (Continental Tectonics, 1980).

The rock masses of mountain belts have diverse origins. Some piles of sediments and volcanic rocks have formed within the mountain systems themselves, and have been folded, faulted and metamorphosed by the mountain-building processes. Also present are fragments of oceanic crust formed at oceanic spreading centers, as well as oceanic sediments and volcanic rocks. These are rafted into the subduction zones where oceanic lithosphere slides down into the mantle beneath the flanks of the mountain belts. Some components of these subducted materials can be mixed with hot mantle magma and injected into the deforming mountain systems. Details of what actually happens within these complex subduction systems are still poorly known. We need better experimental data on the possible interactions among rocks, magmas, and aqueous fluids or solutions to set our thinking in a better context.

Recent stratigraphic, petrologic, and paleomagnetic work indicates that most mountain belts are composite features composed of multiple terranes. Each terrane possesses a unique stratigraphy and internal structure reflecting its own particular origin and history. Many terranes have been displaced long distances by plate movements and have been juxtaposed by motions operative over long intervals of time. A characterization is needed of these different terranes by coordinated stratigraphic, paleontological, petrochemical, and geochronological studies. The histories of their displacements can be derived from well-targeted paleomagnetic and biogeographic studies. The internal composition and structure of the disparate terranes, coupled with the means and timing of their creation or juxtaposition, have major implications for strategies of resource exploration.

The convergent plate motions that give rise to mountain belts induce telescoping and contractional deformation of rock masses by large thrust faults within and adjacent to the mountain systems. Subhorizontal sheets of displaced rock termed thrust complexes or nappes are characteristics. Surface exposures of these bodies and the surface traces of their bounding thrust faults have been known and mapped for decades. But until recently no direct means existed for rigorous control of inferences about the subsurface configurations of thrusts and nappes. Seismic reflection profiling and related geophysical techniques now provide a way to attack this problem. Recent preliminary results suggest evidence for telescoping of masses of crystalline basement as well as sedimentary cover by hundreds of kilometers. Correlation of these new types of data with improved petrological and geochronological information has major implications for exploration for buried occurrences of both mineral and hydrocarbon resources.

Similar geophysical probes offer techniques to gauge the internal structure of the crystalline basement rocks that underlie the so-called stable cratons. Most of these interior parts of the continental blocks are masked by sedimentary cover or are poorly exposed in lowlands lacking significant topographic relief. The buried basement terranes are typically ancient rocks of Precambrian age. It seems likely that they represent the eroded roots of ancient mountain belts formed along plate margins of the past. If so, they represent assemblies of orogenic terranes that were created and juxtaposed sequentially through geological time. However, it is possible that some Precambrian continent-forming processes were different from those of today. Determination should be made of whether these rock masses have familiar structural configurations or whether they differ in ways that current theory cannot predict. Clarification of this point has major implications for resource exploration in these ancient terranes.

Documentation of continental rifting and drifting through studies of seafloor spreading has brought the realization that continental crustal blocks can undergo episodes of stretching and extensional faulting on a grand scale. In a sense, this distension is the obverse of the telescoping observed in mountain belts. The process is best studied along rifted continental margins and within marginal seas like the Gulf of California and the Gulf of Mexico. However, evidence for arrested or aborted stretching, and for distension in progress, is accumulating for areas such as the Great Basin and some intracontinental sedimentary basins. Of special interest are widespread detachment faults, which are subhorizontal surfaces along which distensional movement has uncovered basement rocks. Exposure of the latter at the surface provides direct access to crustal levels hidden elsewhere. The subhorizontal detachment faults have a general geometric similarity to the subhorizontal thrusts of mountain belts, and the two have often been confused in the past. The nature and geometry of the distensional detachment faults and related structures need to be better established both by surface mapping and by seismic profiling of the subsurface. As with the thrusts that telescope mountain belts, understanding the distensional phenomena has major implications for mineral and hydrocarbon exploration.

The continental blocks include not only the major land areas of the world, but also the submerged continental shelves and slopes. Locally, parts of mountain belts, stable cratons, and distended zones are beneath shallow seas and on the flanks of the oceanic basins. Only the deep seafloor beyond the toes of the continental slopes has a fundamentally different geological character. Consequently, a major task for the future is the integration of land-based and marine geoscience toward the goal of understanding the margins of the continental blocks. More emphasis should be put on key transects across continental margins on a worldwide basis, from the terrestrial interior to the deep seafloor, by coordinated teams of land-based and marine geoscientists. Results will have major implications for strategies of hydrocarbon exploration in offshore areas.

In sum, the multiple opportunities for future study of the deep structure of the continental blocks represent new points of departure from which fundamental new scientific data can be gained. For the first time, we stand on the threshold of pushing our confirmed knowledge of relations within the continental blocks deep into the subsurface and out beneath the shallow seas that lap onto the edges of the continental blocks.

2. Origin and Evolution of the Continental Crust

The sequential formation and destruction of oceanic crust are understood in broad outline, although many details need to be explained and several critical aspects of its evolution in transit from spreading centers to subduction zones are still problematic. By contrast, the origin and evolution of the continental crust pose a number of problems that remain unresolved.

Fundamental to the question is the fact that many segments of the present continental blocks date from a time far back in earth history. It is uncertain how long the present geodynamic system governed by plate tectonics has operated. Nor is it clear what changes may have occurred in the overall style and mechanisms of plate tectonics during the long course of earth history.

Processes of magmatism, metamorphism, and rock deformation associated with the process of subduction clearly give rise to rock masses of generally continental character. However, this does not exclude the possibility that other processes unrelated to plate tectonics may also have contributed to the formation of continental blocks. In concept, two opposing hypotheses can be entertained. On the one hand, it may be supposed that the processes of plate tectonics, operating over a long span of time, have caused the gradual emergence of continental crust from the mantle. On the other hand, it may be supposed that the full volume of the continental crust was generated early in earth history by processes lacking apparent modern analogs. In this latter view, the processes associated with plate tectonics are simply a means by which crustal materials have been recycled and refashioned through time. As stated, these two ideas represent extreme points of view, and a variety of intermediate or mixed hypotheses are possible.

There are sound reasons to suppose that tectonic regimes and geodynamic mechanisms have changed and evolved through time. For example, the radiogenic heat flux coming from within the earth must exercise a strong influence on the progress of mantle convection, which in turn sets constraints for plate motions. However, the rate of heat production by radioactive decay has steadily decreased at an exponential rate throughout the course of earth history. The implications for tectonics and magmatism are presently not well understood. Models of crustal evolution must be continuously revised in the light of better understanding of the dependence of the processes involved.

Several kinds of evidence can be brought to bear on the critical issues. Geologic mapping of ancient rocks, geophysical studies of deep crustal structure, and geochemical studies of crustal composition all have a role to play. Studies in isotopic geochemistry allow the more precise dating of processes that affected ancient rock masses. Recent analysis of selected radiogenic isotopes in crustal rocks also demonstrates a growing ability to infer the time of emergence of crustal materials from mantle reservoirs. At the heart of this line of reasoning lies the realization that some radiogenic daughter isotopes are preferentially transferred to new-grown crust with respect to radioactive parent isotopes that remain preferentially in the mantle during crust-forming events; in other cases, the reverse is true. By exploiting this diverse behavior of different parent-daughter pairs during crustal genesis, geochemists can potentially make confident statements about the residence times of a particular piece of the earth's substance in crust or mantle.

The initial genesis of a piece of continental crust derived from some mantle source region is only the first step in crustal evolution. Additional questions derive from the extent to which continental crust, once formed, has been modified or redistributed by metamorphism, erosion, and remelting. Many ancient rocks record a multiple history of deformation and reconstitution, suggesting that significant changes have affected some of them long after their initial formation. It is important to establish whether the observed reworking stems from plate tectonic events whose nature is predictable in general terms, or whether other processes that are less well understood have also been significant. Available paleomagnetic data suggest that major movements of the continental blocks have occurred throughout their history, but much additional information is needed to establish the actual history of plate tectonics.

In effect, the inquiry into crustal origins and evolution is an important way to test and extend plate tectonic concepts. If there are spatial or temporal limits to the applicability of plate tectonics, those limits must pertain to features of the continental crust and particularly to early crustal history. Until those limits are established, we cannot be fully confident about the reliability of concepts derived from plate tectonics for explaining more recent and better-recorded events. Appreciating the actual course of crustal evolution on a global scale thus becomes a keystone to further progress in geological thought.

3. Volcanology and Magma Genesis

Magmas range in composition from highly silicic to ultramafic, and probably no invariate model can account for the genesis of all. Volumetrically, basaltic magmas dominate, both in time and space; they constitute most of the oceanic crust and occur also within the continents. An origin of these magmas by partial melting in the Earth's upper mantle is indicated by petrologic models that are supported by both the major and trace element chemistries. In the continental crust, magmas more siliceous than basalts are believed to originate by melting of preexisting crustal materials. These magmas clearly are secondary in volume to basalts. In either case, however, the composition of the magma depends on: the composition of the parental source region, the degree of partial melting of the source (largely dependent on the temperature, pressure, and volatile content), and processes that occur during the transit of the magma from its source to final extrusion or intrusion into the crust (e.g., differentiation, contamination, etc.). Although much remains to be known about the details of the large matrix of factors, the geochemical, petrological (both field and experimental), and geophysical insights gained over the past several decades have established improved working models of the evolution of magmas (Basaltic Volcanism Study Project, 1981).

A major perplexing problem in magma genesis is the question of how the source region gets hot enough to melt. Is it due to a localized enrichment of radioactive elements, to heat transferred from mantle convection, or to admixture of fluxing volatile elements? Or does melting result from a decrease in pressure (the higher the pressure, the higher the melting temperature in dry systems), which could be effected either by erosional or tectonic unloading or by upward solid-state flow? Another factor that could cause melting might be a change in composition of the source due to metasomatism. Research on the melting of magmatic sources will require investigations of the geochemical distribution of radioactive and other trace elements in both the magmas and the supposed sources, the physics of convection and heat transfer, and the movements of fluids.

The amount of partial melting is estimated from the chemical composition of the liquid (the estimates are guided by the relations observed in experimental investigations), and the presumption of equilibrium between melt and solid. Key to relating the composition of magma to the degree of melting of its source is knowledge of the distribution coefficients for various diagnostic elements, but we now have little quantitative information on these coefficients, particularly as a function of temperature, pressure, and composition. New sensitive techniques such as the ion microprobe give us the capability of obtaining the critical data.

Most basalts are believed to represent 10 to 25 percent partial melting of an ultramafic mantle source. Yet considerations of the dynamics of separation of a magma from its source present a quandary because some recent models of the nature of melting and the dynamics of segregation indicate that once about 5 percent partial melting occurs, the melt will segregate from its source. These theoretical models are obviously not commensurate with the chemical and petrologic models calling for 10 to 25 percent melting. Admittedly, many of the governing relations for melt segregation are not well established; and research is needed on the physical aspects of melt segregation (melt-induced porosity and permeability, the viscosity of melts, and the state of stress in regions of melting). Both laboratory investigations and theoretical modeling of geochemical and geophysical relationships will be required to help resolve this enigma.

Much of the above discussion focuses on basalts derived from spreading centers at mid-ocean ridges and from isolated hot spots like Hawaii and continental magmatism. The other major type of volcanism is caused by subduction at active plate margins. Such magmatism is important in the formation of major mineral deposits (porphyry coppers, copper, lead, and zinc massive sulfide deposits, gold and silver deposits, and Mo, Sn, and W deposits), so that there is a strong economic incentive for understanding the processes involved. Examples are mountain chains such as the Andes and the Cascades and island arcs (such as the Aleutians, Japan, or the Antilles). Volcanoes in these areas are commonly explosive, and understanding the processes involved will enhance our predictive capability. One current model pictures magmas produced by subduction processes as a mixture of the subducted materials and the mantle. But the new model is still crude. In addition to the unanswered questions of magma genesis, we need answers about the nature and extent of the mixing process, its effects on the residual mantle source regions, and the thermal and pressure regime during subduction and partial melting. The effects of subducted volatile materials (H_2O, CO_2) on magma compositions and generation are especially important.

Rising magma may be erupted to produce volcanoes, and ash and lava flows or sheets. If the rate of ascent is rapid, the erupted magma constitutes a major fraction of that originally generated; and little heating of the host rock (and its fluids) surrounding the magmatic conduit takes place. Alternatively, if magmatic ascent is slow, the magma system may be emplaced in the subsurface with little or no surface manifestation, and the host rock may absorb most of the heat. These two limiting cases are perhaps less common than intermediate cases which involve a complex sequence of ascent, emplacement, and partial crystallization, followed by partial or complete eruption of the residual magma. During its residence time within a subsurface magma chamber, a given magma, derived originally from deeper within the earth, may equilibrate partly with surrounding crustal rocks. This complex process exerts an important influence on the ultimate composition of the magma and needs careful study.

The fundamental controls on the rate and nature of magmatic ascent are not well understood and represent a promising area for fundamental research in geophysics, geochemistry, and rock mechanics. Knowledge gained in this effort can shed new light on the space-time localization of volcanic and intrusive phenomena, as well as on the origin and potential duration of geothermal systems involving the large-scale circulation of hot subsurface waters. Resulting concepts and models can be of vital importance in the prediction of potentially catastrophic volcanic events and potentially beneficial production of geothermal power.

The potential for a geothermal system exists if a magma system has significant residence time in the near-surface domain, either as a slowly ascending body or as an emplaced unit. Whether or not a geothermal system actually develops depends on the attributes of both the magma and the host rock. Of fundamental importance is the rate and mechanism of mass and energy transfer between the magma system and the host rock. If conduction in the host rock is the dominant control, then by the time the thermal anomaly is sensed at the surface, the heat source may be at subsolidus conditions. On the other hand, if convection of fluids in the host rock dominates, then the cooling rate of the magma system may be significantly increased. In highly convective heat transfer, the near-surface thermal signature should be more diffuse, less intense, and possibly displaced, as compared with a case of conductive transfer. If the magmatic supply is renewed, or if there is a high level of convection within the magma system itself, the situation becomes even more complex. Mass transport between hydrothermal and magma systems is important in generating and sealing fracture systems as well as in modifying initial chemical and mineralogical parameters of both systems. Minerals formed by hydrothermal circulation and deposition are economically important as well as providing direct evidence on the transport process. There is little current effort in the geothermal industry toward understanding regimes at temperatures in excess of 300°C. However, these regimes are potential sources for economically important geothermal systems. In addition, the high-temperature hydrothermal regime is the locus of many mineral deposits (e.g., W, Sn, Mo, Bi). Research on both active and "fossil" thermal systems can aid in exploration for both mineral deposits and geothermal resources.

Crystallization of magma systems emplaced in the crust has been the subject of intensive study using field, analytical, and experimental modeling methods. The relationship between the emplaced body and its host rock has been used to infer physical, mechanical, and chemical properties prior to and during emplacement. Isotopic and elemental compositions of coexisting minerals in both the emplaced unit and the host rock are used to obtain estimates of intensive parameters and their variation with time during and after crystallization. Such estimates are based, most commonly, on laboratory data. Interpretations of crystallization history are often difficult because no magmatic liquids usually remain and because the volatiles have been largely expelled. In addition, the high-temperature magmatic mineral paragenesis may be modified during subsequent cooling and recrystallization. Continued detailed study of exhumed magma systems using modern analytical methods provides the information necessary in both testing and developing models. New experimental studies of mineral paragenesis are of critical importance for the development and calibration of quantitative models.

A research program of shallow, moderate, and deep drilling in a mineralized, active, hydrothermal-magma system would provide valuable new information. The potential for obtaining samples of an <u>in-situ</u> system where data can be obtained on mineralogical, chemical, and physical properties at ambient conditions, uncluttered by the effects of low temperature changes, would provide information directly pertinent to the studies of the ancient systems exposed at the earth's surface through uplift and erosion. In a similar sense, the opportunity to conduct downhole experiments in an active hydrothermal-magma system would provide a new frontier for measurements of <u>in</u> <u>situ</u> properties not obtainable in the laboratory.

There is a general need for further development of geophysical methods to be employed for the identification and location of subsurface magma bodies. Research on the use of multiparameter geophysical data to provide solutions for the joint inversion problem is of major importance. Acquisition of high-quality seismic, gravity, geodetic, electric, magnetic, and thermal data at a specific site is necessary to provide the base for testing, refinement, and use of numerical and analytic models. Some measurements of this nature have been made in the Yellowstone Park area within the past few years.

Research in all areas of magma genesis is stongly dependent on the use of modern geochemical and geophysical instrumentation, which has deteriorated in U.S. academic laboratories over the last decade (see Chapter 5.B). The availability of quantitative and experimental instrumentation, such as ion microprobes, transmission electron microscopes, mass spectrometers, Raman and Brillouin spectrographs, and high pressure and temperature apparatus, has made it possible to tackle problems which we previously lacked the technology to solve. Such technology needs to be made available to the community that can use it.

4. The Physics of Tectonic Processes

Contemporary tectonics requires quantitative understanding of rock deformation on scales ranging from the grains within a rock to continental and global features. Plate tectonics has taken us far toward the synthesis of geological and geophysical data, but it remains primarily a clear description of operative processes and is only slowly evolving toward a rigorous quantitative theory. Many kinds of structural elements of which the earth's crust is composed were well described in favorable exposures long ago. The origin of most of these has been generally understood, at least in terms of the main factors influencing their development. But now, because of the availability of increased amounts of excellent data, rapid advances in continuum mechanics and other relevant physics, and the easy access to computers, the time is right for the development of more effective structural models.

Progress in tectonics, as in all geoscience, is based on studies in three arenas: field observations, laboratory investigations and theoretical research. Laboratory studies of rock failure provide information about the processes in a rock leading to rupture. Observed changes in rock properties associated with stresses near failure provide a basis for interpreting geodetic and geophysical data in tectonically active regions. Studies of petrofabrics, in the field and laboratory, provide clues to conditions of temperature, stress, and flow that prevailed during deformation.

Knowledge of the state of stress in the lithosphere is fundamental to understanding the physics of tectonic processes. Because of its nature as a tensor quantity describing forces internal to a mass of material, stress is a very difficult quantity to observe. Techniques have been developed for direct measurement of in situ stress in the crust, and these have yielded valuable information. However, these methods are applicable only to small depths and some are costly to apply. More reliable, efficient and inexpensive methods of measuring stress at depths typical of tectonic activity are needed. Even a good measurement of stress at one point is only a beginning toward the data needed for use with analytical models of tectonic processes. We need to know the distribution of stress, including stress gradients and the position of stress concentrations in the crust, because it is the spatial variations of stresses that drive dynamic earth processes.

Earthquakes are a dramatic manifestation of the stresses in the neighborhood of the hypocenter. They provide important data for evaluating the stress regime in tectonically active areas. A major task of contemporary seismology is the derivation of geometric and dynamic parameters of seismic sources from the information in the radiated waves. Theoretical seismograms corresponding to an input source and a selected earth model are routinely calculated. Source properties can be adjusted until the synthetic and observed seismograms are in good agreement. The spectra of the seismic signals offer another approach to probing the stress field at a seismic source.

The rheology of rocks determines their response to stress. Mantle rheology and thermodynamics are identified as one of the major problem areas for the next decade and are discussed as such elsewhere in this report. Crustal rheology involves more moderate temperatures and low-to-intermediate levels of stress. More complete knowledge of the elastic and inelastic mechanical and thermodynamic properties of crustal materials is needed as a basis for modeling processes from metamorphism to basin formation and mountain building. Elastic wave anisotropy in the crust and upper mantle is currently a subject of vigorous investigation as an indicator of flow processes and stress orientation in the interior. Studies on heterogeneities of the earth's mantle will yield information about the dynamics of the earth.

The success of the plate tectonics model in accounting for geodynamic processes and in correlating apparently unrelated data has led to attempts to establish numerical models of the primary processes involved. Plate tectonics is a convective process, with hot material rising at the spreading centers and moving laterally; the cool, dense product sinks at subduction zones. Having said that, one is left with myriad questions concerning the energy source, the driving mechanism, the scale of the convection, and so forth. Although modelers are incorporating more real data, including geodetic and thermal information, as well as older data on relative plate motions, there remains serious disagreement about the answer to the basic question, "What drives the plates?"

A host of challenging and important problems in the physics of tectonic processes could be listed. The physics and chemistry of plate-boundary interactions clearly stand near the top of any list of major problems of contemporary tectonics research. Most of the dramatic geological action occurs at these boundaries, the sites of great earthquakes, volcanism, orogeny, magma genesis and ore concentration.

Little is known about the determinants of the mechanical behavior of plate interfaces. What gets subducted and how does it behave after subduction? Why is relative plate motion accommodated at some boundaries by aseismic stable creep, while intermittent stick-slip, leading to great earthquakes, characterizes other parts of the same plate boundary or other boundaries? What is the time scale on which the mechanical properties or stress regime at a given plate boundary can change enough that creep takes over from stick-slip or *vice versa*? This question is central to the prediction of plate-boundary earthquakes and the evaluation of so-called seismic gaps. How does subduction originate? How do strength and other properties vary with strike and depth along great transform faults, and how do these variations control the earthquakes on such faults?

Some of the most challenging problems are those that have so far not yielded to plate tectonic analysis. Basin formation, orogeny and volcanism are not well understood in the context of intraplate dynamic processes. A key goal of the worldwide geosciences effort in the next decade, including especially the International Lithosphere Program, is progress toward understanding these processes.

The energy budget of tectonic processes is also not known. For example, even though we have a good qualitative picture of the flow of energy in an earthquake, in terms of its accumulation, release and redistribution, we have a very poor grasp of the quantitative distribution of that energy among the various operative processes that consume the strain energy released by the rupture. We are even less informed about energetics of gross plate motions. It is only when these fundamental questions of energetics are satisfactorily answered that we can begin to feel that we understand how the dynamics of the earth works.

C. EARTH'S INTERIOR

The earth's mantle and core are the major components of its interior. The earth's mantle is directly beneath the crust and extends about halfway to the center of the earth. As the result of seismological and geophysical studies, experimental laboratory techniques capable of attaining the pressures and temperatures of the core-mantle boundary, geochemical analyses of mantle-derived rocks, and complex geophysical and geochemical modeling, we have achieved a rough understanding of the composition and physical state of the mantle and of the core. The level of this understanding bears directly on the developing concepts of plate tectonics, with all of their ramifications.

The availability of more comprehensive data sets and theoretical developments and the consequent new approach of analysis and interpretation indicate that many lines of investigation are on the brink of major discoveries. For example, techniques are now available for mapping of the earth's interior in three dimensions--"tomography." There are two basically independent approaches. One involves the systematic inversion of large amounts of currently available travel-time data of compressional and shear waves that have traveled through the earth; the other approach involves modeling based on incomplete data from free oscillations of the whole earth and surface waves having periods sufficiently long to be affected by the earth's deep interior. In both approaches large computing facilities are required, and a global array of long-period seismometers that record digitally is required for the latter.

1. Physical Properties of Earth Materials

The physical properties of rocks depend on the properties of their constituent minerals plus impurities (grain boundaries, pores, fluid phases, etc.). Knowledge of these physical properties is essential in exploring for and recovering natural resources (including ore minerals and fossil fuels), in analyzing earthquakes, in understanding geologic processes like mountain building, volcanic eruptions, and geothermal energy, and in studying the constitution, age, and origin of the earth. Comparison of *in situ* field measurements with laboratory measurements of rock and mineral properties is a key ingredient in addressing such problems. The following are descriptions of various types of physical properties of rocks and minerals that are important for these purposes.

Aggregate or bulk properties of rocks include chemical composition and mineralogy, density, porosity, and permeability, which are well-known in the laboratory. Measurement of these properties in the earth requires samples recovered at the surface or by drilling and borehole logging at shallow depths. At greater depths, these properties are generally inferred indirectly from seismology.

Thermodynamic properties of minerals, including specific heats, enthalpies of reaction, and phase equilibria, have been compiled to allow calculation of the pressure-temperature-volume condition of some mineral equilibria, melting and other transitions, and mineral and rock interactions with water and carbon dioxide. In conjunction with laboratory experiments at elevated temperatures and pressures, these data can be used to estimate the conditions of origin for deposits of metallic ores and metamorphic and igneous rocks in the crust and mantle, and to constrain the geotherm beneath different kinds of geological provinces.

Optical, spectroscopic, and electromagnetic radiation properties of minerals aid identification and provide data on the paragenesis of minerals in rocks and ore deposits. The electron microscope and microprobe have revolutionized studies of small mineral grains. Electron paramagnetic resonance is a useful technique in studying mineral defect structures.

Crystallographic properties help to solve a variety of problems of significance in the understanding of earth processes. These problems include: phase transformations at elevated pressures and temperature; non-periodic aspects of mineral structures; the physics of bonding in silicates, sulfides, and other mineral groups; the relationship between crystal structure and elasticity;

the rules governing the distribution of minor and trace elements among crystalline phases, order-disorder phenomena, and dynamic minerals processes; characterization and theory of twinning in crystals; the connection between crystallographic and thermochemical data; and crystallographic factors affecting mineral stability. Minerals that are of special interest because of their complex nature and/or economic importance are sulfides, sulfosalts, zeolites and clay minerals.

Elastic properties include elastic moduli and elastic wave velocities and are related to anelastic attenuation and velocity dispersion as well as viscosity. Measurement of seismic velocities has become more critical for understanding the stratigraphy and structure of the lower continental crust due to the advent of COCORP studies, and remains the most direct method by which the physical properties of the mantle and core can be inferred. Laboratory measurements of the elastic properties of minerals and rocks can now be performed on specimens as small as 100 microns or as large as several meters, at pressures of 50 kilobars by acoustic methods and 5 megabars by shock waves, at temperatures of 1000°C, and at frequencies from 1 hertz to 10 gigahertz. Only rarely can these extreme conditions be attained simultaneously at the present state of the art. Further work is needed to understand the relationship of elastic to rheological and anelastic properties (see also Section C.2).

Mechanical properties include strength, rheology, friction and fracture. Laboratory deformation studies on actual rocks have been made up to high pressures and temperatures, which help to explain fracturing and ductility of rock-masses in the earth. Mechanical equations of state are available for application at pressures and temperatures of the crust and upper mantle.

Thermal properties include conductivity, diffusivity, expansion, and thermal inertia. Recent emphasis on geothermal resources and radioactive waste disposal has aroused renewed interest in measurements of thermal properties of rocks and minerals, and their dependence on temperature and other environmental conditions.

Electrical properties include resistivity and dielectric constants, which have been studied on some minerals to high temperatures and pressures. The dependence of resistivity on saline-water saturation has received much study. Electrical measurements in boreholes are essential in petroleum exploration. Measurements of electrical properties of materials located in depths to tens of kilometers beneath the earth's surface have provided information about the composition and thermal state of the rocks at depth. Such information is important for understanding geological processes including mineral accumulations, and the nature and extent of partially molten zones.

Magnetic properties include susceptibility, permeability, coercivity, Curie temperature, and remanence. Paleomagnetic studies on the reversals of the magnetic poles have provided a new chronology for rocks, and are now playing an increasingly critical role in magnetostratigraphic studies. Airborne magnetic anomaly maps have become a great aid to interpreting subsurface geology.

Paleomagnetic investigations of both igneous and sedimentary rocks have played a key role in working out the past positions of continents and fragments of continents. These investigations will continue to be important, especially as they pertain to the emerging concept of fragments of the lithosphere from distant sources being accreted to continents.

Investigation of all the listed physical properties requires extensive laboratory instrumentation. Current funding for such instrumentation is unsatisfactory at best. This is particularly unfortunate because recent advances in techniques and equipment for mass spectrometry, x-ray powder diffractometry, scanning-transmission electron microscopy, cryogenic magnetics, nuclear magnetic and electron paramagnetic resonance, high pressure mineralogy and mineral physics, spectroscopy (including Raman, Brillouin, and infrared), and computing represent a revolution in the quality and quantity of experimental data that can be obtained.

Measurement of the physical properties of rocks in place in the earth is also of great importance to many basic and applied problems, such as groundwater flow, heat transfer by water and magma, stresses *in situ* in earthquake-prone areas, detection and mapping of fractures at depth, scaling between laboratory and field measurements, numerical modeling, and measuring thermal conductivities in place. These important topics deserve additional attention and support.

2. Thermodynamics and Rheology of the Mantle

Flow in the mantle is most likely the ultimate cause of plate tectonics, earthquakes and the orientation of the mass distribution in the earth relative to the spin axis. It is also the main process by which heat escapes from the earth's interior. Mapping and understanding the patterns of flow in the mantle are now within the grasp of geophysicists because of applications to the problem of new concepts and innovative interpretations of data. Among the new tools available for investigating mantle flow are quantitative interpretations of the details of the shape of the earth, new uses of several kinds of seismic data, such as the tomographic approaches described previously, and improved theoretical expressions of flow in complicated media.

It has long been recognized that, below a thin cold outer brittle shell, the earth's mantle, which responds as an elastic solid to relatively short-period driving forces, behaves as a fluid over geological time. Plate motions are probably the result of convection in the mantle. The pattern of this convection is, however, controversial. Some advocate whole-mantle convection; others separate convective systems in the upper and lower mantle. It is not now known whether mantle convection occurs on a scale that is small compared to the large-scale flow associated with plate motions, nor whether the time dependence associated with the plate configurations is the only time-dependent aspect of mantle convection.

These questions have not been answered in a satisfactory way during the fifteen years since the development of the plate tectonics theory, primarily because of the difficulty in placing observational constraints on the flow in the earth's interior. Among the breakthroughs claimed above are methods for establishing such constraints.

The geoid is an equipotential surface that represents the shape of the earth, including departures from a simple figure of revolution. The geoid should give first-order information about the density contrasts driving plate motions and flow in the interior. Unfortunately, studies of the geoid and of closely related gravity observations have low resolution, and interpretations of structure based on knowledge of the gravity field alone are inherently non-unique. However, when combined with observations such as surface deformation or seismic velocities, gravity-field measurements provide an extremely sensitive constraint on earth structure.

Measurements of the shape of the geoid are so sensitive because the net geoid anomaly results from two competing effects. A dense region in the mantle will, of course, by itself cause a positive geoid anomaly. However, it will also induce flow which will lead to deformation of the earth's surface and internal boundaries. These deformed boundaries also cause geoid anomalies. The total anomaly can be either positive or negative depending on the viscous structure of the mantle and the presence or absence of chemical layering. The competing effects can be understood by doing detailed calculations of flow in temperature- and stress-dependent layered systems, by obtaining more detailed information about the structure and mechanics of the interior, and by extracting the variation in the earth's topography not directly related to variations in crustal thickness. The calculations require large, fast computers.

To understand convection in the mantle, we must know the stress and temperature in the interior and the orientation of flow. Full analysis of seismic data in terms of recently developed theoretical constructs can yield much of the required information.

Most of our information about earth structure comes from seismic wave velocities. The amplitudes of seismic waves and the attenuation of these amplitudes also contain vital information about the interior. In particular, wave attenuation is dependent on stress and temperature. Theoretical relations linking attenuation to viscosity and stress have been developed. These relations must be calibrated by laboratory experiments which require a large effort in measuring rheological and attenuation properties of minerals at various stresses and temperatures.

Crystals tend to be aligned by flow and stress, and the resulting seismic anisotropy is a direct measure of flow patterns in the mantle. The data needed for mapping stress and flow in the mantle can only come from a global network of digital, broad-band seismic stations, with both vertical and horizontal sensors.

Recent advances in theory and computational capability are available to help in solving these fundamental problems. Most calculations of mantle flow have been based on a single layer behaving as a Newtonian fluid. Mantle silicates actually have strong temperature and stress-dependent rheologies, and the mantle is surely inhomogeneous in viscosity, and probably in chemistry. It is now possible to compute flow in a layered system with these complications taken into account; preliminary results are providing much insight into the dynamics of the interior. It has been found, for example, that heat builds up under continents, suggesting that this buildup initiates mantle flow and that the continents are inherently unstable. When continents are present on the surface, it may be that steady-state never prevails. The required calculations are lengthy and exceed the capabilities of most university computers and computing budgets. Nevertheless, these calculations must be done if theories of mantle dynamics are to develop beyond the present qualitative stage.

The data most pertinent to investigations of mantle flow, rheology and evolution are: global geoid, gravity and topographic data bases; global maps of crustal and sedimentary rock thickness; heat flow; dispersion and attenuation of Love and Rayleigh waves over many paths; and body-wave travel times over many paths. Some information on sedimentary rock thickness and local gravity is proprietary, and provisions are needed to make them available to the general scientific community.

Of special value among newly available data are satellite-borne radar measurements of the shape of the sea-surface (geoid). The systematic treatment of these data will make it possible to place bounds on the locations of density and velocity anomalies in the mantle. Large computers and extensive computations are needed for this work also. Much of the data is already collected or is being acquired on a routine basis.

In summary, the explanation of the geoid is one of the most perplexing problems of modern earth science and promises to lead to major new knowledge of the interior. When we understand the geoid, we should know whether the rheology of the mantle is strongly temperature and stress dependent, whether convection is layered or whole-mantle, and whether convection is statistically steady or not.

It has been suggested recently that the geoid has a long memory and that its present shape reflects conditions in the distant past. If this is true, then we have the exciting prospect of being able to identify the past configurations of the continents, ridges, and subduction zones. Entirely new possibilities for knowing the general style of plate tectonics in the past are thus presented.

The great needs for progress on these problems are access to appropriate computers and the further development of the global digital seismic network. In addition, we need laboratory measurements of the rheology of silicates at high temperatures and controlled dislocation densities. New laboratories, filling the gap between rock mechanics and ultra-sonics, are required to provide the basis for interpreting seismic attenuation and anisotropy observations in terms of rheology, stress, and flow.

3. Mantle Heterogeneities

Since the turn of this century a variety of geophysical data, particularly seismological data, have firmly established the gross structure of the earth's crust, mantle and core. The evolution and dynamic behavior of these three zones remain key topics in solid earth studies, although much has been learned about the physical properties and chemical composition of these three zones in recent years. A major advance in our understanding of the mantle has been the discovery that the mantle is chemically heterogeneous on a variety of scales. Evidence for this is provided by the precise isotopic analyses of Pb, Sr and Nd of young volcanic rocks from the ocean basins. For example, these data show that the mantle sources which yield mid-ocean ridge basalts (MORB) have been depleted, relative to other basaltic rocks, in their trace elements for a period of time of the order of 1.5-2.0 billion years. Furthermore, the mantle sources which produce MORB appear, at least to a first approximation, to be a distinct global entity, relatively homogenous chemically, but distinct from all other mantle sources which produce basalts. Partial melting is the most likely process for this depletion, and hence the inference is that a segment of the mantle had been partially melted a long time ago, leaving behind the residual mantle which now produces MORB. In contrast to this, the ocean island basalts (OIB) appear to be derived from undepleted or enriched segments of the mantle, and are quite heterogeneous. Much less is known about the sources of continental flood basalts which are considered by some to be the continental analogs of ocean ridge or hot spot basalts. Some are derived from undepleted sources, while others are clearly derived from depleted sources.

The long-term preservation of heterogeneities in the mantle provides a powerful constraint for modeling convection processes. What is needed is a much more detailed understanding of the nature, scale, extent and location of mantle heterogeneities, in both the oceanic and continental mantle.

A major chemical and physical heterogeneity on the surface of the earth is the continental crust. Although it is a small fraction of the mass of the earth, the continental crust is the major reservoir for many incompatible and volatile elements in the earth, whereas the mantle is enriched in more refractory elements such as Mg, Cr, and Ni. Although it is known that partial melting in the earth can lead to this type of chemical differentiation, the actual origin of the continental crust is not clear. Such questions as when and how rapidly the continental crust was created; whether the process was episodic or continuous; and what its rate of generation was as a function of geological time remain unanswered.

Fortunately, we appear to be at the threshold of understanding some of these details. Isotopic measurements that yield ages and the chemical characteristics of the major reservoirs of the earth have shown that it is reasonable to expect that the crust may be complementary to the residual mantle sources mentioned earlier. This process started at least as early as approximately 3.8 billion years ago; details of what happened in the first 700 million years of earth history, prior to 3.8 billion years ago, are, however, obscure. In order to generate the continental crust, only 20 to 30 percent of the mantle need necessarily have differentiated chemically, implying that a significant fraction of the earth's mantle may still be pristine. The location of this pristine mantle and its preservation as a distinct entity remain to be explained.

Answers to many of these questions can now be attempted. To define the rate and manner of continental formation, more extensive isotopic and chemical measurements of the rocks from the upper and lower continental crust are needed. To define the shape, scale size and distribution of the convection cells, we need to learn more about the distribution of the mantle heterogeneities and their mean lifetimes.

4. Composition and Evolution of the Earth's Core

Most scientists today believe that the earth's core was formed by segregation, accumulation and settling of metallic Fe-Ni from an earth that was accreted homogeneously, at least to a first approximation. Core formation in such a model probably began well before accretion was completed and occurred rapidly because of the strong temperature dependence on viscosity and the rheology of the cold center. The gravitational energy released would represent the greatest single thermal event in the history of the earth. The metal involved in core formation is thought to be mostly primordial in origin, with possible contributions in the deep mantle from the proposed decomposition of $2Fe^{2+} = Fe^{3+} + Fe^{0}$ at high pressures.

Seismic data indicate that the bulk density of the outer core is less than that of pure Fe, and that some 8 to 15 weight percent of a light element must be present. A number of candidates have been suggested; the most popular contenders are oxygen, sulfur, and silicon, or a combination of sulfur and oxygen. Clearly other elements such as the noble metals, some Cr, C, Pb and P must be present in the core in greater abundance than their content in the crust and mantle if chemical equilibrium were closely approximated during core formation. The suggestion has been made that some potassium, K, was partitioned into the core. If so, ^{40}K could provide the heat to drive the earth's dynamo. However, it is the informed consensus that gradual solidification of the inner core provides sufficient energy for the geodynamo; there are also strong geochemical arguments against the existence of K in the core.

Core formation must have removed most of the lead from the mantle, so that the age of the earth as determined by the lead isotope method must actually date the time of core formation. Latest calculations suggest this was no more than 100 million years after the time of formation of the earth. Some time after this, but prior to 3.8 billion years ago, the core dynamo originated, thus imprinting the evidence of a magnetic field on the rocks of the crust and mantle since that time.

Core formation is of considerable relevance to our understanding of the primordial crust, and a greater understanding of the processes involved will contribute answers to the following questions involving crust:

- Did the postulated reaction $2Fe^{2+} = Fe^{3+} + Fe^{0}$ affect the redox state of the early crust through convection of mantle upward?
- Was S involved in the process of core formation? How did this affect the S budget of the mantle?

- What effect did core formation have on the thermal regime of the crust and mantle?
- Was appreciable K involved in core formation? If so, what effect has this had on the earth's thermal history, mantle convection, and plate tectonics?
- Was core formation a one-time process early in earth history, or is it still slowly continuing, with additional release of thermal energy?
- How did the bulk chemistry of the protocrust change as a result of the elemental partitioning involved in core formation?
- What can we learn about the early history of the core dynamo and how it works today?
- How much chemical equilibration occurred during core formation? Can the chemistries of crustal and mantle rocks teach us anything about the chemistry of the core?
- If equilibration occurred, why is the crust so oxidized with respect to the core? Was this oxidation a gradual process related to the earth's evolution or was this related to heterogeneous accretion in which the outer earth accreted material richer in volatiles than the interior?
- What causes magnetic reversals?
- How did the core form? How can the process be quantified? As Francis Birch stated, it is "reminiscent of the parlor trick of removing the vest without the jacket, and cannot well be followed in detail."

Answers to such questions are necessary for understanding the nature and evolution of the protocrust, as well as understanding the kinetic, chemical, and thermal evolution of the mantle and core. Such answers would help greatly in our understanding of the process of core formation on planets other than the earth. The solution to many of these problems involves many subdisciplines within the earth sciences, and a consortium approach would be applicable in many areas.

D. EARTH IN THE SOLAR SYSTEM

In the decade of the seventies, spacecraft opened new worlds to geological exploration. Only 20 years ago, planetology was almost exclusively the province of astronomers; geologists, however, were in the forefront of scientific planning for the manned Apollo program that provided our first field samples from another planetary body, the moon. Experience gained on the moon permitted expansion of geological horizons outward to more distant globes, now accessible to increasingly sophisticated unmanned spacecraft. Planetary geology has changed remarkably as our data base has expanded. Initially, we tried to understand landscape phenomena in topical fashion: the origin of craters, the nature of surface modification, the role of ice or volcanism. Today, the science has matured, and research materials have increased to the point that we are engaging in true comparative planetology. We have used the experience and education gained by trial and error on the moon--together with our knowledge of terrestrial processes--to extrapolate and speculate about totally unexpected and unfamiliar territories beyond.

This perspective of the geology of the earth is in its infancy; some research opportunities are described below.

1. Comparative Planetology and Early History of the Earth

One of the major objectives of planetary science is to predict the evolution of various planetary bodies from various sets of initial conditions, or to deduce initial or other prior conditions from observations of evolved conditions. Both approaches should strive to produce comparable results for the evolution of any planetary body. Thermal, tectonic, chemical, and petrological models, all of which are closely interrelated, can be combined; they are constrained by observed or assumed parameters that serve as initial and boundary conditions. Planetary evolution depends on such initial conditions as size, chemical composition, content and distribution of volatiles, abundance and distribution of heat-producing radioisotopes, and the flux of impacting materials during accretion and late-stage bombardment. Boundary conditions are constrained by such observable data as chemical and isotopic compositions of materials formed at various times in the body's evolution, and by structural patterns, surface conditions prevailing (e.g., weathering, atmosphere, ocean) and physical conditions occurring (e.g., temperatures, pressures, magnetic fields) at various stages in the body's history.

The history of crustal processes varies markedly from one planet to another. The surfaces of some planetary bodies indicate an active early history followed by quiescence; these record the processes of early evolution (e.g., Mars, Ganymede). The surfaces of others indicate early quiescence and record whatever processes occurred during and immediately after formation (e.g., Moon, Mercury, Callisto). The surfaces of still others reflect a currently active state and provide insight into processes that are still evolving (e.g., Earth, Io, and perhaps Venus). Various stages of development represented by the various bodies provide a panorama of planetary evolution.

Much of our modeling of planetary bodies relies on ideas developed for the thermally senescent moon or the highly active earth. However, when attempting to compare the evolution of the earth with that of other planets, one must select from various segments of earth history, because there is no comprehensive model for earth evolution. In particular, processes that occurred during the first half of earth history are poorly understood in comparison with those of its more recent history. Yet it is the first half of earth history that should provide the conceptual link to the evolution of other planetary bodies, for it is during this earth period that the various paths of evolution were determined by the initial conditions. The moon and earth are drastically different in such initial conditions as size, content of volatiles, and density. Explanation of the differences in their observed evolutionary trends should help provide better models to explain features observed on other bodies. There has been a well-organized effort over the past decade to develop such models for the moon, but such efforts for the early earth have been more fragmented and less comprehensive.

The earth's accretion was dominated by impacts, but post-impact processes produced complementary oceanic and continental crust. Accretion greatly affected the distribution of volatiles and the components that formed the core and mantle. Where these important components of the earth were located at various times in the first two billion years of its history is a first-order problem, the answers to which will influence many other hypotheses. Secondarily, the evolution of the mantle strongly influences crustal development, epecially in the extent to which convection occurred. The time-frame and effectiveness of the impact process must also have influenced mantle heterogeneities through the addition of energy, and the evolution of the crust through mechanical reworking. The post-accretion, post-impact internal evolution of the earth produced units preserved from 3.8

billion years onward, and possibly earlier. Each of these stages may have contributed observable remnants that can be studied, with varying degrees of difficulty, from existing ancient terranes. A number of key questions of crustal evolution are listed below.

- How did the earth accrete from the solar nebular condensate and what were the initial thermal conditions?
- Did a surface magma ocean form which solidified to form a differentiated thermal protocrust and mantle?
- How did later differentiation and recycling between crust and mantle proceed?
- What was the extent of meteorite and asteroid bombardment of the earth during its first 3 billion years of history?
- What was the thermal regime of the earth during its first 3 billion years?
- When did plate tectonics begin, what were tectonic processes before that time, and what caused the transition?
- How did the oceans and atmosphere form and how did their compositions differ from those of today?
- What caused the continents to grow, and what stablized the ancient shield areas of the continents?
- How do changes in the types of sediments deposited during earth history reflect changes in tectonic processes?

2. Extraterrestrial Impacts and Influence on Earth's History

Study of the other planets and moons of the solar system has shown that each one has had a history of bombardment by smaller objects. Much of this took place during the first billion years of solar system history, but evidence on both the earth and the moon shows that impacts have continued to occur, although much less frequently, since those early times. The frequency of these later impacts and the degree to which objects of particular sizes may affect habitability on earth is now a topic for major research.

Stimulation for this research has been brought about by the currently controversial hypothesis, based initially on trace element geochemistry, that rapid evolutionary changes about 65 million years ago, typified by the extinction of dinosaurs and many marine invertebrates and by the emergence of mammals as the dominant terrestrial vertebrates, were related to an asteroid or comet impact with earth. Earlier dramatic changes in marine invertebrates may have had similar causes and are being examined in this light.

Solution of this highly interdisciplinary problem involves knowledge of evolutionary paleontology, biostratigraphy, regional geology, sedimentary processes, geochronology, plate tectonics, meteorite geochemistry, oceanography, celestial mechanics, cratering theory, and atmospheric physics; and it depends on the availability of opportunities for effective interchange of ideas and for educational cross-fertilization. Progress in the critical fields of study is dependent on the availabilty of computational, analytical and engineering test equipment.

E. MARINE GEOLOGY AND GEOPHYSICS

Because marine geology and marine geophysics are not funded through the Earth Sciences Division, extensive discussion is not presented here. The opportunities in these areas have been described in the COSOD, Ocean Crustal Dynamics, and Continental Margins reports.

Since the 1950's, marine geology and geophysics have played a major role in describing the basic patterns and motions of the major lithospheric plates of the earth's outer shell. It was the geophysical and geological evidence for seafloor spreading gleaned from studies in the ocean basins that resulted in wide acceptance of the theory of plate tectonics during the late 1960's. Marine studies have also demonstrated that changes in climate, sea level, sea water chemistry, and even the evolution and extinction of organisms may be related to the horizontal and vertical motions of the plates in the oceanic realm.

Although the seafloor is no more than 200 m.y. old, the ocean waters and their dissolved salts, and the sediments on the seafloor not only hold the best preserved record of this portion of geological time, but also are the integrated record of processes occurring earlier in earth history.

As we have moved to greater appreciation of the dynamics of the earth, we have become concerned not only with the nature of the seafloor, but also with seafloor processes and variations in their rates. The geological record gives a picture of plate motions at integrated rates of centimeters per year, but just as the biologists and the paleontologists are debating whether evolutionary processes occur gradually or in bursts in response to environmental stress, we are finding indications that tectonic movements are not continuous and gradual, but are intermittent and rapid. While the driving mechanism may be slow and constant, the response to the driving mechanism may be sporadic.

Determination of the time dependence of geological phenomena requires observations where the phenomena are taking place. At diverging plate boundaries, opportunities for observations are relatively limited on land and more diversity is available at sea. Opportunities for observations of transform boundaries on land are somewhat better. In the case of subduction zones, coordinated measurements both on the landward and seaward portions of the affected regions will be required to determine the time dependence of the motions. Very Long Baseline Interferometry methods may provide measurements of the relative motions of plates with an accuracy of a few centimeters.

Marine geological, geophysical, and geochemical investigations are in a transition stage from broad-scale reconnaissance to high-resolution studies in specific areas, from a more descriptive phase to one focusing on processes and mechanisms. We are beginning to realize, through new instruments, techniques, and vehicles and through drilling, that understanding the seafloor spreading process, whose imprint remains in all ocean crust, requires detailed investigations within the oceanic plate boundary zones. Similarly, detailed investigations of transform faults and subduction zones are improving our first-order models of these features and revealing insights into the dynamic forces acting upon them.

At the present time, we know a great deal about the shallow structure of the continents, but little about their tectonics. In the seas, the reverse is true. In the oceans, the current focus is on processes and on the structural details we are now equipped to examine. On the continents, the focus is on a different, though in part related, set of processes and the manner in which these have created the geological relations that we see today. The boundary areas between the two, the continental margins, are most complex; understanding how, when, and why they were shaped will depend upon observations coordinated from clear-cut continental crust to well-defined oceanic crust.

Use of the drill in the deep oceans has provided calibration for indirect geophysical measurement, has confirmed the concept of seafloor spreading and the nature of the oceanic crust, has initiated a new field of study, paleo-oceanography, and has demonstrated the presence of hydrocarbons in deep ocean basins. It is clear from the report of the Conference on Scientific Ocean Drilling (COSOD), held in 1981, that the opportunities for increasing our knowledge through drilling in the oceans and their margins continue to be outstanding. Future drilling, as a part of a larger program of geological and geophysical study of specific areas, promises to be of great importance in integrating continental and marine geology.

F. THE RESEARCH PRIORITIES

Contemplation of the research challenges and opportunities discussed in this chapter led to the development of the research priorities that form the principal recommendations of this report, as given in detail in Chapter 1. Each of these priorities cuts across and encompasses a number of the specific research challenges. The list of priorities follows, with the reminder that only the first item is considered to be of overriding importance and that no significance is to be attached to the ordering of the other topics.

1. A more detailed and accurate definition of the structure and composition of the continental lithosphere, including the continental margins.
2. Quantitative models for sedimentary basin evolution.
3. Improved understanding of magma generation and emplacement.
4. Knowledge of the physical and chemical properties of rocks.
5. A better understanding of tectonic processes, the physical and chemical states that produce them, and the structures that result.
6. A model of convection in the Earth's interior.
7. Evolution of life.
8. Surficial processes.

The importance to mankind of the results to be achieved by this research is discussed in the next chapter.

4

BASIC RESEARCH AND MAN'S USE OF THE EARTH

Man has taken from the earth and significantly altered its surface. The success with which he can continue to do so, without the destruction of his style of living and the character of the earth itself, will depend on his ability to improve his understanding of the earth and his impact on it. This understanding will come in part through the types of basic geoscience research discussed in this report. Most basic research in the geological sciences can be expected to assist mankind in identifying the best or least harmful ways to use the earth. Some of these applications of geoscience knowledge will be immediate, while others will evolve over decades.

Man's expanding use of the earth as a habitat, as a source of water, mineral and energy resources, and as a receptacle of wastes cannot continue indefinitely at the current pace. Resources are becoming depleted, portions of the environment have become contaminated, and construction projects continue into areas that are potentially hazardous or that would be better used for agriculture, resources or recreation. Inevitably, we must have better understanding for resource discovery, waste disposal, and geologic hazard reduction. In many of these areas the need is immediate.

Basic geoscience research underlies the development of predictive and remedial technology for our improved use of the earth. Nearly all the research areas discussed in this report are directly applicable to man's current use of the earth. Geoscience research can be separated into the following three principal categories that relate most closely to mankind's use of the earth:

A. Mineral and Energy Resources;
B. Geological Hazards; and
C. Man's Impact on the Earth.

These are briefly discussed below.

A. MINERAL AND ENERGY RESOURCES

Thomas Malthus outlined, over two hundred years ago, the potential conflict between an exponentially increasing population and a finite resource supply. Although the potential supplies of many commodities are enormous if we are prepared to pay the costs to recover them, mineral and energy resources are in fact limited. For many important commodities, the projected recovery costs for many deposits become staggering in only a few decades.

The United States uses huge amounts of energy resources and new mineral resources each year. Total U.S. consumption is about four billion tons of minerals, or about 40,000 pounds annually for each person. The 1981 per capita consumption of oil (25.39 barrels or 1,066 gallons), gas (86,956 cubic feet), and coal (6,000 pounds), are equally impressive. The United States is self-sufficient in most non-metallic minerals but is a net importer of several strategic metals (Yoder, 1982). Moreover, we imported approximately 2.18 billion barrels of oil in 1981, or about 37% of our total petroleum consumption. Discovery of new mineral and energy resources must thus continue, even though the deposits easiest to find have already been found, and lands are continually being withdrawn from further exploration and dedicated to other uses.

Energy and mineral resources are not evenly distributed within the earth's crust. Rich bodies of ore and large oil fields are only local anomalies, but their positions can be determined by understanding geological processes. Exploration technology for the discovery of these valuable anomalies ranges widely in effectiveness. For example, oil was first discovered from surface seeps. Now sophisticated, computer-enhanced, reflection-seismic techniques are employed to better interpret structure and stratigraphy, and even to indicate the presence of hydrocarbons within rocks. Exploration for many metals or other resources is less sophisticated, relying on surface indications, core drilling, or shallow-penetration geophysical techniques.

Discoveries of new mineral and energy resources will rely increasingly on improved predictive models. As our understanding increases, models can be developed that explain how oil, gas, and mineral accumulations have formed. This, in turn, will allow the development of improved techniques for their detection. Both the models and the detection techniques (geophysical, geological, and geochemical) require basic data on the physical characteristics and physical and chemical processes of the earth. The link, therefore, between basic research and resource discovery is remarkably direct and short.

Current geoscience research is achieving breakthroughs in the study of energy and mineral resources. For example, plate tectonic theory has provided a more coherent basis for investigating the accumulations of metal and fuel resources, allowing us to interpret exploration information more quickly and to pinpoint economical deposits. Improved understanding of depositional systems, mechanisms of subsidence, and the generation of fluid hydrocarbons from buried organic matter have led to the development of genetic models for the evolution of sedimentary basins containing oil and gas. Advances in isotopic chemistry, fluid inclusions, and thermodynamics have contributed to the development of genetic models for formation of mineral deposits. These genetic models, combined with improved airborne, surface and subsurface geophysical, geochemical and geological techniques, provide the only means for continuing the discovery of earth resources. Both the genetic models and improved detection techniques will require a continuing program of fundamental geoscience research in the properties and processes of the earth and its components.

Some areas of basic research also contribute to improvements in the applied technology used for development and production of mineral and energy resources. For example, improved understanding of the physical properties of subsurface reservoirs for petroleum and natural gas allows the design of effective techniques for so-called secondary recovery to produce hydrocarbon fluids that would otherwise be left in the ground. It is estimated that the nation has twice as much petroleum still locked in known reservoirs than has been produced to date in the entire history of domestic petroleum development. Comparable advances in the technology available for deep drilling for hydrocarbons and deep underground mining for metals can come only from a better understanding of how rocks fracture at depth.

B. GEOLOGICAL HAZARDS

The dynamic processes of the earth expose man to the natural hazards of earthquakes, floods, tsunami, landslides, expansive soils, erosion, and volcanic eruptions. Such phenomena are projected to result in cumulative losses in excess of $500 billion by the year 2020. Most of these events occur relatively infrequently, making them difficult to monitor, predict, or plan for effectively. The level of acceptable risk to the public is especially difficult to evaluate. The level of catastrophe for many of these hazards is largely a function of location. In most cases, these natural phenomena are relatively harmless in sparsely inhabited regions. As population density increases, however, the impact of future events on man may dwarf previous events in terms of loss of life and damage to property.

Natural hazards are the result of ongoing geological processes. Although an improved knowledge of these processes in no way eliminates the hazards, appropriate study and action can substantially reduce losses. Cities and structures could be more effectively sited and designed with geological factors in mind, so as to minimize the hazards. The prediction of locations and magnitudes of impending natural events could permit protective measures to be taken. The development of methods for the prediction of the nature and timing of such events is dependent upon additional basic research in many areas of the geosciences.

As an example, intensive earthquake prediction programs in Japan, China, and the U.S.S.R. have reported important successes, although few in number. It is beyond our current ability to forecast accurately the approximate time or magnitude of a future earthquake, and this is likely to remain the case for many years. In the meanwhile, disaster can be averted through prediction of the location and the maximum size of earthquakes, allowing the application of seismic design in construction projects. This is especially crucial when siting critical facilities (*Earthquake Research for the Safer Siting of Critical Facilities*, 1980). Through fundamental research, seismic parameters can be determined that, when recognized by land-use planners and structural designers, can significantly mitigate earthquake effects. This capacity can be enhanced by the development of risk maps, contingency plans, and plans to lessen the socioeconomic impact of an earthquake (*Working Group on*

Earthquake Hazards Reduction, 1978). Basic to both improved prediction and mitigation, however, is research on the properties and failure charateristics of earth materials, and the geological processes and domains related to earthquake occurrence.

Volcanic eruptions have recaptured the public fancy since the first eruption of Mount St. Helens in 1980. As early as 1975, Mount St. Helens was recognized as the domestic volcano most likely to erupt before the end of the century. Since the initial eruption, many of the eruptions have been successfully anticipated. Promising monitoring techniques, such as the measurement of gas emissions from active volcanos and the state of stress in seismically active areas, are likely to improve the prediction of eruptions further. However, reliable predictive methods will only be possible with the data derived from an improved worldwide monitoring network and with continued basic research on the geologic processes, applicable to both seismic and volcanic events.

Other geologic hazards, such as floods, landslides, and swelling soils, are probably even more damaging, albeit generally less spectacular. As population density continues to increase, and more unsuitable terrain is invaded by construction and development, geological hazards will increasingly take their toll. As with resource discovery, predictive methods based upon models of geological processes will have to be improved to identify likely sites of serious geologic hazards. Basic geological research on the physical and chemical characteristics of earth materials and the fundamental earth processes of magmatism, tectonism, and material failure will increasingly assist man in his coexistence with the partly dangerous habits of the earth.

C. MAN'S IMPACT ON THE EARTH

Man's presence on the earth has interfered with certain earth processes and significantly changed aspects of the earth's surface. These impacts are perhaps insignificant in geologic time, but many will affect man's use of the earth, and others may have lasting and even devastating effects. The most awesome impact is the contamination of water, soil, and air with all forms of chemical, nuclear, and animal waste. The effective disposal of toxic wastes is being delayed or obstructed in part because past research has not provided an adequate fundamental understanding of pertinent geological factors (Geological

Aspects of Industrial Waste Disposal, 1982). The simple combustion product (from fossil fuels) carbon dioxide has a great apparent potential for disturbing global climate over the next few centuries (Europe and Climate, 1977). Man-induced erosion and the use of portions of the earth's surface for inappropriate purposes are significant as well. Geoscience research related to these impacts involves a wide range of subfields.

Some three billion tons of urban solid waste are already being generated yearly. Human, animal and chemical (including radioactive) wastes are being produced in lesser tonnages, but are of much greater actual and potential health hazard. To the degree that wastes are not recycled or disposed of by burning, they end up in the earth and therefore become geological materials.

Undramatic but substantial losses arise from the destruction of mineral, water, and agricultural resources as a result of inadequately controlled urban growth. When construction sites preempt deposits of building materials or coal, groundwater recharge areas or prime agricultural lands, a national loss occurs. Estimates of these losses have not been made, but are surely in the billions of dollars annually. For example, estimates for California indicate that the annual loss in construction materials due to preemptive land use for other purposes is greater than $500 million in 1973 dollars. Smaller but significant losses are due to the collapse of slopes resulting from excavations for structures or roads, and subsidence of land through the mining and the withdrawal of water, oil, and gas.

Although the chief impacts of man are in urban areas, there are rural effects as well. Rural waste disposal is a large problem. Some two billion tons of agricultural waste are generated every year, composed of about one-third vegetable and two-thirds animal waste. Disposal of this mass poses potential serious health risks that may be reduced or avoided by proper management and consideration of geological factors. Likewise, minimizing the contamination of ground and surface water by chemical fertilizers and pesticides requires geoscience data and understanding. Other rural applications of the geological sciences include applications in coastal erosion control, flood control, soil conservation, water supply protection, and highway engineering.

In summary, basic geoscience research is one of the fundamental keys to improving man's use of the earth and minimizing his adverse impact upon it.

5

ESSENTIAL SUPPORT

This chapter reviews critical aspects of personnel and facility support necessary to advance our fundamental understanding of the continental lithosphere. To be sure, these essential support components will also be of great value in other aspects of basic and applied research in the earth sciences. Well-trained and creative scientists, their instrumentation, and supporting computational facilities are necessary to address adequately the priority research areas. Major new projects will require logistical and technological support unprecedented in fundamental research in the geological sciences.

A. MANPOWER AND TRAINING

In 1980, the National Science Foundation and Department of Education published Science and Engineering in the 80's and Beyond, which predicted the growth of certain fields of science from 1980 to 1990 in terms of increased employment by industry, universities, and government. Among the highest of the predicted growth rates was that of the earth sciences--more than 40 percent.

This increasing awareness of the importance of geosciences has already resulted in a 100 percent increase in the number of college seniors in the past decade--4,389 in 1971, 9,399 in 1980--with a total enrollment in the geological sciences of 40,000 in 1979-80 and 44,450 in 1981-82 (Student Enrollment in Geoscience Departments: 1981-82, 1982). About 8,900 degrees in the geological sciences were granted in 1980 and 9,400 in 1981. We estimate that there are at least 70,000 professionals in the geological sciences.

The training also grows ever more stringent. Geologists must keep pace with the best methods of physics, chemistry, biology, and engineering and consequently require strong preparation in those topics as well as in their specialties. At the same time, geological scientists must be cognizant of the social and economic influences of their work and in the new knowledge and techniques peculiar to geology.

A high proportion of scientists working in the geological sciences were trained in other fields--mathematics, physics, chemistry, engineering, etc. Exciting unsolved problems in the earth sciences, such as the dynamics of the earth, have fascinated a wide variety of scientists either after or while attaining their degrees in disciplines other than the geological sciences. The history of geology is replete with examples of dramatic jumps in knowledge brought about by this infusion of new talent and novel approaches. For example, it was a meteorologist--Alfred Wegener--who initially stimulated the plate tectonics "revolution." This interchange of personnel and ideas between disciplines is critical to the continuing health of the geological sciences and must be encouraged by the intellectual excitement in the field and by providing the resources that will attract the best bright young scientists to it.

If society is to benefit fully from the potential of geoscience, the concepts should be included in the general education of our citizens during elementary and secondary school years. Although a few states and institutions have moved somewhat in this direction, most students leave high school without much understanding of the planet earth. Moreover, there has been a large decrease in the number of future earth science teachers being trained for secondary schools. Of students who planned to be earth science teachers, there were 2,569 at graduate and undergraduate levels in colleges and universities in 1971; in 1980, there were 536, and in 1982, 350. The top salary offered to bachelor's and master's degree candidates for earth science teaching in 1982 was $14,000 as compared to industrial salaries of $24,000 to $28,000.

More and more our citizens are faced with problems that require geological information to help solve. The problems include the availability of resources, safe disposal of toxic and other wastes, avoidance of unnecessary exposure to natural hazards, sufficiency and quality of water, safe siting of critical facilities. No science is more germane to the everyday life of our citizens, and the subject matter should be the background of an informed layman. More of our educational programs should include knowledge of the geological sciences.

B. INSTRUMENTATION

In the earth sciences, as in science in general, advances in instrumentation lead to the capability to make new measurements, to increase spatial or temporal resolution, and to increase precision or accuracy. Historically, such advances have opened new areas for basic research and have led to the development of a higher level of fundamental understanding of processes and materials of the earth. There is a need to foster developments in instrumentation, to maintain such progress, and to insure that state-of-the-art instruments and facilities are available and used in the training and education of future researchers.

The decadent state of instrumentation in the research laboratories of American universities has been well documented for a decade or more. The NSF/NRC study of 1972 entitled Survey of Research Equipment Needs in Ten Academic Disciplines estimated the deficit for all equipment to be $276 million. In 1982, the NRC report entitled Revitalizing Laboratory Instrumentation estimated the accumulated need to be at least $1 billion.

The geological sciences were deficient by $33 million in instrumentation in 1972, and this translates to between $100 to $200 million in 1982. This problem has serious repercussions. Academic research cannot stay at the cutting edge of science. Students are not familiar with state-of-the-art instrumentation. Society does not have the benefit of cutting-edge science to help solve many pressing problems. The economic progress of the U.S. instrumentation industry declines.

The specific current instrumentation needs in geochemistry and mineralogy are being studied (Smith, Joseph V., Instrumentation Crisis for VGP, EOS, V. 62, No. 47, November 24, 1981). An ad hoc committee of the Board on Earth Sciences (formerly the Geological Sciences Board) submitted an unpublished report to the Board indicating a great need for geochemical and mineralogical instrumentation based on correspondence with departmental chairmen at sixty-five universities and other individuals (Report of the Ad Hoc Committee on the Status of Geochemical/

Mineralogical Instrumentation, 1982). Indeed, the needs are real as they also are in other parts of the geological sciences. In addition, field instrumentation has become increasingly complex. The first critical need is to increase the funding for instrumentation. Secondly, there is need for a forum to establish strategy for ensuring long-term stability in state-of-the art university instrumentation.

It is important to note that the status of instrumentation is a problem that will remain as long as technological innovation in instrumentation continues. The user of off-the-shelf instruments will always find the need, in terms of his research problem, for higher precision, resolution, speed, and measurement of new parameters. The developer of frontier instrumentation needs incentives for his pioneering efforts. The rapidity of change from vacuum tube to transistor to very-large-scale integrated circuitry in the last two decades has exacerbated the problem we face today. Consequently, there is a major need to provide a means for recovering from this transient problem, while still recognizing the need for long-term and continuing efforts to avoid recurrence.

C. THEORETICAL STUDIES

This report has emphasized the observational and experimental nature of the geological sciences, but sound interpretation of data requires that the theoretical underpinnings of the subject be developed in parallel with other new efforts. The research objectives are often stated in terms of the development of a quantitative model of geological processes. Because the conclusions of the geological sciences are often inferences about conditions and processes that are remote in space (in the deep interior) or remote in time, these conclusions can be no better than the theories on which they are based. Attempts at modeling span subjects from the convection in the mantle to paleoclimatic histories.

Many areas of theoretical physics and chemistry produced results applicable to the earth sciences. The physics of continuous media, including modern nonlinear continuum mechanics, is one area on which geophysicists have drawn and to which they have made important contributions. Elastodynamics and fracture mechanics are topics within this subject that are especially important to earth science. Solid-state physics and physical chemistry at elevated temperatures and pressures offer results of prime importance to earth studies. At the same time, the earth itself is the best laboratory available for observations of the behavior of materials under extreme conditions. Basic work in both organic and inorganic chemistry provides insight for interpreting geological data.

Theoretical studies sometimes seem less glamorous than big data-gathering efforts, but the field cannot reach its full potential unless these studies continue to receive adequate support.

D. GEOSCIENCE DATA AND COMPUTATION

The rapid developments in digital computers and their extensive use in acquiring, manipulating, reducing, and storing data have led to the dawn of the "information age." Micro- and mini-computers are in extensive use in acquisition of geochemical and geophysical data. Their widespread application has resulted in many advances in the instrumentation arena. High-speed megabit/second communication links provide information transfer capability, sharing of peripherals, and real-time graphics display that would have been inconceivable only a decade ago. Software and high-level programming languages are becoming machine-independent at an increasing rate. These and related developments are providing new options for the examination, analysis, and interpretation of geoscience data.

New approaches to data base management, such as self-formatting file structures, offer the potential for reducing or eliminating many of the problems associated with accessing, disseminating, and standardizing information in large data bases. Application of computers in text and figure preparation, as well as in accounting and inventory functions, are not considered research, yet their use provides free time for thinking. It is in this sense that both the data and computation *per se* are considered essential support for research in the geological sciences.

The problems of the National and World Data Centers have been reviewed (*Geophysical Data Interchange Assessment*, 1978). However, this is a problem area which will require continued effort and attention (by NSF and other organizations) if these data resources are to be fully utilized.

Part and parcel of the opportunity presented by advances in computation and data processing is the need to fully utilize modern telemetry and satellite communications links to transfer data from remote sites to centers for rapid dissemination and use in academic and applied research.

Scientists should be prepared to invest time in data management, and the NSF should help fund data centers. The scientific community and the government have largely ignored the problem for too long. Geophysics, geochemistry and petrology and other areas are in need of sophisticated data management.

E. MAJOR PROJECTS AND FACILITIES

During the 1980's, we can be confident of an increasing need for medium to large interdisciplinary scientific programs in the geological sciences that will require major funding for equipment and staff for operations and maintenance. A characteristic of these programs is that the support required exceeds that obtainable by the normal funding methods for standard research grants. Commonly these complex programs require research consortia, and they may also require major new facilities to provide essential specialized services.

One of the most exciting and productive programs ever undertaken in the geological sciences has been the NSF-supported Deep Sea Drilling Program, a good example of the programmatic scope discussed in this section.

Another example is deep seismic reflection investigations, including the very successful NSF-supported Consortium for Continental Reflection Profiling (COCORP), which have added the third dimension to detailed geological observations made at or near the surface. This program, operated by a consortium of universities using dedicated facilities, has extended methods of seismic profiling and data processing to study the earth's crust and upper mantle, and detailed data on complex structures are now available to depths of about 40 km from a number of locations. Applications to deeply buried geological structures represent an exciting new scientific frontier. The method offers a means to determine the structure and hence the evolution of the deep continental crust. Programs using this method of investigation should be expanded, even though costly.

Such projects as the DSDP and COCORP require careful management and so scientific administrators are needed in ways not foreseen before. Persons trained in both science and management will be increasingly required to dedicate their careers to such undertakings, and will need satisfactory recognition both monetarily and through recognition by their peers. Especially needed are projects requiring contributions from industry, universities, and government agencies such as the USGS. Many western nations are now attacking major scientific problems through the efforts of such a triumvirate, but we have hardly started in the United States. We may soon lose our "cutting edge" if we fail to draw on the skill within the triumvirate, but the organizational and personnel task is imposing. In the study of sedimentary basins, for example, much data that was originally proprietary for sound reasons in our competitive economy no longer need be confidential. But once so classified, they seem to remain under lock and key. Properly conceived and organized prospects aimed at investigating the history of certain basins will require the cooperation of industrial companies in bringing these data to light and helping in their interpretation. A new breed of science leader will be required to meld the talent and uncover the data from all the segments of this triad. Programs of sound research involving the cooperation of academia, government agencies, and industry which show promise of opening new ways for doing consortium research in the United States therefore require special nurturing and funding.

Deep continental drilling provides a means of measuring rock properties, establishing geologic structure, and observing earth processes at exceptional depths. The Soviet Union has been embarked on an ambitious scientific drilling program for over a decade, and the governments of France and West Germany are also starting scientific drilling programs. The drilling done by industry (estimated to be billions of dollars annually) and government (about $500 million annually) can be used to great advantage by making scientific measurements when appropriate opportunities arise. Some holes should also be deepened to scientifically exciting depths using special funding. A report, Continental Scientific Drilling Program (1979), discusses major scientific problems that could be attacked by experiments using deep holes drilled for other purposes. Relatively small amounts of money (compared to the cost of the drill hole) spent on add-on scientific programs can yield significant data. An NAS/NRC Committee on Scientific Drilling has been established to coordinate the use of existing holes and to plan for future work. Special organizational emphasis in NSF and a considerable increase in funding are required to foster an add-on scientific program on holes drilled for other purposes.

Deep holes drilled entirely for scientific reasons or "dedicated deep drilling" have always been a wistful gleam in the eyes of geologists but have always seemed too expensive in comparison with other needs. Now, however, based on the information we are getting about the deep continental structures from geophysical probes, we are rapidly approaching the time when direct sampling by drilling will be the only way to calibrate geophysical data. We believe that dedicated drilling on the continents requires immediate planning and drilling should be undertaken in 3 to 5 years.

The NSF supports marine seismic profiling and a program for recording long-period earthquake waves. The former is currently being considered in a report by a committee of the Ocean Sciences Board (NAS/NRC) on marine geology and geophysics and the latter has been considered with global seismographic networks in an NRC report entitled Seismographic Networks: Problems and Outlook (1983). The NAS/NRC Committee on Seismology is partially supported by NSF, and a report is being prepared on seismological studies on the continental lithosphere. Tasks are addressed in which seismology can make significant contributions to understanding the evolution, dynamics, and nature of the continental lithosphere. An area in which NSF coordination and oversight would be fruitful is the maintenance and management of the growing national and international systems of seismographic networks. The networks fall into two categories: (1) those for recording earthquakes over long time intervals and (2) those established for specific purposes, basic research, and/or applied problems. All networks require continual upgrading, including numbers and locations of stations, due to changing problem requirements and improved technology. Severe financial problems are now involved in their continued development and operation.

The rapid development of digital seismographic equipment and the increased availability of computers suitable for processing the digital data have led to demonstrations of the power of digital data for the solution of major problems that have been previously unapproachable. Calculation of the kinematic and dynamic properties of seismic sources and resolution of details of the structure of the earth's interior in three dimensions are two areas in which the use of digital data is already yielding significant new knowledge. These developments make it timely to evaluate the global seismographic observatory system and take remedial actions as needed. The global system--consisting of hundreds of stations supported by many countries, and, in particular, the World-Wide Network of Standardized Seismographs (WWSSN), supported by the United States since the early 1960's--is the principal data source for much of whole-earth geophysical research using seismology. These are mostly analog-recording systems. The WWSSN observatories use 1960's vintage equipment, which has been reliable and effective. Now is the time to develop a plan for the replacement of this network by a digital network, one which incorporates near-real-time data transmission by satellite and a national data center to which the data will come and from which users can be served. In making this plan, not only the needs of those scientists who have the facilities for processing digital data must be considered, but also the needs of others less well-equipped. No funds are allocated in this program plan for the development of a global digital seismographic network, but a start should be made soon on the plan for one.

The plate tectonics concept has unified the field so that workers in different subfields of the science are cooperating as never before. This cooperation will result in an increased number of consortium studies. We recommend that NSF give special consideration to administration and funding of major projects and facilities. It may become desirable to establish a special program to shepherd projects of this scope.

6

FUNDING IMPLICATIONS*

The objectives of NSF's Earth Sciences program are a better understanding of the earth, its history (including the history of living things), its composition and structure, and the various physical and chemical processes that have contributed to its evolution. To accomplish these objectives, the Foundation supports work in all subfields of the geological sciences through the following programs: Stratigraphy and Paleontology, Environmental Geosciences, Crustal Structure and Tectonics, Seismology and Deep Earth Structure, Experimental and Theoretical Geophysics, Petrogenesis and Mineral Resources, Mantle Geochemistry, and Experimental and Theoretical Geochemistry. About 1,000 proposals were received by the Earth Sciences Division in 1982. About 40 percent of the proposals are funded; a successful investigator gets about 75 percent of the support requested. If sufficient money were available, the quality of the proposals would justify about 70 percent of them being successful--about twice the number that is funded. The average grant in 1981 was about $25,000 as compared to $48,000 in 1967 (both in 1972 dollars), even though the proportion of a grant spent on overhead and salaries has increased markedly since 1967.

The Foundation supplies about 90 percent of total federal funds to colleges and universities for basic research in the geological sciences. The budget of the Earth Sciences Division was $24.9 million in FY 1979 and increased to $34.2 million in FY 1983. This increase of 37 percent (an average of 7.5 percent per year) has not kept up with inflation.

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*Appendix B summarizes the budgets of federal agencies other than NSF for research in the geological sciences.

Between 1965 and 1981, however, striking new developments in the field (especially plate tectonics) and an increased application of earth sciences to societal needs caused a tremendous growth in the number of earth scientists (see Chapter 5, Manpower and Training). As a result, NSF now receives annually 330 percent more research proposals than it did in 1965. To restore the Foundation's 1965 record of percentage of proposals funded and dollars awarded per approved proposal would require an increase in the NSF's Earth Sciences budget by a factor of about three.

The earth sciences are perceived by most nations as one of the more dynamic branches of science. The science funding agencies in several Western European countries devote about 5 percent of their total budgets, including those for medicine and the humanities, to support for research in the earth sciences. NSF allocates about the same percentage of its budget to earth sciences research; the NSF budget, however, does not include funds for medicine and the humanities and, as a consequence, the percentage of the total science budget allocated to the earth sciences is smaller in the United States than in other countries. Moreover, many countries are expanding their support for the earth sciences. France, for example, is planning for a 20 percent real growth in earth sciences funding over the next several years, and West Germany intends to support an ambitious drilling program, costing some $85 million, in order to gain a better understanding of the structure and evolution of the earth's crust. The Soviet Union has supported a scientific drilling program costing many tens of millions of dollars per year for the past 13 years.

Both the Soviet Union and the People's Republic of China allocate very sizeable financial and personnel resources to huge earth sciences research programs, with the express intent of strengthening their economies and solving internal problems related to geological hazards. Countries like Poland and Czechoslovakia direct a large proportion of their geological budgets to studies in third-world countries. In contrast, the United States in recent years essentially has abdicated its former preeminent position in supplying technical assistance in the earth sciences to developing countries.

Unlike most other branches of science, most graduate students in the earth sciences do not receive NSF support due to the tight funding in this discipline. This must be changed in order to attract more capable students to the field.

Support within the field of earth sciences has been broken down into a number of subdivisions. These subdivisions are major scientific areas that warrant increased attention. Superimposed on these subdivisions are the funding elements that implement the science. These are:

- Standard Research Grants: The NSF "Standard Research Grants Program" is the backbone of the Foundation's research funding. Increased funding is recommended for those grants awarded to individuals submitting unsolicited proposals predominantly for research or instrumentation at or with an academic institution. At present, investigators are being funded at levels so low that they can barely accomplish their tasks.
- Instrumentation: Over the past decade the capability to acquire and support major items of capital equipment in the academic community has been deficient by almost 20 percent per year (see Chapter 5, Instrumentation). The result is a reduction in research potential and a significant decline in the capabilities for the training and development of young scientists who are the nation's resource. Correction of this critical situation will require funding of about $15 million per year. This increased funding is needed immediately both for instrumentation and for support of technical staff, amounting to an annual cost of at least a third of the cost of the instruments.
- Consortium Projects and Major Facilities: The earth sciences, through the unification provided by plate tectonics theory, have reached the stage where a consortium approach can yield large dividends and where costly experiments and facilities are required to attack obvious problems. Multidisciplinary teams can address problems too large for individual investigators. NSF should support these teams vigorously.
- Seismic Networks: Support of seismic networks is the responsibility of a number of government agencies. Such support is failing to provide upgraded networks and supply optimum data for seismological research. NSF should take an interest in this problem and provide additional funding for networks in order to satisfy the research discussed in this report.

- Deep Crustal Seismic Studies: Seismic reflection and refraction methods provide the only means to determine the structure and hence study the evolution of the deep continental crust. The very successful NSF-supported Consortium for Continental Reflection Profiling has provided exciting information on the structure of very limited areas of the United States. Such efforts need to be expanded. The coverage is now limited and needs to be complemented by the powerful technique of seismic refraction. Effort in this whole area should be at least doubled over the next few years.
- Data Management and Computation: Many subfields of the geological sciences now require major data handling and computation facilities. Some seismological problems cannot be attacked at present because of the lack of a major computational facility, and modeling by computer is becoming an important part of many areas of the science. Data management will become an increasingly important component of research over the next decade, yet both the scientific community and NSF have largely ignored the problem. We recommend increased attention and the necessary funding to accomplish useful data management.
- Continental Drilling: Relatively small amounts of money (compared to the cost of the drill hole) spent on add-on scientific programs and deepening of existing holes can yield large amounts of hydrological, geophysical, geological, mineralogical, and geochemical data on that portion of the continental crust that is inaccessible without the drill. Such information could provide much greater understanding of the structure, evolution, and composition of the continental crust. We recommend that funds, up to a few million dollars per year, be made available to fund holes of opportunity, for deepening existing holes, for downhole instrumentation, and for sample studies.

A program of drilling holes dedicated to scientific studies is highly recommended over the next few years, as the structure of the continental crust becomes better understood. Just as critical areas of the oceanic crust have been tested by the drill with remarkable results, similar exciting discoveries can be anticipated from a program of continental scien- tific drilling. Such a program would be expensive, but the returns would be great. By 1990, $20 million per year could be dedicated usefully to this goal. We recommend that a research initiative for continental drilling start in 1985 at a funding level of $4 million per year.

The committee concludes that support of fundamental research by the Earth Sciences Division of the National Science Foundation should be augmented substantially. Increased funding is fully justified by the potential discoveries, and the additional effort should be an essential part of national science policy. In our view, an appropriate response to the needs of the field would require an annual increment of $21 million to the President's 1984 budget. A goal of an additional $53 million over the 1984 budget for NSF's Division of Earth Sciences is a justifiable and realizable goal by 1990.

APPENDIX A

COORDINATION OF ACTIVITIES

The Board on Earth Sciences undertook to publicize this study widely to insure that the committee report would be broadly representative of the opinion of leaders in the field regarding research priorities in the geological sciences for the decade of the 1980's. A statement dealing with the activities of this committee was sent to 24 professional earth science societies for publication in their journals. A list of these societies appears at the end of this appendix. Letters were sent to the chairmen of 140 U.S. university departments of geological sciences inviting the collective suggestions of the faculty, to members of the NAS sections on Geology and Geophysics soliciting their opinion, and to the chairmen of committees and boards of the Commission on Physical Sciences, Mathematics, and Resources of the National Research Council having activities related to the earth sciences. Letters were also sent to all state geologists and to the heads of governmental agencies doing fundamental research in the geological sciences. All the letters requested the identification of research topics with high potential for major advances in the scientific understanding of the earth. A list of respondents follows, many of whom wrote insertions that were used in this report.

RESPONDENTS

William C. Ackermann, University of Illinois at Urbana-Champaign
Moid U. Ahmad, Ohio University
Robert C. Aller, University of Chicago
Richard C. Allison, University of Alaska, Fairbanks

Alfred T. Anderson, University of Chicago
Don L. Anderson, California Institute of Technology
Raymond E. Arvidson, Washington University
Thomas C. Bache, DARPA
Paul B. Barton, Jr., USGS
Louis J. Battan, University of Arizona
Charles R. Bentley, University of Wisconsin-Madison
Leon L. Beratan, USNRC
Joyce R. Blueford, USGS
E. G. Bombolakis, Boston College
John D. Bossler, NOAA
Douglas G. Brookins, University of New Mexico
Peter R. Buseck, Arizona State University
James B. Campbell, Virginia Polytechnic Institute
Anil K. Chopra, University of California, Berkeley
Robert N. Clayton, University of Chicago
Robert Coleman, Stanford University
David C. Crane, D.C. Crane, Inc., Dallas
John N. Davies, University of Alaska
Ray F. Dawson, Winter Park, Florida
John S. Dickey, Jr. Syracuse University
Bruce R. Doe, USGS
Richard Y. Dow, NRC - Committee on Recommendations for
U.S. Army Basic Scientific Research
David H. Elliot, Ohio State University
E. R. Engdahl, USGS
Albert E. J. Engel, Scripps Institution of Oceanography
Larry W. Finger, Carnegie Institution of Washington
R. Michale Gall, USGS
Robert N. Garrels, University of South Florida
Jenny P. Glusker, The Institute for Cancer Research,
Philadelphia
T. Gold, Cornell University
Julian R. Goldsmith, University of Chicago
Julian W. Green, Harvard University
Lawrence Grossman, University of Chicago
Kate Hadley, Exxon Company, Houston
William W. Hambleton, University of Kansas
Robert M. Hamilton, USGS
John W. Harbaugh, Stanford University
Christopher G. A. Harrison, University of Miami
C. Vance Haynes, University of Arizona
John G. Heacock, ONR
John F. Hermance, Brown University
M. King Hubbert, Bethesda, Maryland
John C. Jamieson, University of Chicago
Barclay Kamb, California Institute of Technology

G. R. Keller, University of Texas at El Paso
Ole J. Kleppa, University of Chicago
Leon Knopoff, University of California, Los Angeles
George A. Kolstad, DOE
Paul F. Krumpe, AID
Mark Langseth, Lamont-Doherty Geological Observatory
Kai N. Lee, University of Washington
Robert C. Liebermann, State University of New York at Stony Brook
L. T. Long, Georgia Institute of Technology
William C. Luth, Sandia National Laboratories
Thomas F. Malone, Butler University
John C. Maxwell, University of Texas at Austin
Thomas V. McEvilly, University of California, Berkeley
L. D. McGinnis, Northern Illinois University
Christopher N. K. Mooers, Naval Postgraduate School
Paul B. Moore, University of Chicago
Benjamin A. Morgan, USGS
John D. Morgan, Jr., U.S. Bureau of Mines
Haydn H. Murray, Indiana University
Norman D. Newell, American Museum of Natural History, New York
Robert C. Newton, University of Chicago
*Meredith E. Ostrom, University of Wisconsin-Madison
John A. Philpotts, University of Hawaii
David Pimentel, Cornell University
John C. Reed, Jr., USGS
Gregory J. Retallack, University of Oregon
John H. Reynolds, University of California, Berkeley
Frank M. Richter, University of Chicago
William R. Riedel, Scripps Institution of Oceanography
G. D. Robinson, USGS
J. William Schopf, University of California, Los Angeles
Thomas J. Schopf, University of Chicago
John H. Schuenemeyer, University of Delaware
John S. Scott, Geological Survey of Canada
J. John Sepkoski, University of Chicago
Mark Settle, NASA
Joseph V. Smith, University of Chicago
Sean Solomon, Massachusetts Institute of Technology
A. F. Spilhaus, Jr., American Geophysical Union

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*Dr. Ostrom is the President of the Association of American State Geologists. He coordinated all responses from state geologists.

Don W. Steeples, University of Kansas
Christopher J. Stohr, Illinois Department of Energy and Natural Resources
Spencer R. Titley, University of Arizona
John Verhoogen, University of California, Berkeley
H. Jesse Walker, Louisiana State University
Thomas L. Wright, USGS
Oliver R. Wulf, California Institute of Technology
Peter J. Wyllie, California Institute of Technology (formerly University of Chicago)
Hatten S. Yoder, Jr., Carnegie Institute of Washington
John M. Zeigler, College of William and Mary
Alfred M. Ziegler, University of Chicago

PROFESSIONAL SOCIETIES RECEIVING STATEMENTS FOR PUBLICATION

American Association of Petroleum Geologists
American Congress on Surveying and Mapping
American Crystallographic Association
Anerican Geographical Society
American Geological Institute
American Geophysical Union
American Institute of Mining, Metallurgical, and Petroleum Engineers
American Society of Limnology and Oceanography
American Society of Photogrammetry
American Water Resources Association
Association of American Geographers
Association of Engineering Geologists
Clay Minerals Society
Geological Society of America
International Association of Mathematical Geology
Marine Technology Society
Paleontological Society
Seismological Society of America
Society of Economic Geologists
Society of Economic Paleontologists and Mineralogists
Society of Exploration Geophysicists
Society of Vertebrate Paleontology
Soil Science Society of America

APPENDIX B

EXISTING FEDERAL RESEARCH PROGRAMS

The earth sciences touch many facets of our lives, and, as a consequence, many federal agencies are concerned with them. For the most part the activities are mission-oriented and focused on particular needs; many could best be described as applied research rather than the basic research supported by NSF.

Various estimates of annual totals of dollars expended range from about $200 million to more than a billion. All are equally valid. The wide variation reflects differences in the number of activities that are included as efforts in the geological sciences and also how the programs are cost-accounted. For example, a survey in the early 1970's estimated NASA's research support of the geological sciences at $10 to $15 million, whereas NASA's own figures totaled $100 million because the cost of space shots were pro-rated over all substantive activities. Similarly, today the whole USGS budget for FY 1983 totals about $528 million. But if one excludes topographic mapping and cooperative water programs (with states) the total is decreased by about $134 million. The proportion of the remaining sum that should be called research is still largely a matter of definition. With this caveat, we submit the following descriptions of agency programs.

U.S. Geological Survey

The Geological Survey has had for years the primary tasks of classifying Public Lands and compiling a geologic map of the United States. As an outgrowth of these missions the Survey has acquired, either by implication or explicit direction, other related functions, such as topographic mapping, water resource activities and, more recently, offshore surveys and lunar studies. In the process, the Survey has become the largest geological research institution in the country. Most of its work is "in-house," some of it funded by transfer of funds from other agencies. A small grants program provides some support to universities in earthquake prediction.

Annual Budgets (millions of dollars)

Activity FY	78	79	80	81	82	83
Topography	70	78	83	89	87	90
Geologic Mapping & Mineral Resources	163	178	194	208	208	197
Water Resources	146	169	185	194	187	194
Conservation*	77	85	106	127	130	---
Land Information & Analysis**	23	21	24	23	21	19
Nat. Pet. Reserve	203	217	170	122	7	---
General Support	16	17	20	21	12	28
Total	698	765	782	784	650	528
Amount estimated as geologic research	96	101	99	114	112	115

* Item will be dropped from USGS total after FY 82 due to reorganization in Dept. of the Interior.

**Called "Earth Sciences Applications" after 1980.

National Aeronautical and Space Administration (NASA)

Research in the geological sciences supported by NASA is related to lunar and planetary studies and to the application of space technology to the study of the earth. Part of the work is "in-house," part is extramural. The following budget summary includes only the earth applications.

Annual Budgets (millions of dollars)

Activity	FY 79	80	81	82	83
Geologic Mapping Techniques	3.0	3.2	3.3	3.5	3.5
Geopotential Fields Research	1.2	1.6	2.5	1.5	1.5
Crustal Dynamics Project	---	8.8	11.6	14.0	23.5
Total	4.2	13.6	17.4	19.0	28.5

Department of Energy

In the Geoscience Program of DOE, research support is provided with a long-range and generic view of energy technology and resource needs. Necessarily, much research is high risk. The abilities of DOE laboratories to move rapidly on technical problems and to integrate basic research with technology programs play a key role in the success of many of the geoscience research projects. Special research emphasis is currently on geochemical migration, rock mechanics, and continental drilling.

Annual Budgets (millions of dollars)

Activity	FY 79	80	81	82	83
DOE Labs	4.1	5.6	6.2	6.5	7.1
Universities	1.9	2.5	3.0	3.1	4.7
Equipment	0.4	0.6	0.9	0.9	0.9
Total	6.4	8.7	10.1	10.5	12.7

Research in the geological sciences is also supported in the Office of Health and Environmental Research, Office of Energy Research. Priority is given to core research directed to the processes that control the subsurface transport of energy residuals in the biosphere.

This research is directed to developing scientific information about hydrological transport processes and the characteristics of soils and lithologies that affect the transport of trace elements, organic compounds, and radionuclides. Important areas of disciplinary interest are groundwater geology, hydrology, and geochemistry.

Annual Budgets (estimated millions of dollars)

Activity	FY 81	82	83
Hydrological Transport (surface and ground water hydrology)	1.3	1.6	1.8
Marine and Estuarine Goechemistry	1.2	1.2	1.2
Total	2.5	2.8	3.0

U.S. Army Research Office

The Geosciences Division of the U.S. Army Research Office (ARO) supports research in the atmospheric and terrestrial sciences. The continental environments involved cover the entire range of land surfaces, including perpetual snow or ice, tundra, deserts, rivers and lakes, forests, etc., and under all weather conditions, favorable and adverse, clean air and polluted air. Interdisciplinary aspects of those atmospheric and terrestrial problems of concern to the Army are currently targets for increased funding.

Annual Budgets (millions of dollars)

Activity	FY 81	82	83
Geoscience	3.4	3.9	5.1
Percent GS	7.3	6.6	6.8
Total	47.0	59.0	74.0

Office of Naval Research

ONR supports research in solid earth properties, sediment transport, geomorphic features, tectonics and geomagnetic phenomena. Additional support in the fields of acoustic and oceanic research is not reflected in the following table.

Annual Budgets (millions of dollars)

Activity	FY 78	79	80	81	82	83
Seismic Propagation and Seafloor Interaction	1.8	2.0	1.8	2.8	2.6	2.6
History and Dynamics of the Oceanic Crust	2.3	1.9	2.6	1.8	2.4	3.0
Ocean Sediment Dynamics	1.4	1.6	2.0	1.7	1.9	2.0
Marine Geodesy	.12	.15	.19	.17	.28	.29
Geomagnetics	.17	.22	.29	.30	.24	.41
Coastal Morphology Sediment Transport	.60	.65	.75	.83	.72	.71
Total	6.4	6.5	7.6	7.6	8.1	9.0

Defense Advanced Research Projects Agency

The Geophysical Sciences Division (formerly Nuclear Monitoring Research Office) of the Defense Advanced Research Projects Agency supports basic research in the geological sciences primarily to improve U.S. capabilities to verify compliance with treaties limiting nuclear explosion testing. The program also includes some research for other military geophysics applications. All the funds are spent outside of this agency with about 90 percent going to universities and other non-government organizations. A portion of the total budget has been identified as fundamental research. The remainder of the funds are for field tests, engineering prototype development and testing, and specialized systems or software development and evaluation.

Annual Budgets (millions of dollars)

Activity	FY 78	79	80	81	82	83
Fundamental Research in Geological Sciences	5.5	5.5	5.5	6.5	7.0	7.5
Total Research in Geological Sciences	10.07	9.30	11.63	16.96	18.53	18.23

Agency for International Development

The Agency for International Development, Office of U.S. Foreign Disaster Assistance (AID/OFDA) has supported activities in the geological sciences since 1979 in the areas of seismic monitoring, tsunami hazards and analysis, earthquake hazards mitigation, and engineering seismology. AID places great importance on developing seismic early warning systems and earthquake disaster preparedness in the developing world as a means of reducing the disaster death toll resulting from geological hazards. Accordingly, program development has expanded rapidly in the recent past.

Annual Budgets (millions of dollars)

Activity	FY 79	80	81	82	83
Seismic Monitoring Networks	0.14	-	0.419	0.680	0.970
Tsunami Analysis	-	0.030	0.171	0.233	0.225
Earthquake Hazards Mitigation	0.092	0.500	0.551	0.532	0.575
Engineering Seismology	-	0.007	-	0.324	0.055
Total	0.232	0.537	1.141	1.769	1.825

Nuclear Regulatory Commission

Research on earthquakes and other geologic hazards is conducted by NRC to help ensure the safety of nuclear facility sites and to provide information to help resolve problems of interpretation and implementation of the Seismic and Geologic Siting Criteria for Nuclear Power Plant Sites. The kinds of data required include the distribution of present seismic energy release, the kinds and distribution of other deformational processes, physical criteria for discriminating between active and inactive faults, and the location and extent of subsurface as well as surface features of the earth's crust that influence the occurrence of earthquakes. Development of the explanatory and unifying hypotheses that are needed for realistic assessment of seismic hazards requires extensive surface and subsurface geological and geophysical information like that being gathered in NRC research programs in seismology and geology.

Annual Budgets (millions of dollars), excluding Meteorology and Hydrology.

Activity	FY 78	79	80	81	82	83
Seismology/Geology	3.3	3.9	4.3	4.8	3.7	4.4

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