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Future Directions in

ADVANCED EXPLORATORY RESEARCH RELATED TO OIL, GAS, SHALE, & TAR SAND RESOURCES

Panel on Future Directions in Fundamental Research Related to Fossil Energy

Board on Chemical Sciences and Technology

Commission on Physical Sciences, Mathematics, and Resources

National Research Council (U.S.).

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PREFACE

The Office of Technical Coordination (OTC), under the Department of Energy (DOE) Assistant Secretary for Fossil Energy, is responsible for long-range, high-risk research that could provide major advances in technologies for the use of fossil fuels. From its inception through FY 1986, the programs of this office focused mainly on chemical, physical, biological, and engineering issues pertinent to coal combustion and clean fuels.

As part of a reorganization of responsibilities within the DOE Office of Fossil Energy, in late 1986 the Office of Technical Coordination was given responsibility for an existing program of research in Advanced Process Technology (APT) for oil, gas, shale, and tar sands. This significant broadening of the OTC mandate presented its management with a number of challenges and opportunities. Among the challenges was the prompt, effective integration of the APT program, which included some development activities, into the more fundamental research program of the office. Among the opportunities were the possibility of identifying future thrusts for APT in highly promising areas of fundamental research and the possibility of identifying and addressing important research questions that cut across the spectrum of fossil fuels and that would be appropriate for OTC sponsorship and support in light of the office's new and broader range of responsibilities.

To meet these challenges and opportunities, the OTC, with the concurrence of the then Acting Assistant Secretary for Fossil Energy, approached the National Research Council with a request to organize an advisory panel to examine future directions in fundamental research appropriate for sponsorship by the Advanced Process Technology program. The National Research Council's Board on Chemical Sciences and Technology agreed to empanel an advisory group with broad representation from the geosciences, physical sciences, and engineering disciplines to accomplish this task. The charge to the panel was to prepare a report for the director of the Office of Technical Coordination, identifying critical research areas. These research areas might include the following:

- novel and promising areas of fundamental research related to oil, gas, shale, and tar sands;

- scientific questions that cut across the full spectrum of fossil fuels, and for which OTC sponsorship would now be appropriate; and
- cross-cutting research areas currently being supported by the OTC coal science and technology programs that might be broadened or refocused beyond their current focus.

The panel was organized in the following way. A group of 14 researchers in chemistry, the earth sciences, chemical engineering, and other disciplines relevant to fossil energy research was invited to participate in a three-day workshop. The first part of this workshop was devoted to identifying and discussing a large number of key research needs related to oil and gas resources. After the panel had established an initial list of topic areas considered to be important and appropriate for federal support, a series of presentations were made by DOE staff on existing research programs relevant to the panel's interests. These presentations included a briefing on the activities of the Office of Energy Research related to fossil energy, to give the panel a comprehensive view of DOE research support in this area. With these presentations as background, the panel went on to discuss and list by priority promising research areas relevant to the development of non-coal fossil energy resources. In formulating its recommendations, the panel considered a number of potential scenarios for enhanced funding of such research.

This report contains the findings and recommendations that emerged from this process. It is written both to advise the research management of the Department of Energy on research opportunities and needs, and to stimulate interest and involvement in the research community in fundamental research related to fossil energy, and in particular, oil and gas resources.

I would like to take this opportunity to acknowledge in particular the intellectual contributions to this report by Dr. Aaron J. Seriff and Mr. Riley Needham, of the Shell Development Company and Phillips Petroleum Company, respectively. Their insights and generous efforts to advise the panel on specific topics in petroleum exploration and production were most valuable and much appreciated.

James F. Mathis
Chairman, Panel on Future
Directions in Fundamental
Research Related to Fossil
Energy

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EXECUTIVE SUMMARY

INTRODUCTION

Our nation stands at the crossroads with respect to fundamental and advanced exploratory research to facilitate the exploration, production, conversion, and processing of domestic oil, gas, shale, and tar sand resources. Assuring national security in energy supplies requires a vital research effort focused on reducing the risks and costs of looking for and producing petroleum from domestic resources, and providing better ways for using oil and gas from difficult-to-refine or remote resources. Yet the national committment to research and development in fossil energy has plunged remarkably in the last few years. With the decline of energy prices in the early 1980s, both private industry and the federal government substantially retrenched their research and development efforts. At the same time, energy demand has continued to grow, and the United States has moved to greater levels of dependence on foreign supplies of petroleum products.

Given these conditions, the panel believes that the Department of Energy should substantially increase its funding for advanced exploratory fossil energy research. Research success in this area could yield major payoffs in improving the efficiency with which oil is found and produced, or the efficiency of conversion and processing for oil and gas. These opportunities, which are briefly described below, are so provocative and potentially rewarding in the long term that increased support should be invested even in a time of significant budgetary stress for the federal government.

RESEARCH FRONTIERS

Exploration and Production

The research frontiers identified by the panel as being of highest priority naturally divide themselves into "upstream" research areas described in this section, and "downstream" research areas described in the following section on conversion and processing. Within each of these sections, topics are listed in rough order of their perceived importance by the panel.

Physical and Chemical Properties of Rocks

A better grasp of the fundamental physical and chemical properties of rock is needed if we are to understand the importance of variations in these properties in the exploration for and production of oil and gas. An order-of-magnitude increase is needed in the publicly available empirical data on both the mechanical properties of rocks (e.g., V_p/V_s, intrinsic velocity anisotropy, thermal conductivity, electrical conductivity) and those properties that limit the flow of fluids through porous rocks (e.g., permeability). This data base would allow significant progress to be made on problems such as the following:

- detailed reservoir modeling for enhanced oil recovery (EOR);
- fundamental geological modeling of the origin and development of sedimentary basins--including thermal and pressure history--as well as stratigraphy and fluid history; and
- the use of geophysical methods, particularly surface seismic reflection methods, to determine properties of buried sediments such as lithology, fluid content, and fracturing.

Predicting Reservoir Internal Structure and Heterogeneity

Local variations in texture, mineral composition, bedding, and other rock properties produce heterogeneities on both large and small scales within a reservoir. A detailed understanding of such heterogeneities is required to aid in the planning and implementation of reservoir exploitation for maximum oil recovery under a given set of economic constraints. An important advance would be achieved if we could predict large-scale variations of internal structure within a reservoir--as well as the magnitude, distribution, and type of expected smaller heterogeneities--on the basis of an interpretation of the depositional characteristics and subsequent history of the sedimentary unit that contains the reservoir. This predictive capability could be realized by an extensive field, laboratory, and library research program that developed quantitative models of the types of sedimentary units that commonly serve as petroleum reservoirs. With proper funding, such a comprehensive and ambitious program is within the reach of determined and highly interdisciplinary teams of researchers.

Dynamics of Dispersed Systems

Dispersed systems--where a solid, liquid, or gas phase is dispersed within a continuous liquid phase--are extremely common in the extraction, production, and transportation of fossil fuels. The behavior of these poorly understood systems cannot be predicted. Improved fundamental understanding of chemical behavior in dispersed systems could enhance our ability to recover a significant portion of the estimated 300 billion barrels of domestic oil still trapped underground and not available with today's enhanced oil recovery technology. New and more powerful tools are available to study systematically the behavior of dispersed systems. It is now possible to image the complex structures involved in dispersed systems and to examine their chemical behavior under a variety of conditions. These new opportunities should be vigorously exploited.

Seismic Determination of Lithology and Fluid Content

Advances in the acquisition and processing of seismic data are opening the way to new and powerful applications of seismic techniques to the determination of lithology and fluid content in subsurface sediments, including oil and gas reservoirs. In combination with more extensive basic data on rock properties, the sophisticated measurement and interpretation of seismic reflection data could lead to improvements in direct hydrocarbon detection, better quantitative estimation of amount and types of fluids present in a reservoir, and estimation of reservoir properties such as porosity and fracturing. Used more broadly, these same techniques could significantly enhance our understanding of the geology and geological history of the earth.

Novel Methods for Enhanced Oil Recovery

An estimated 300 billion barrels of domestic oil are still trapped underground in reservoirs. This oil cannot be recovered by conventional enhanced oil recovery (EOR) technologies; known improvements and projected refinements of these conventional techniques are not expected to make economical the production of more than a very small fraction of this untapped resource. Fresh, unconventional ideas are needed. This problem is an important target for fundamental research.

One promising line of research, the study of dynamics in dispersed systems, has already been identified and discussed (see above). Research that provides a better understanding of reservoirs and of fluid-rock interactions (see above) may also suggest novel ideas for EOR. While this area should be considered an ongoing companion of these efforts, still other fresh approaches are needed. A source of funds should be created to stimulate fundamental research in pursuit of new insights into the problem of EOR.

Improvement of Seismic Resolution

High-frequency sparker and shallow-penetrating seismic systems are currently used to obtain data to depths of several hundred meters. Research on high-resolution seismology, using frequencies double to triple those currently used in petroleum exploration seismology, could lead to doubling or tripling of the resolution capabilities of seismic methods. Current results at depths of several hundred meters offer encouragement for success at considerably greater depths. Such advances would bring a valuable new tool to the problems of fossil resource location and characterization.

Borehole Measurements

Successful geoscience research described in a number of the previous topics (e.g., physical and chemical properties of rocks, prediction of reservoir internal structure and heterogeneity, seismic lithology studies) would be facilitated by the development of new tools to measure rock properties and their variation in lateral distances on the scale of reservoir dimensions. Improvements are needed in the ability to obtain and interpret data collected or measured within a borehole--whether these improvements result from advances in various logging methods, from the development of entirely new logs, or from the sophisticated use of measurements made with sources and sensors located in adjacent boreholes, or with sources and sensors distributed between a borehole and the surface.

Developing Sensing Methods for Monitoring EOR Processes In Situ

The successful operation of enhanced oil recovery methods is limited by the lack of methods to monitor the advancement of the injected fluids or thermal fronts in the interwell spaces. If one could determine the detailed location of these fluids and the displacement of oil in an EOR process, one could adjust and possibly optimize the process. While research undertaken in support of other topics on this list (e.g., seismic determination of fluid content and improved borehole measurements) could be used to monitor EOR processes in situ, additional efforts focused specifically on this problem are needed.

Detecting and Monitoring Corrosion, and Developing Lifetime Prediction Methods for Materials Susceptible to Corrosion

Corrosion is an important problem, both in the exploration and production of oil and gas, and in the eventual transportation, conversion, and processing of these fossil fuels. Traditional approaches to corrosion control have been narrowly focused and of limited success. A generic approach to detecting and monitoring corrosion phenomena, undergirded by a strong laboratory simulation effort, could provide significant improvements in our ability to understand the basic phenomena involved, monitor them, and develop methods of predicting failure probabilities for materials and systems undergoing corrosion.

Conversion and Processing

Characterizing Feedstock Structure and Structure-Property Relationships

The processing of hydrocarbon feedstocks (e.g., petroleum, oil from tar sands, and kerogen from shale) is strongly dependent on the molecular structure of the feedstock. Little headway can be made on important needs--such as improved processing for kerogen, tars, or asphaltenes--until a better understanding of their molecular structure is achieved. Only then will it be possible to make revolutionary advances, based on new reactions of the molecules in these feedstocks.

It is essential, then, to carry out research to establish feedstock structure at the molecular level and the relationship between molecular structure and physical and chemical properties. Traditional methods of feedstock analysis, which have served well for the routine characterization of petroleum, must give way to analytical and structural methods that will provide a more detailed understanding of feedstock structure. Research is also needed on the relationship between feedstock structure and reactivity. Such relationships should be sought through studies involving thermal and catalytic reactions involving compounds, oligomers, and polymers that are thought to be representative of the substances present in feedstocks. The identification of weak structural links for cleavage, or better methods of eliminating heteroatoms and organically bound metals, are important research issues.

Catalysis

Continued improvements in catalysis are vital to maintaining the productivity and economic competitiveness of the petroleum and gas processing industries. Of particular importance is the development of more efficient catalytic processes for converting (often heavy) crude oil to gasoline and to the petroleum fractions used in the manufacture of petrochemicals. Longer catalyst life in processes for hydrodesulfurization, hydrodenitrogenation, and hydrodemetallization will be facilitated by knowledge of how metals accelerate coking, how they deactivate catalysts, and how metals deposited on catalysts can be passivated. Fundamental information on factors governing hydrogen utilization and economy in hydrodesulfurization, hydrodeoxygenation, and hydrodenitrogenation would also be very valuable.

Chemical Processing of Methane

Supplies of natural gas (a principal constituent of which is methane) are remaining abundant, and remote gas is either reinjected into the well or flared (burned) because there is no economic way of using it. Capturing this huge amount of energy in liquid form could significantly extend liquid fuels supplies worldwide.

The principal technical need that must be overcome to achieve this goal is research to facilitate the discovery of selective, economically viable methods to convert methane to liquids by catalytic routes. Better catalytic and kinetic understanding is required to carefully control selectivity toward oxidation, in order to avoid further oxidation of any methanol product. Selectivity might also be controlled by methods to separate products from reactants quickly. Another route to selectivity might be to study biological organisms responsible for methane oxidation, to determine the mechanisms by which they operate. This knowledge could ultimately be used to design a catalytic system.

A second problem, important for those sources of gas that are contaminated by substantial amounts of hydrogen sulfide or carbon dioxide, is the need for more economic methods to selectively remove these contaminants from natural gas feedstocks. Current gas-cleaning technology for methane relies on wet scrubbing methods, which entail high costs for construction and operation and are hard to make economic for remote gas.

Materials

The science and engineering of materials is a research area of critical importance in fossil fuel production and conversion because these fuels are produced and processed in unusually harsh and corrosive environments. The following factors are directly dependent on material selection and capability: on-stream time in processing plants, lowered failure rates in equipment for exploration and production, minimization of undesirable contamination of process streams and final products, and construction of reaction vessels capable of withstanding elevated temperatures, pressures, and reactive or corrosive charges. In the last 10 years, academic researchers have made great strides toward ceramic materials with substantially improved structural capabilities, including increased toughness and better ability to withstand high temperatures. The investigation of ceramic and composite materials in environments marked by severe chemical and thermal stress, the investigation of thermally rugged plastic and ceramic coatings, and the development of high-temperature, tough, structural ceramics and economic forming processes that might be used in manufacturing important process equipment all deserve high priority.

Instrumentation Development

It is essential to pursue further research on the development of instruments and methods for application to problems of petroleum processing. One area that would have a significant impact on the economic vitality of the U.S. oil and gas industry would be the development of miniaturized instruments for on-line analysis of process streams. While gas chromotography is now in use on-line, other instrumentation, such as Fourier transform infrared (FTIR) spectroscopy and nuclear magentic resonance (NMR) spectrometry, are still confined to a laboratory environment. Research and development is needed to provide small rugged sensors and microcomputers capable of operation at high temperatures and pressures, and in the presence of corrosive gases or liquids. A second research area that might make a contribution is the use of x-ray and NMR imaging. The application of imaging techniques to the characterization of kerogen in shale and oil in core samples is expected to provide an unprecedented level of information about the structure of the organic matter in its original environment. Information from such studies will contribute to an understanding of the content of organic material and could suggest sound ways for extracting it.

Extraction of Oil from Shale and Tar Sands

A major drawback to the utilization of oil shale and tar sands is the development of efficient and cost-effective means for separating the organic matter in these resources from the inorganic matrix in which it is found. In the case of shale, the organic material, called kerogen, is a three-dimensional polymeric material that must be broken down to oil. Current approaches involve pyrolysis of the shale rock, which is energy inefficient since one must heat and cool about 10 kg of rock in order to get about 1 kg of oil. Solids handling in the process also becomes a major consideration, and some size reduction in the shale particles being processed is generally carried out. For both shale and tar sands, an understanding of the factors controlling the absorption of oil and kerogen on the host matrix could contribute to the suggestion of better ways for disengaging these organic substances from the matrix.

Alternative Transportation Fuels

Two-thirds of current annual consumption of petroleum is in the transportation sector. In the event of a sudden curtailment of imports, there are relatively few opportunities for this sector to switch to alternative fuels. A long-term effort to increase the technological options for other transportation fuels could significantly benefit national energy security. The range of possible alternative fuels is very broad and includes such candidates as alcohol-based fuels and fuel blends containing alcohols such as methanol and ethanol, new fuels with hydrogen-carbon atomic ratios approximating modern gasoline, new fuels with hydrogen-carbon ratios approximating today's diesel fuels, and fuels with low hydrogen-carbon ratios.

Although end-use oriented, research on alternative fuels should be considered appropriate for federal funding because of the intrinsic long-range nature of any scenario involving fuels with markedly different hydrogen-carbon ratios than those in use today. Given the formidable institutional barriers to the introduction of new fuels and engines, significant attention to alternative fuels research from industry is only likely to occur when independent, high-risk research can demonstrate feasibility.

The panel believes that the APT program is well placed to stimulate fundamental research on the most significant scientific and technological questions. For example, combustion systems that can effectively utilize fuels of less than 12 to 13 weight-percent hydrogen (equivalent to a H/C atomic ratio of about 1.6) should be a high priority target, since such engines would facilitate the introduction of substitute fuels derived from minimally processed petroleum fractions and lower quality shale oil and coal liquids. More research is needed in this area. An appropriate APT thrust to address this need might be to concentrate on combustion properties and science of low-hydrogen fuels.

Another appropriate activity for the APT program would be to commission a study of the transportation fuel implications of automotive- oriented ceramic programs in the United States and abroad that may give rise to higher temperature engines.

Other Research Frontiers

There are two areas of current interest in the APT program, environmental research and arctic research, where the panel judged potential research thrusts to be of lesser priority than those recommended above.

Several projects in the current APT program deal with environmental matters. Although environmental aspects of fossil fuel production, processing, and use are very important topics for research, many of these aspects are already being addressed well in other programs--both within DOE and in other agencies--that have more funding available to them and a specific mission in environmental science and technology. Given the other high-priority areas in this report that are appropriate only for the APT program, the panel feels that the related environmental issues could perhaps be dealt with better in these other, more targeted, programs.

The Arctic and Offshore Research (AOR) program is currently in a pattern of decreasing support from year to year, and a major part of the program (the AOR Information System) is scheduled to be completed and become self-supporting in the next year or two. As existing AOR projects come to their scheduled completion, the Office of Technical Coordination should direct these funds to the support of topics perceived by this panel to have higher priority.

RECOMMENDED LEVEL OF EFFORT AND SUPPORT

The present Advanced Process Technology Program in the Office of Fossil Energy is budgeted in the FY 1988 Administration Request at approximately $2 million. Its average size in recent years has been closer to $5 million. This panel has concluded that, to be effective, this vital program should support the high-priority research topics outlined in this report at a minimum level of $10 million. A budget totaling $20 million, with $10 million allocated for the geoscience-related topics presented in this report and $10 million devoted to the other topics, can be easily justified. The panel's recommendations for supporting high-priority fundamental research through the APT program, under a number of potential budget scenarios, are summarized in Table 1-1 and are described briefly below.

Scenario 1: Minimum Effective Level of Support

The panel estimates that the minimum effective level of effort needed to pursue the areas outlined in this report is equivalent to a budget level of about $10 million per year. This budget level is about twice the average funding level for the APT program in recent years. In the panel's view, increasing the total budget for the APT program from its traditional level to about $10 million represents an increment in support that has a realistic chance of success in the budget process.

Even at this minimum effective level, the depth of treatment possible for a number of high-priority research topics would not be ideal. Several of these topics encompass a number of subareas (for example, three subareas are identified in this report under Extraction of Oil from Shale and Tar Sands). An APT program with a budget total of $10 million could probably build a critical mass of projects in only one of several possible subareas for these research topics. In other words, the research frontiers outlined in this report are sufficiently promising that they could be the basis of a high-quality program with a multiyear pattern of budgetary growth beyond the $10 million level.

Scenario 2: Enhanced Support for Geoscience Research Frontiers

The Energy Research Advisory Board (ERAB), which reports to the Secretary of Energy on research matters, has recently issued a report entitled Geoscience Research for Energy Security.[1] The ERAB report strongly supports additional emphasis on a number of research frontiers in the geosciences, several of which have been identified independently by this panel in Chapter 3. In the event that the ERAB report becomes the basis for a special initiative to fund geosciences research in DOE, this panel recommends that the APT program receive sufficient funding to pursue the research opportunities described in Chapter 3 at a level well above the minimum target proposed in the preceding section. The high-priority research frontiers related to the geosciences that have been described for exploration, extraction, and production are sufficiently promising that a high-quality research program could easily require a budget of $10 million for these topics alone.

Funding of these topics by the APT program under a departmentwide geosciences initiative could also have the benefit of allowing enhanced support of other high-priority topics described in this report. In such a case, the panel recommends that the non-geoscience topics be funded at a level of $10 million, thus yielding an APT program with a total budget of $20 million.

Scenario 3: Funding at the Average Recent Budget Level

In making its recommendations for topics that might be supported at a budget level near $5 million (see column B of Table 1-1), the panel adopted three criteria:

- Scientific and technical importance of the research topic;
- Lack of support or grossly insufficient support from other sources, both private and public; and
- Prospects for a success that would allow development to proceed in an intermediate timeframe.

The topics that would be recommended for funding under this scenario would be as follows:

- Physical and chemical properties of rocks;

- Predicting reservoir internal structure and heterogeneity;
- Dynamics of dispersed systems;
- Novel methods for enhanced oil recovery;
- Seismic methods for determining lithology and fluid content;
- Characterization of feedstock structure and structure-property relationships; and
- Chemical processing of methane.

An APT program funded at merely the average budget it received before its reorganization and redirection in late 1986 seems not to take advantage of the opportunities afforded by such a change. While such a scenario is certainly better than the worst-case scenario described in the following section, most of the exciting and potentially rewarding research frontiers identified in this report would be left unsupported, including some high-risk research opportunities with a high potential payoff if successful. Also unsupported would be a number of opportunities in conversion and processing where research advances could provide synergism to coal research supported by the existing Advanced Research and Technology Development Program.

Scenario 4: Funding at the Level in the FY 1988 Budget Request

At the funding level requested in the President's FY 1988 budget, little progress can be made on the critical research needs spelled out in this report. The recommendation by the panel in this worst-case scenario would be simply to maintain those research projects in the APT program that currently address the highest priority needs identified in this report (see column C in Table 1-1). This is a different philosophy than that expressed in the list of topics given under the three preceding scenarios. The recommended projects would be in the following areas:

- Predicting reservoir structure and heterogeneity;
- Characterization of feedstock structure and structure-property relationships; and
- Instrumentation development.

The combined current budgets for these projects are approximately at the FY 1988 requested level. It must be emphasized, though, that this level of effort is far short of meeting the national need for fundamental research related to oil and gas resources. This worst-case scenario would require the APT program to forego funding most of the research areas that hold great promise for significant advances.

CONCLUSION

Fundamental research to facilitate the efficient detection, recovery, and processing of domestic oil and gas resources is of critical importance to a national strategy of preparedness for a future of uncertain supplies and prices for liquid fossil fuels. A program structured along the high- priority research frontiers identified in this report promises significant benefits to national security and economic competitiveness. In a political environment dominated by fiscal and budgetary constraints, it is perhaps tempting to think of postponing investments in energy research with a long-term payoff. Such a policy would be extremely shortsighted, as it would leave the United States vulnerable to disruptions in supplies and prices for fossil fuels. The opportunities for fundamental research related to oil and gas resources are intellectually tantalizing and remarkably diverse. A timely response is required to address these opportunities, so that the nation will be able to face its long-term energy future with confidence.

TABLE 1-1 Panel's Recommendations for APT Program Funding Under Three Possible Scenarios

Program Area	Funding Recommendations[a]		
	A	B	C
Exploration and Production			
Physical and chemical properties of rocks	X	X	
Predicting reservoir internal structure	X	X	X
Dynamics of dispersed systems	X	X	
Determining lithology and fluid content	X	X	
Novel methods for EOR	X	X	
Monitoring EOR in situ	X		
Improving seismic resolution	X		
Borehole measurements	X		
Detecting and monitoring corrosion	X		
Conversion and Processing			
Characterizing feedstock structure	X	X	X
Chemical processing of methane	X	X	
Catalysis	X		
Materials	X		
Instrumentation development	X		X
Extraction of oil from shale and tar sands	X		
Alternative transportation fuels	X		
TOTAL FUNDING (millions of dollars)	10	5	2

[a] Column A summarizes those areas recommended for funding in order to achieve the minimum effective program. Column B shows those areas that could be funded if the APT program budget does not grow beyond its average level over the past few years. Column C shows which recommended topics are already part of the APT program. Research in these areas should be maintained if the current Administration proposal to halve the APT budget in FY 1988 is accepted. A fourth scenario, involving enhanced support for the geosciences at DOE, is discussed in the text.

2

INTRODUCTION

Advanced exploratory research--research that is intermediate between basic scientific research and applied, developmental activities--is critically needed to facilitate the exploration, production, conversion, and processing of oil, gas, shale, and tar sand resources in the United States. America's dependence on foreign sources of oil is growing dramatically. Low and uncertain prices for fossil fuels are driving out marginal and small-scale petroleum production from domestic sources, and are curtailing the search for new, potentially exploitable resources of oil and gas. Assuring national security in energy supplies requires a research policy focused on reducing the risks and costs of finding new resources, producing petroleum economically from both old and new domestic resources, and providing better ways for using oil and gas from difficult-to-refine or remote resources.

Why should the federal government, and the Department of Energy (DOE) in particular, take a role in supporting such research? While the DOE has traditionally supported basic energy research and has a program of considerable size supporting fundamental research related to coal, recent cutbacks by the petroleum industry in their fundamental and advanced exploratory research makes it timely to investigate ways in which the department can ensure that such cutbacks do not jeopardize our nation's ability to respond to future uncertainty in the price and availability of oil and gas. There is a paucity of support for a number of research areas where success could yield major payoffs in improving the efficiency with which oil is found and produced, or the efficiency with which oil and gas are converted and utilized. These opportunities, which are listed and described in the next two chapters, are so provocative and potentially rewarding in the long term that the federal government should substantially increase its support to achieve a level commensurate with the opportunities. This latter point is explored in more detail in Chapter 6 of this report.

3

EXPLORATION AND PRODUCTION: RESEARCH FRONTIERS

INTRODUCTION

The research opportunities identified by the panel as being of highest priority naturally divide themselves into "upstream" research areas (exploration and production of fossil energy resources) and "downstream" research areas (conversion and processing). These research frontiers are described in this chapter and the next. Within each chapter, topics are listed in a very rough rank order of perceived importance.

The description of each topic is divided into three parts: an introduction to the topic and its importance, an exposition of scientific and technical challenges related to the topic, and a description of the proposed research thrust that the panel feels would best address the key research needs and opportunities. Some topics in this chapter overlap with APT or other DOE programmatic interests. Where the APT is already funding research that matches well with the panel's highest priorities, or where the panel sees an opportunity for synergism between research that would be undertaken in pursuit of a particular topic and other DOE- or industry-sponsored research, this is noted in a special final section under each topic.

PHYSICAL AND CHEMICAL PROPERTIES OF ROCKS

Introduction

A significant increase in our knowledge of the chemical and physical properties of rocks, the fluids they contain, and the geological processes that form them and alter them (e.g., deposition, burial, diagenesis) is required in order to make progress on problems such as the following:

- detailed reservoir modeling for enhanced oil recovery (EOR);
- fundamental geological modeling of the origin and development of sedimentary basins--including thermal, pressure, and fluid history--as well as stratigraphy; and
- the use of geophysical methods, particularly surface seismic

reflection methods, to determine properties of buried sediments such as lithology, fluid content, and fracturing.

Scientific and Technical Challenges

The chief scientific challenge is to increase, by an order of magnitude, the publicly available empirical data on both the mechanical properties of rocks (e.g., V_p/V_s, intrinsic velocity anisotropy, thermal conductivity, electrical conductivity) and those properties that limit the flow of fluids through porous rocks (e.g., permeability). These properties need to be related to other rock data such as lithology, porosity, depth of burial, and diagenetic history.

Though significant amounts of data have been taken on a number of sandstones, limestones, and dolomites, these data are inadequate to provide a sound basis for attacking problems such as those mentioned in the introduction. The data are also scattered among a number of disparate, usually proprietary, compilations.

The data base on the physical and chemical properties of shales is even poorer. Shales are important components of hydrocarbon reservoirs. They are a source of fluids and elements that play a significant role in post-depositional (diagenetic) processes. They are also important structural elements in reservoir seals and intrareservoir barriers to fluid flow. Yet most of the existing studies of reservoir rocks have provided little information on the shales. Knowledge of shale properties is essential in interpreting reflection amplitudes and in constructing geological models of sedimentary basin development. Our nearly complete ignorance of shale properties is typified by the absence of reliable data on their intrinsic velocity anisotropy. This property plays a vital role in the accurate determination of seismic velocities from surface data. A fair number of measurements of the ratio of velocities perpendicular and parallel to bedding exist. Unfortunately, this information does not allow the prediction of the C_{13} elastic constant that controls the usual seismic estimates of velocity, and almost no data of sufficient accuracy is available.

The rock properties data base that is required should be built both from laboratory work--where detailed physical descriptions and measurements of the rocks can be obtained, both under confining loads similar to those found in situ and at atmospheric conditions--and from measurements made within boreholes using wire-line logs. Improvements in interpreting lithology from well data would certainly be desirable if these data are to be used with confidence. For instance, in-situ measurements of detailed mineralogy, lateral inhomogeneity, permeability, microporosity, shear velocity, fracturing, and stress are often extremely difficult to obtain, completely unavailable, or simply unreliable. The need for additional research in these areas is discussed later in this chapter.

A better grasp of the fundamental physics and chemistry of rocks and their contained fluids would help researchers to make better predictions from limited but representative empirical data bases. In a number of cases, quantitative predictions from first principles and good models should be possible. Indeed, the development and testing of such models should be of primary importance. There has been a considerable number of publications describing microscopic models for computing both the mechanical properties of porous granular and igneous rocks and the movement of fluids through the pore system of these rocks, but none of these models has been adequately tested against a broad spectrum of experimental data. Moreover, it is not clear that any of the models will simultaneously predict the mechanical and fluid-flow properties with a small set of free parameters. The state of models for shale properties is in an even more preliminary state; the study of shale models represents an exciting research opportunity where fundamental research advances would be of great practical importance.

A better understanding of the geochemistry of rocks might result by integrating new insights from the catalytic chemistry of aluminosilicates into the modeling of resource generation in these rocks.

Proposed Research Thrust

Current research in the fundamental physics and chemistry of rocks is being carried out by both industrial and academic investigators. Despite the fact that nearly every new piece of reliable data finds some immediate application, this research area should be considered as fundamental research, because the ultimate scope and impact of applications of research in this area is almost impossible to predict. Thus, this area would be appropriately addressed by the APT program.

There is a strong interaction between theoreticians and physical scientists involved in laboratory measurements, but less apparent interaction between these researchers and geologists. An important goal of APT involvement in this area should be to enhance the participation of geologists in interdisciplinary research on rock properties. There are two benefits that would accrue from such improved participation by geologists: the proper use of log measurements would extend the data base on rock properties dramatically, and participation by geologists would also facilitate the study of the variation of rock properties with geological environment and history.

There are enough unsolved problems of critical importance in this area that a research thrust could sustain program excellence for several years, without fear of exhausting important problems. Because some industrial consortia are involved in funding of work at some academic institutions and private laboratories, a new APT program in this area should emphasize (1) coordination with existing efforts, (2) promotion of interaction between geologists and geophysicists, and (3) strong encouragement for joint funding and publication of results by industry.

Opportunities for Synergism with Other DOE Programs

The Geosciences Research Program in the Office of Energy Research contains a subcategory entitled "Properties of Earth Materials" that funds a few projects whose descriptions resemble the work that this research thrust seeks to stimulate. At the same time, the majority of the projects indexed under this subcategory (exemple topics include deep electromagnetic sounding and properties of minerals found in the mantle) do not seem to be within the boundaries of the research topic described in this section. The panel concludes that the activity in the Office of Energy Research is at a sufficiently low level that it represents more of an opportunity for synergism and contact among research groups that might be funded by both programs, than it does an overlapping or duplicative program.

PREDICTING RESERVOIR INTERNAL STRUCTURE AND HETEROGENEITY

Introduction

Most oil and gas reservoirs, whether they are made up predominantly of sandstone rocks or carbonate rocks, are nonhomogeneous and anisotropic in the properties that control fluid flow. These reservoir heterogeneities must either be accurately predicted or be specifically determined in order to promote efficient recovery of the maximum possible amount of oil. For example, the determination of significant large-scale heterogeneities in a reservoir can be used to identify the proper location for infill wells targeted to recover bypassed oil, or to design treatments for controlling the mobility of oil in enhanced recovery projects. Geologists have been aware of the variations in certain rock properties (for example, cementation and grain size) that affect porosity and permeability within a reservoir since the 1950s, and in a more rudimentary way for several prior decades. Reservoir simulation models designed during the 1960s were able to incorporate the effects of large scale heterogeneities. During the mid-1970s research commenced at several universities and major oil companies to specifically determine the influence on reservoir heterogeneity on reservoir performance. Significant benefit would be gained, though, if the prediction of heterogeneities in reservoirs could be placed on a more fundamental basis.

Scientific and Technical Challenges

Reservoir heterogeneity must be described on three scales. At the microscopic scale (micrometers to millimeters), one must determine the types and textures of clay minerals and other small particles that partially fill pore spaces or coat the framework grains. At the macroscopic scale (millimeters to meters), one must examine variations in the reservoir that result from sedimentary structures such as ripple marks, crossbedding, and thin layers (laminae) that are responsible for changes in texture within a genetically related sedimentary unit. At the large scale (10 meters to hundreds of meters), one must develop methods for determining quantitatively the effects of large-scale heterogeneity on multiphase fluid flow. Moreover, these methods must be developed in such a manner that they can be incorporated into digital reservoir simulation models.

An important objective of scientific research in understanding the internal structure of a reservoir is to develop an ability to predict large-scale variations, as well as the magnitude and spatial distribution of expected microscale and macroscale heterogeneities, on the basis of an interpretation of the origin and subsequent history of the sedimentary unit. Important factors in such an analysis would be a determination of the dynamic flow regime that was responsible for the deposition of a detrital reservoir, or the variations expected in a biogenic unit deposited under certain environmental conditions.

The interpretation of specific depositional environments in which sedimentary units were laid down can be made from general considerations--such as the regional geological setting of the formation in which the reservoir zone occurs--as well as from more limited, specific data obtained from cores, samples, and a variety of well logs. Once a reservoir unit has been classified according to the type of dynamic flow regime or biogenetic setting responsible for its deposition, statistical predictions can be made regarding the scale and type of heterogeneities to be expected. The validity of such predictions can be greatly affected by the influence of post-depositional (diagenetic) changes of the cementing material and other reservoir properties. Interpreting the extent and influence of the "diagenetic imprint" on reservoir properties requires a variety of physical geochemical analyses.

Proposed Research Thrust

A comprehensive program in understanding and characterizing internal reservoir structure will require extensive field, laboratory, and library research to develop quantitative depositional models of many of the specific types of sedimentary units that commonly serve as petroleum reservoirs. A number of methodological improvements will be required to obtain the data necessary to classify reservoirs genetically and to improve our ability to extrapolate and interpolate the physical properties influencing fluid flow. These improvements will include better methods of interpreting well logs and improved surface imaging techniques. Also needed will be the development of computer-assisted classification methods based on the comparison of a limited set of observed properties with the full set of properties of many depositional models.

Not surprisingly, this type of research will be highly interdisciplinary, requiring close cooperation and integration of results from sedimentologists, paleontologists, stratigraphers, computer-oriented statistical geologists, geophysicists, geochemists, and reservoir engineers developing or using reservoir flow-model simulators. Cooperation between academic and industrial researchers is essential for success; industrial partners could help DOE leverage its funds considerably by providing their academic colleagues access to core semples and data that may be of less interest from a proprietary standpoint than from the standpoint of studying sedimentary units.

This is an extremely comprehensive and ambitious fundamental research program. A typical research project, seeking to classify and characterize reservoir structure in a specific type of sedimentary unit, might require a minimum of 1 to 2 years to begin to produce useful results. Comprehensive results from this research thrust, to be gained after the analysis of a number of sedimentary unit types, would require a minimum of 4 years, depending on the success with which funds could be leveraged among several projects. Stable funding of at least $1 million per year over a multiyear period is essential.

Overlap With Current APT Research

The Advanced Process Technology (APT) program currently provides partial support to a number of reservoir characterization projects, including one at the National Institute of Petroleum and Energy Research (NIPER) that resembles the sort of comprehensive project envisioned by the panel. Since the goal is to understand and characterize the structure of a variety of common types of sedimentary units, this is an area where more projects of the same type, focusing on different kinds of sedimentary units, are needed. A number of research groups working in parallel, whether at NIPER or in universities and industry, would also benefit from the opportunity for cross-fertilization of techniques and insights among groups.

DYNAMICS OF DISPERSED SYSTEMS

Introduction

Dispersed systems--with a liquid as the continuous phase and a solid, a liquid, or a gas as the dispersed phase--are extremely common in the extraction, production, and transportation of fossil fuels. Very little is known about transport of compounds from one phase to another within these systems, and even gross properties of dispersed systems, such as rheology, are poorly understood. It is not surprising, then, that the behavior of dispersed systems, particularly in underground environments, cannot be predicted. The following examples illustrate this fact.

- A microemulsion of the type used in enhanced oil recovery may form a stiff gel at the microemulsion-water interface, instead of rapidly accepting more water into its structure. The resulting gel may delay the attainment of equilibrium between the microemulsion and the water phase for years.
- A foaming system for gas-driven oil recovery may suddenly cease to foam because an inversion of the emulsion system from oil-water to water-oil has taken place.

- Transportation of heavy oils in pipelines in the form of an oil-water emulsion is not a realistic proposal because an inversion of the emulsion would have catastrophic consequences.

Improved fundamental understanding of chemical behavior in dispersed systems could enhance our ability to recover a significant portion of the estimated 300 billion barrels of domestic oil still trapped underground and not available with today's EOR technology, would enable heavy oils to be shipped great distances in pipelines, and would make available simple new chemical extraction methods with potential for spin-off into technologies as diverse as pharmaceutical production and nuclear waste segregation.

Present research in this area is concentrated on equilibrium systems, which in themselves are important and deserve more high quality contributions. Academic research groups undertaking significant investigations in equilibrium thermodynamics can be found at a number of institutions, and researchers at NIPER are making outstanding contributions in this area. Several other groups are analyzing the structure of micellar and microemulsion systems at equilibrium using neutron-scattering methods. Industrial research related to structural determination is extensive; some industrial laboratories are making excellent theoretical contributions and several have large groups engaged in microemulsion recovery.

Most of this research is targeted at understanding dispersed systems at equilibrium conditions. The dynamic behavior of these systems has attracted considerably less interest, and represents fertile ground for targeted research support.

Scientific and Technical Challenges

The scientific challenge is to relate the change of phase regions--the increment of the interface--to the relative diffusivities and interfacial resistances of the individual chemical components in a typical multiphase system. This might appear to be a rather formidable task, taking into consideration the variety of structures that exists in systems of this kind during practical applications. For any dispersed system, one finds a multitude of different amphiphilic association structures such as premicellar aggregates, normal and reverse micelles of various structure, bicontinuous colloidal structures, vesicles and larger aggregates of dispersed liquid crystals, all in different stages of the process or reaching equilibrium. These structures control the diffusion rates of their components, one factor in the transport of a component from one phase to another--a process whose rate determining step is the drive for reorganization of the association structure at the interface.

With unknown entities this task would involve a huge scientific effort, and using the experimental technologies of 5 to 10 years ago, the practical obstacles to investigating this problem experimentally would be insurmountable. Today, such is not the case. The development of NMR spin-echo field gradient methods have enabled researchers to obtain information on the diffusion coefficients for all components in a dispersed system in less than an hour. A combination of investigations should be undertaken, including (1) the use of scattering methods and a variety of imaging methods to elucidate the structures involved in dispersed systems, (2) the determination of diffusion coefficients for individual compounds within in these structures, and (3) the performance of conventional diffusion experiments on carefully selected systems to clarify interfacial resistance and the influence of back-diffusion on the total transport of substances in a system. These combined experimental efforts should make it possible to discover the rules governing spontaneous emulsification of heavy oils in non-stirred reactors, and to relate the behavior of a system containing crude oil, a microemulsion, water, and electrolytes in a way that would be useful in direct application to problems in the recovery and transportation of oil.

Proposed Research Thrust

Present research in the area of multiphase system behavior is limited to just a few institutions in the United States, and training, where it exists, is rudimentary. The APT program has the opportunity to make a major contribution to breakthroughs in this area by focusing support on dynamics of behavior within dispersed systems. One way in which it could do this is by developing a program of targeted support for fundamental research in this area. Such support should be carefully designed to promote communication and joint efforts between researchers in universities, industry, and national laboratories. A complementary step could be to take advantage of the expertise that exists at NIPER in equilibrium thermodynamics by encouraging their researchers to explore research topics in the dynamics of dispersed systems.

SEISMIC DETERMINATION OF LITHOLOGY AND FLUID CONTENT

Introduction

Advances in the acquisition and processing of seismic data are opening the way to new frontiers in the application of seismic techniques to determination of lithology and fluid content in oil and gas reservoirs and the surrounding rocks. For example, lateral variation in stacked seismic P-wave reflection amplitude has become virtually a routine diagnostic technique for use as a qualitative indicator of the presence of gas in a reservoir. The comparison of P-wave and S-wave reflection amplitude is increasingly used to eliminate some ambiguity in conclusions based on P-wave amplitudes alone. More recently, both academic and industrial seismologists have reported that diagnoses based on P-wave and S-wave reflection amplitude comparisons can be further confirmed by use of the variation of P-wave reflection emplitudes with source-receiver offset.

In some cases, the use of these measurements along with a knowledge of the physical properties of certain rocks (e.g., V_p, V_s, attenuation) may permit the recognition of the type of rocks involved in a given reflection, as well as other physical properties such as the presence of fracturing. In addition, widespread observation of both polar and azimuthal seismic velocity anisotropy suggests the possibility of using these characteristics for seismic remote sensing of lithology, fracturing, and stress distributions.

Clearly, successful development of our ability to use these procedures could lead to significant advances in understanding the geology and geological history of the earth. Improvements in direct hydrocarbon detection and more quantitative estimation of the fluids present in the rocks would also be facilitated.

Scientific and Technical Challenges

The research challenges and opportunities here involve two important areas. One of these, developing an extensive data base on the physical properties of various rock types, has already been described in detail in the beginning of this chapter. The other important area is the development of techniques for accurately measuring and interpreting not only the lateral variation of stacked P- and S-wave amplitudes, but also the variations of amplitude with source-receiver offset on either single traces or appropriate mixes of traces. These measurements must be made with considerable vertical resolution. The development of methods to provide absolute reflection amplitude calibration could be most important. At least, the correction of various attenuation effects should be considered so that vertical amplitude comparison could be made for reflections separated by fairly large time intervals.

Though many aspects of research in this area are currently under intensive study by geophysicists, the panel highly recommends that an effort be made to combine all of the elements mentioned above--including absolute reflection calibration, which is probably not receiving careful study at the present time. There are many outstanding research problems in each of the steps involved. One example problem is the search for ways to reduce both noise and multiples that appear on single traces that are to be used in offset dependence studies. New ideas are needed. Moreover, the combination of all these measurements at particular sites, and subsequent comparison of the joint predictions to detailed VSP studies, will be needed to help establish the utility and reliability of these new combined methods.

Proposed Research Thrust

The breadth and scope of the problems at the center of this research thrust require the participation of seismologists with skills in data acquisition and processing, as well as geologists interested in sedimentary stratigraphy and research geoscientists familiar with the physical properties of rocks and the interpretation of logs.

This topic might be an ideal subject for individual or cooperative projects involving researchers at several different organizations, both academic and industrial. Significant results might be obtained by funding a suite of coordinated projects.

NOVEL METHODS FOR ENHANCED OIL RECOVERY

Introduction

The United States will have more than 300 billion barrels of oil remaining in known reservoirs after currently known EOR processes are exhausted. According to a 1984 study by the National Petroleum Council,[2] implementing currently known EOR processes and advancing these processes with already identified improvements could lead to increased oil reserves of 28 billion barrels, assuming that the price for oil is $30 per barrel (in 1983 constant dollars). Given current oil prices, probably less than one-quarter of the 28 billion barrels of "increased" reserves could be realized economically.

The DOE, through its line programs in the Office of Fossil Energy, already funds some work related to the characterization of reservoirs, the impact of heterogeneities on fluid movement, and microbial EOR. This work is targeted at tapping the resource represented by the 300 billion barrels in known reservoirs. Yet most of this effort, along with industrial research and much of the relevant academic work, are not directly aimed at developing entirely new oil recovery methods. Virtually all ongoing work has as its goal the adaptation of current oil recovery processes to specific reservoirs and the improvement of those processes by correcting well established deficiencies in each process.

Scientific and Technical Challenges

The fundamental scientific and technical challenge in enhanced oil recovery is the identification of methods to mobilize oil trapped by capillary forces and to displace this mobile oil to production wells. It is possible that, with the incentive of additional funding, researchers in and out of the field of EOR research might be stimulated to explore completely new schemes for accomplishing the mobilization and displacement of oil in reservoirs. EOR research needs a home for unconventional thinking and novel approaches. By definition any research carried out under this topic should be very high-risk and long-term in nature.

Some of these novel approaches may emerge from research supported under other high-priority research thrusts recommended in this report (e.g., dynamics of dispersed systems, fluid-rock interactions, and understanding and predicting reservoir internal structure). Others may come from other areas of science, including fundamental research to elucidate oil-water-rock interaction and wetting. For example, it now appears that polar compounds in oil may sorb onto rock in some cases, creating an oil-wetted oil-water-rock system instead of the classical picture of a water-wetted system. This insight might lead to different approaches to the problem of oil mobilization.

Proposed Program Thrust

A proposed program in this area should consider any good novel idea; thus, competition for funds should be widely advertised and open to industry, universities, federal laboratories, or any combination thereof. Such a program should be given a 3- to 5-year trial commitment at a level of at least $1 million per year. Successful research efforts emerging from this program should receive a feasibility evaluation and separate efforts to develop those methods of highest perceived merit should be funded outside this program.

IMPROVEMENT OF SEISMIC RESOLUTION

Introduction

Seismic reflection methods have steadily improved since their initial use to define gross geologic structure in the 1920s. By the late 1960s these methods were being used for direct detection of hydrocarbons and by the mid-1970s seismic stratigraphy became a useful tool. Today seismic reflection methods are used for virtually all major exploration efforts, and increasingly for reservoir characterization. To a lesser but increasing extent, shallow reflection methods are used in coal exploration and exploitation and in hazardous-waste disposal projects.

The method is limited in resolution because most surveys have concentrated on depths greater than 1000 m and have used dominant seismic frequencies of 20 to 60 Hz. Since resolution (both vertical and horizontal) improves linearly with increasing frequency, it is desirable to develop methods to generate and record higher frequencies. If frequencies that were three times greater could be used in seismic reflection studies, the resolution of this technique would increase by a similar factor. The result would be a much more valuable technique, since it could then locate and evaluate smaller objective targets. Successful resolution improvement, then, would be useful in seismic stratigraphy, reservoir characterization, and the detection of sand lenses in advance of coal mining machines.

Major oil companies currently spend tens of millions of dollars per year on seismic reflection research. Most of the effort is concentrated on targets 2 km or more deep. For this depth range most of the existing data are restricted to frequencies below 100 Hz. Recent research in the United States, Canada, and the Netherlands has shown that reflected frequencies of 200 Hz to 500 Hz are detectable from depths on the order of 200 meters under favorable conditions. These accomplishments have been made with equipment designed about 15 years ago.

Scientific and Technical Challenges

The technical challenge is to use state-of-the-art seismographs in conjunction with shallow seismic reflection techniques to explore the limits of high-resolution reflection seismology. An important target would be to extend the usefulness of this technique to depths of 700 to 1700 m. Success in doubling or tripling the depth range attainable with 200-Hz energy would lay the foundation for resolution improvement at depths exceeding 1 km, by providing new designs for seismographs, geophones, and seismic sources.

Recent results from investigators working in this area are promising. High-quality 250-Hz seismic reflections have been obtained from depths of about 200 m in Pennsylvanian limestones in Kansas. A 30-cm thick coal seam can be detected at a depth of 60 m using a dominant seismic frequency of about 400 Hz. While these depths are far from those that would be useful in oil and gas exploration and production, they indicate substantial promise for these methods. Research to extend these results to greater depths and to other seismic environments is not currently funded within DOE or other government agencies, and no recent reports on this subject have come from the major oil companies or geophysical contractors.

Proposed Research Thrust

This research could be undertaken at any university where CDP seismic reflection data processing facilities and a state-of-the-art high-resolution seismograph are available. There are at least a dozen universities that have the necessary data processing capability. At the present time, there does not appear to be any university or government agency that owns the necessary seismograph equipment, although the Corps of Engineers is likely to procure it in the near future. The equipment necessary to run a proper set of experiments will cost about $250,000. Operational funding at the $500,000 level for two to three years would be necessary for a single group to perform experiments in several geologic situations.

Success in research on high-resolution seismic techniques would allow seismic reflection methods to be applied effectively to smaller reservoirs. This is important--small reservoirs are all that is left to be discovered in many mature U.S. petroleum basins. Success will also improve the resolution of rock thicknesses by seismic reflection. This will be of interest to seismic stratigraphers, whose interpretation capabilities would be enhanced by such improvements. Some of the technology that might emerge from fundamental research could also enhance resolution in hole-to-hole seismic studies.

This research is likely to generate cross-disciplinary interest with stratigraphic geologists and petroleum engineers interested in EOR. Detailed documentation could be provided on acoustical changes in shallow (100 to 200 m deep) production reservoirs during EOR operations. A strong likelihood of substantial academic-industrial cooperation also exists in this area, because of industry interest in improved seismic resolution and reservoir characterization.

BOREHOLE MEASUREMENTS

Introduction

Most of the specific geoscience proposals described elsewhere in this section (e.g., physical and chemical properties of rocks, reservoir characterization, seismic lithology studies) rely heavily on the development of new sources of data concerning rock properties and their variation within lateral distances represented by reservoir dimensions. In this context we have gathered here several interrelated suggestions for borehole measurements research (logging and crosshole) that would support these proposals. In most cases, the further development of existing measurements is used as an example. It is important, though, to keep in mind the possibility that new types of logs might develop the data desired.

Scientific and Technical Challenges

In spite of the very advanced state of wire line logging, more reliable lithology identification from currently available logs is needed. More detailed mineralogical description of the rocks is important even where present gross lithology identification is good. In many areas even the gross lithology identification must be made more reliable. Both of these requirements are essential if quantitative relations between parameters such as V_p/V_s ratio or thermal conductivity and rock type are to be established.

Crosshole studies, such as the tomographic reconstruction of lateral rock property variations or of fluid flow distributions, would benefit from the development of downhole sources and emplacement methods that reduce the amount of energy both generated and radiated by the strong tube waves. These waves are an unwanted and highly undesirable by-product of the generation of P- and S-waves. Tube waves interfere directly with measurements made in the same hole, and the energy radiated by these tube waves interferes with the observation of P- and S-waves in other boreholes.

It is also desirable to have sources where the energy and frequency content can be tailored for studies at different distances and with different resolving powers. These sources would also be valuable for use in VSP surveys where the source is downhole and the receivers are placed on or near the surface. This would allow much more extensive subsurface coverage with little more expense than is involved in current surveys that use a single surface source and a single downhole receiver.

The development of borehole methods for measuring permeability would be of great value to production engineers. Recently described experimental work, in which tube-wave velocities and amplitudes are empirically related to permeability measured in cores in the laboratory, suggests that elastic wave methods for permeability logging are possible. Since the theoretical basis for interpreting these results is unclear, further theoretical and experimental work is strongly indicated.

Longstanding interest among production engineers in measuring the magnitude and direction of stress and fracture in the earth has now been augmented by the explorationists' desire to measure azimuthal variations of seismic velocity. The phemomena of velocity anisotropy, fracturing, and present- and paleo-stress direction are surely interrelated. Acoustic log methods for measuring velocity anisotropy and possibly fracturing have recently been suggested and should be exploited in an expeditious manner. New direct methods for measuring stress in the borehole are also being developed and the simultaneous measurement of several properties should be considered. Some of these new acoustic logging methods, involving dipole sources and bending modes of the borehole, also give primary information on shear velocity and the important V_p/V_s parameter as a function of lithology, porosity, geological history, and other factors.

Proposed Research Thrust

For improvements based on existing approaches, significant accomplishments in the development of specific sensors could be achieved at a cost of $50 thousand to $100 thousand per year over two or three years. Actual instrumentation of logging tools would likely require additional effort. Since many different applications exist, this task would require up to $1 million per year. The work could be carried out at universities, national laboratories, or industrial organizations. In any event, field logging experiments would certainly require the involvement of logging companies, at least on a commercial log procurement basis. Here the participation of industrial organizations could leverage federal funds by making available proprietary drill holes and possibly auxiliary petrophysical and geological information.

DEVELOPING SENSING CAPABILITIES TO MONITOR EOR PROCESSES IN SITU

Introduction

The successful operation of EOR methods is limited by the lack of methods to monitor the advancement of the injected fluids in the interwell spaces. If we could determine the detailed location of injected fluids and the displacement of oil in an EOR process, then we could adjust and possibly optimize the process. Such a capability would enable oil producers to adjust their processes as the reservoir or geological conditions changed with well location.

Scientific and Technical Challenges

The fundamental scientific and technical challenges involved in developing sensing capabilities for in situ monitoring of EOR processes are closely related to the challenges already described in this chapter for improving borehole measurements and for developing new seismic methods for determining the fluid content of rock formations. Progress on these fronts will provide results that can be used to improve the ability to monitor complex flows of multiphase fluids in EOR processes.

Beyond this, some work has been done directly on sensing in EOR processes. This includes the development of a high-temperature, cross-borehole, high-frequency electromagnetic system to obtain tomographs of oil sands undergoing steam injection. There has also been some industrial effort to measure flood fronts in carbon dioxide floods. More work along these lines needs to be stimulated.

Proposed Research Thrust

The implementation of a research thrust in this area should be closely coupled with those already proposed for improving borehole measurements and for developing seismic measurements of lithology and fluid content. Depending on the work proposed in response to solicitations developed for these areas, research related to this priority area could be added on in a complementary fashion.

Funding of work specific to this research thrust should also be undertaken. For example, a variety of centers of excellence in sensor design already exist at several of the DOE national laboratories. Research could be stimulated that would attempt to transfer expertise in sensing capabilities initially gained in response to military needs to the problems of EOR process monitoring.

DETECTING AND MONITORING CORROSION, AND DEVELOPING LIFETIME PREDICTION METHODS FOR MATERIALS SUSCEPTIBLE TO CORROSION

Introduction

Corrosion is a limiting factor in the development of new technologies and engineering systems, particularly where sufficiently corrosion- resistant materials are not available. It may also limit the performance of existing systems, and threaten safety and health when failures occur. Recognition of the critical importance of corrosion has led to the development of new advanced materials, and has led to the documentation of corrosion phenomena for most metals, alloys, ceramics, composites, and other materials.

Despite this progress, significant problems remain unsolved, largely for two reasons. First, the traditional approach to corrosion control and prevention has been narrowly focused on problem solving, development through enlightened empiricism, and perhaps unsystematic application of existing knowledge. Second, the complex nature of corrosion phenomena makes it difficult to develop an understanding of the processes and reactions that lead to corrosive failure.

These unsolved problems have received detailed treatment in a recent report of the National Research Council <u>New Horizons in Electrochemical Science and Technology</u>.[3] That report has conducted a critical evaluation of issues and opportunities in corrosion research and engineering, research on advanced materials, and information dissemination. While research problems in corrosion treated in that report--needs in theory and modeling, the development of in situ and high resolution experimental probes, and lifetime prediction for system applications--are important in general, two problem areas loom particularly large for fossil energy applications, because they are receiving little attention and funding elsewhere. First, there are simply no monitors and detectors that measure the extent or rate at which localized corrosion occurs, at least not until the flaws introduced have become large in area. A need exists to develop new and more effective monitoring techniques for detecting various forms of corrosion and for indicating that replacement or refurbishment measures must be taken. New and more effective corrosion monitors are also required for laboratory studies, so that the kinetics of various forms of corrosion can be accurately defined. This is particularly the case for corrosion phenomena that occur under conditions of extreme temperature and pressure, where direct visual examination of a corroding surface frequently is not possible. Second, lifetime prediction for materials in corrosive environments is a very inexact art, because much is not known about the fundamental processes and chemical events in corrosion.

Scientific and Technical Challenges

The principal goals of corrosion monitoring in oil and gas applications are to detect the onset of corrosion, to determine how far the damage has proceeded, and to predict the probability of failure at some future time. The corrosion problems associated with pipelines, with drilling and production equipment, and with conversion facilities will all be different, and will require the development of monitoring and detection techniques that are specific to the problems encountered in each. In one case, the problems may be related to exposure to hydrogen sulfide and hydrogen embrittlement; in another, aqueous corrosion; and in another, degradation in a highly resistive reaction-conversion mixture. The basic processes that lead to failure are different; the temperature, pressure, and chemistry of the environment are different; and the use of electrochemical techniques may be appropriate for some but not all of the systems described.

The development of the monitoring and prediction capability represents an approach to corrosion control and prevention that focuses on generic issues. Success will provide better capability to extrapolate data and make more effective use of the information currently available. Given sufficient resources, it is likely that significant advances can be made in this area over the short term (10 to 15 years) and that sophisticated monitoring systems will result over the much longer term. The research will complement current work to develop more corrosion resistant materials and will lead to a more comprehensive base on which to control and prevent corrosion.

Research and development for field applications of corrosion detection and monitoring techniques should be undergirded by a strong laboratory-based simulation effort. The principal thrust of the laboratory effort would be to explore new techniques for sampling the properties of a corroding interface, with the objective of determining not only the corrosion rate but also the mechanism of attack. Significant improvements can be made in existing monitoring methods, and new techniques might be devised by transferring technology from other energy systems to fossil energy systems, and from other areas of science and engineering, particularly from electrical engineering, where sophisticated methods have been developed for determining the properties of complex electrical circuits. Spectroscopic techniques might be employed as well, but the methods must be applicable in situ if they are to be developed for monitoring purposes. Applicable techniques may include acoustics and ultrasonics, electrochemical impedance, microstructured electrodes, self-generated noise analysis, scanning electrochemical probes, hydrogen analyzers, and specific ion and compound sensors. Newly emerging in situ techniques for studying corroding surfaces are farther from application as field detectors, but their development should be encouraged for laboratory studies. These include scanning tunneling microscopy, scanning ellipsometry, photoelectrochemical microscopy, surface-enhanced raman spectroscopy, and x-ray and neutron scattering through thin electrolyte films.

The development of life prediction techniques requires advances beyond the present empirical evaluation of in-service failures. There is often inadequate documentation of materials and environmental variables that are related to the failures. Information on the causes of failure, or remedies to it, are not widely disseminated.

Localized corrosion is by far the most difficult area in which to perform lifetime predictions. Few models exist that correlate experimental conditions of alloy or environmental chemistries with pit or crevice growth rates, and there are virtually no available models that can accurately predict incubation times. There is a need to continue basic research related to passive film formation and stability, and to integrate new concepts and findings into life-prediction models. This effort will require the use of mathematics and modeling. This entire fundamental research area is very underdeveloped; progress would make a significant impact not only on fossil fuel applications, but also on many other technologies affected by corrosion problems.

Proposed Research Thrust

A long-term commitment will be required to create the techniques and methods needed to detect and monitor corrosion, and to develop lifetime prediction methods for materials affected by corrosion. Because of the complexity of the phenomena involved, cutting-edge fundamental research will require multidisciplinary efforts involving the participation of materials scientists, electrochemists, metallurgists, physicists, and mathematicians. For some research projects, computational facilities will be needed; some of these facilities are already in existence.

Much of the fundamental work is appropriate for academic research, and APT funds should be used to stimulate greater participation from this sector. At the same time, much of the present empirical information concerning properties and failure histories is in the industrial sector. The development of expert systems and data bases for corrosion engineering has been explored in more detail in _New Horizons in Electrochemical Science and Technology_.[3] An important feature of any federal program supporting research in this area, then, would be to ensure that good coupling evolves between academic and industrial research groups interested in corrosion phenomena.

4

CONVERSION AND PROCESSING: RESEARCH FRONTIERS

INTRODUCTION

This chapter lists in order of priority and describes in greater detail the high-priority "downstream" research frontiers identified by the panel. These research areas provide significant opportunities for cross-fertilization and interaction with the existing Advanced Research and Technology Development (AR&TD) Program under the Office of Technical Coordination. This is because, in contrast to exploration for and production of oil, gas, oil shale, and tar sand resources, there is significant overlap in the problems faced in the conversion and processing of heavy petroleum fractions, shale oil, and coal liquids.

The entire spectrum of hydrocarbon fuels can be classified on a scale of hydrogen-to-carbon atomic ratios (Figure 4-1). Above the scale shown in the figure are approximate H/C atomic ratios for a variety of fossil resources. Below the scale are ratios for the most

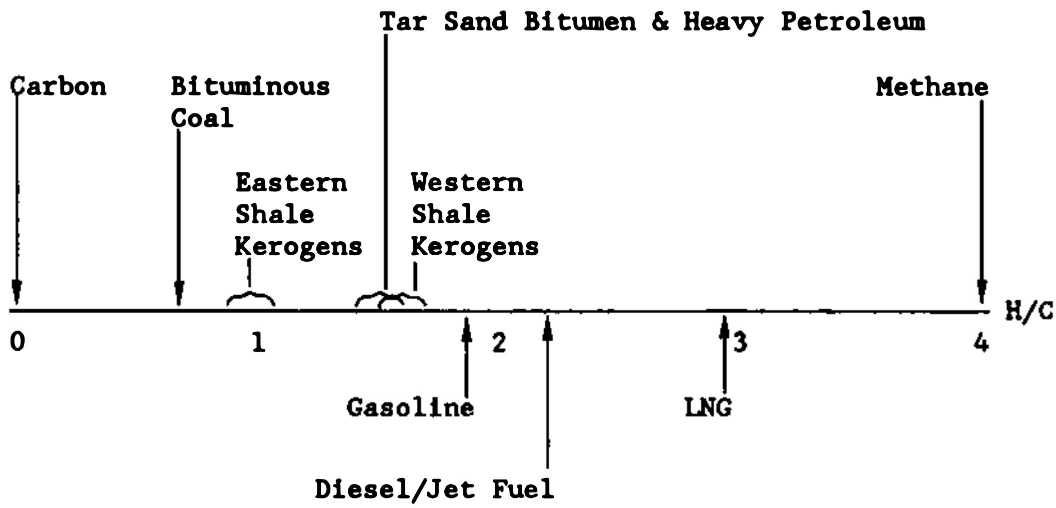

FIGURE 4-1 Hydrogen-to-carbon atomic ratio of fossil fuels and products.

important hydrocarbon products: gasoline, diesel and jet fuels, and liquified natural gas (LNG). Turning fossil resources into hydrocarbon products can be viewed in a simplified way as adjusting their H/C ratios to match those of the desired products. This is done by various chemical reactions, including conversion reactions, where the molecular weight of various resource components is reduced by bond cleavage, and upgrading reactions, where unwanted elements (nitrogen, sulfur, oxygen, various metals) and other inorganic matter are removed. Table 4-1 shows the relative importance of some of these reactions to the processing of coal and a variety of oil resources.

The conclusion that is obvious from this discussion is that the downstream processing of coal liquids, shale oil, and heavy oil resources differs mainly in degree, not in kind. There are common needs that cut across these fossil fuel types, and hence there are great opportunities for application of research results gained from one area to the other. Table 4-2 illustrates some types of coal research currently being funded in the AR&TD program, and indicates potential usefulness of the results to petroleum and gas processing.

For many of the detailed descriptions that follow, the panel has discussed potential synergisms of work undertaken on these topics and research supported by the AR&TD program or other DOE programs.

TABLE 4-1 Importance of Conversion and Upgrading Reactions to Coal and Oil Resources

		Upgrading by Removal of			
Resource	Conversion	Nitrogen	Sulfur	Oxygen	Other
Heavy petroleum	Heavy liquid to light liquid	√	√	--	V, Ni
Tar sand bitumen	Heavy liquid to light liquid	√	√	--	"Sand", V, Ni
Oil shale kerogen	Solid to liquid	√√	√	--	Arsenic
Coal	Solid to liquid	√	√	√√	Mineral matter

NOTE: -- means less important, √ means important, and √√ means very important.

STRUCTURAL CHARACTERIZATION AND DETERMINATION OF STRUCTURE-PROPERTY RELATIONSHIPS

Introduction

The processing of hydrocarbon feedstocks (e.g., petroleum, bitumen from tar sands, kerogen from shale) is strongly dependent on the molecular structure of the feedstock. Knowledge of this structure provides a basis for describing the physical properties of the

TABLE 4-2 Potential Impacts of Research Funded by the AR&TD Program on Oil and Gas Resources

AR&TD Program	Program Component	Potential Usefulness to Oil and Gas Resources
Direct utilization	Beneficiation/comminution	Shale and tar sands
Gasification	Oxidation of methane	Natural gas utilization
	Acid gas separations	Gas refining
Liquefaction	Catalysts for hydrogenating polynuclear aromatics at low temperatures	Upgrading
Materials	Corrosion-resistant alloys and ceramics	Well casings and downstream equipment
Components	Pressure/flow devices; fouling in heat exchangers	All refining operations
Solids transport	Preparation and feeding of coal	Shale and tar sands
Instrumentation, control, and diagnostics	Nonintrusive techniques	Applications upstream and downstream to all resources
University coal research	Coal structure and reactivity	Shale and heavy oil structure and reactivity
	Novel coal depolymerization reactions	Heavy oil and shale conversion
	Surface properties of catalysts	Conversion and upgrading reactions

feedstock as well as its reactivity. Research to establish feedstock structure at the molecular level and the relationship between molecular structure and physical and chemical properties is essential. Research of this type will have an impact not only on the processing of petroleum, shale oil, and tar sand oil, but also on the processing of synthetic petroleum derived from coal.

Scientific and Technical Challenges

Traditional methods of petroleum feedstock analysis involve the determination of elemental composition, boiling point distribution, proportion of polynuclear aromatics (PNAs) and propensity to form coke. While such methods have served well for the routine characterization of petroleum, the growing need to process lower-grade petroleum, residues, and kerogen in an efficient way requires a more detailed understanding of feedstock structure. The techniques best suited for such characterizations include separation methods such as liquid chromatography (LC) and gas chromatography (GC), and structural analysis methods such as infrared spectroscopy (IR), mass spectrometry (MS), and nuclear magnetic resonance spectrometry (NMR). Of particular value are combinations of separation and analytical techniques, such as GC/MS and LC/NMR, that permit the separation of a feedstock into individual compounds (or classes of compounds) prior to structural characterization. Modern analytical techniques can provide a detailed view of functional group distribution, the assemblage of molecular subunits into meso-units, and the conformation of the final macro-molecular unit. Using such information, it should be possible to construct prototypical molecular structures. The models will, in turn, form the basis for understanding intermolecular interactions between feedstock compounds and the interactions of these components with solvents, catalysts, and other processing agents.

Research is also needed on the relationships between feedstock structure and properties. Of particular interest is the relationship between structure and reactivity. Such relationships should be sought through fundamental studies involving compounds, oligomers, or polymers that are thought to be representative of the substances present in the feedstock. Both thermal and catalyzed processes should be studied. An important issue to be addressed is the identification of weak structural linkages that might provide means for molecular weight reduction. Other issues include the elimination of heteroatoms and organically bound metals, and elucidation of the influence of substituent groups and molecular conformation on the reactivity of key structural elements.

Proposed Research Thrust

The APT program can make a significant contribution to the fundamental understanding of petroleum by supporting research groups interested in investigating the structure of complex hydrocarbon feedstocks, and the relation of structure to feedstock properties and reactivity. The research problems identified above are well suited for investigation by individuals or small teams of investigators working either at a university or at a national laboratory. To be successful, such projects should have access to state-of-the-art analytical equipment. It is also envisioned that for certain problems it may be necessary to adapt standard analytical techniques or develop new combinations of techniques.

A typical level of effort for such a project would translate roughly to an annual budget of roughly $100,000 per year per research group, excluding the cost of acquiring special instrumentation. A suite of at least 10 such projects, in addition to the work underway at NIPER, should be supported.

Overlap With Current APT Programs

The APT program currently supports a number of projects as part of a major technical activity in fundamental petroleum chemistry (FPC). Of the three stated goals of the FPC program, "to develop an improved understanding of the composition and reactions of heavy crudes and their products, . . . to acquire data on thermodynamic and thermophysical properties of model compounds and their mixtures that are typical of materials found in liquids derived from petroleum, oil shale, and tar sands, . . . [and] to develop improved techniques for the analysis of fossil fuel derived liquids," the first is identical to the research thrust outlined above. In FY 1987, APT is supporting work at NIPER and Sandia National Laboratories that is focused on understanding the composition and chemistry of feedstocks.

Potential Linkage to the Advanced Coal Program

The Advanced Coal Program includes programs in coal liquefaction and determination of physical, chemical, and thermodynamic (PCT) properties in which research is supported on the structural characterization of coal liquids and other heavy fossil liquids. There may be some opportunities for linkage or synergism between these structural characterization projects and those carried out under the initiative envisioned by the panel.

CATALYSIS

Introduction

Catalysts are materials that selectively direct chemical reactions to produce desired intermediates and products in economically high yields. As treated in this section, they include both heterogeneous and homogeneous catalysts in chemical processes, and exclude biocatalysts.

Catalysts are instrumental in the U.S. production of petroleum products, a sector with nearly $200 billion of manufacturers' shipments in recent years. Continued improvements in catalysis are vital to maintaining the productivity and economic competitiveness of this sector in the future. Of particular importance is the development of more efficient basic refining processes for converting (often heavy) crude oil to gasoline, diesel and jet fuel, and petrochemical fractions. This efficiency could be critical to the economic survival of the industry--when crude costs rise, maximum fuel yield from the barrel of crude is the difference between whether a refiner operates at a profit or loss.

The processes involved in crude upgrading include catalytic hydrodesulfurization, hydrodenitrogenation, and hydrodemetallization. Longer catalyst life for these three important steps will be facilitated by knowledge of how metals accelerate coking, how they deactivate catalysts, and how metals deposited on catalysts can be passivated. Fundamental information on factors governing hydrogen utilization and economy (hydrogen is costly, no matter how it is provided to a given process) in desulfurization and denitrogenation would also be very valuable.

Scientific and Technical Challenges

There are a number of high-priority challenges in catalysis research related to the conversion and processing of oil and gas resources. They include the following:

- Determination of factors governing hydrogen manipulation by the catalyst in hydrodesulfurization, hydrodeoxygenation, hydrodenitrogenation, and hydrodemetallization;
- Determination of catalytic oxygen manipulation by catalysts in methane oxidation to liquids and methane oxidative coupling;
- The possibility of generating hydrogen in situ from the molecules undergoing desulfurization, denitrogenation, demetallization or reformation (If the need for an external source of hydrogen could be avoided, the result would be a major cost savings.);
- Identification of reaction pathways involved on catalyst surfaces in the cracking (molecular weight reduction) of naphthenic compounds;
- Discovery of <u>stable</u> large pore zeolites, and related structures such as pillared clays, that could exhibit much better activity and selectivity in cracking of heavy crude and partial oxidation of paraffins and olefins to organic intermediates; and
- Identification of reaction pathways that lead to the formation of less desirable light ends in cracking and refinery reactions.

The state of the art in each of the above areas is now practiced in industry on a large scale at levels of efficiency that are below attainable levels. However, industrial research typically focuses on optimizing state-of-the-art systems and, especially in recent years, less frequently focuses on fundamental research that could lead to breakthroughs not possible with existing catalysts. When such discoveries are made, industry does take advantage of them, using the highly sophisticated instrumentation that is available in their laboratories to carry these discoveries forward.

Academic research on catalysis has made enormous strides in the last ten years in understanding how catalysts function at the molecular level, in the making and breaking of chemical bonds in substrate molecules. These discoveries need to be carried forward to the place where industry can adapt them to its commercial processes.

Proposed Research Thrust

The research program in catalysis in the DOE Office of Basic Energy Sciences (BES) has supported a number of research groups in basic catalysis in recent years, to the extent that budgets have allowed. This program is very basic in nature. A complementary program in the Office of Fossil Energy, supporting catalysis research that is more applied in nature, but still too fundamental to be carried out on a large scale in industry, would be a useful thrust for the APT program.

Such a program would share interests with the basic BES program in gaining a more complete understanding of chemical transformations of organic molecules on catalytic surfaces at the molecular level. But the flavor of the research supported would be more closely oriented to critical problems involved in the conversion and processing of complex fossil liquids. For example, understanding how a better hydrodesulfurization catalyst could be designed might involve characterization of the surface of a model catalyst by a combination of ESCA and Auger techniques, with scanning ATR employed subsequently to determine the mode of adsorption. In studies leading to a more efficient reforming catalyst, EXAFS techniques could be used to correlate better the spatial relationship of the catalytic metal atoms involved in substrate activation and the mechanism by which they cause bond breaking and remaking to occur. This type of information would provide insights that are fundamental in nature, but that would be oriented toward the design of a more efficient catalyst. Moreover, the fundamental research envisioned here would require significant academic-industrial cooperation and communication.

This program could be carried out in an academic environment containing the necessary combination of solid-state, surface science, synthetic chemical, and engineering expertise. Many universities across the United States offer the necessary combination of expert faculty and advanced instrumentation and facilities. Development of significant ties to industry in terms of communication and, possibly, research cooperation should be an essential part of the development of a successful proposal. A successful proposal should also have a cross-disciplinary character, as might be inferred from the sweep of expertise identified above as being necessary.

The minimum size of such a program, to be effective, is at least $1 million per year.

Potential Synergism with the Advanced Coal Program

There are obvious synergisms between the research proposed in this section and that supported by the Advanced Coal Program. The applicability of fundamental catalyst research to a variety of feedstocks, including coal as well as petroleum, shale, tar sands, and natural gas is quite broad.

Potential Synergism with the Catalysis Research Funded by BES

The design of a research thrust in this area should be carefully coordinated within DOE to avoid needless duplication between the Office of Fossil Energy and the Office of Basic Energy Sciences. This panel feels that the scope of this thrust will fill a useful niche between the basic research funded by BES and the applied research found in industrial laboratories.

CHEMICAL PROCESSING OF METHANE

Introduction

Approximately 50 percent of the energy used in the United States is for transportation, which requires liquid fuels. While the reserves of petroleum are declining faster than new sources are found, supplies of natural gas appear to be holding steady or increasing. Further, in many locations worldwide that are remote from potential users, natural gas produced in association with oil is either reinjected into the well or flared (burned) because there is no infrastructure for its disposition. Capturing this huge amount of energy in liquid form could significantly extend liquid fuels supplies worldwide.

The principal technical problem in need of solution to reach this goal is finding selective, economically viable catalytic methods to convert methane to liquids. Current technology for methane conversion to methanol and hydrocarbons proceeds with reasonable thermal efficiencies (about 60 percent). The process requires a first step to make carbon monoxide and hydrogen by steam reforming, for example, and this is followed by selective catalytic rebuilding to form methanol or gasoline. Presumably conventional Fischer-Tropsch catalysis could be used to make higher hydrocarbons, but the process is relatively nonselective, and products containing from 1 to above 40 carbon atoms are formed. An alternative to the conventional chemical route exists in certain biological systems that are capable of converting methane to oxygenates and hydrocarbons, but the rates of reaction in such organisms is currently too low to be part of a commercially viable process. Investigations into the mechanism of the biological reaction, though, might yield clues useful in the search for better catalytic routes.

A second technical problem--one particularly important for gas sources heavily contaminated with gases such as carbon dioxide, nitrogen, and hydrogen sulfide--is finding more economic methods to selectively remove these and other contaminants from natural gas feedstocks. Current gas-cleaning technology for methane relies on wet scrubbing methods, but these methods are complex and are not economically viable to a number of gas resources, including gas streams where methane is a minority component.

Scientific and Technical Challenges

The search for a selective procedure for converting methane to methanol or to higher hydrocarbons is an intellectually challenging problem. Better catalytic and kinetic understanding is required to carefully control selectivity toward oxidation, in order to avoid further oxidation of any methanol product. Selectivity might also be controlled by methods to separate products from reactants quickly. With biological systems, the challenge is to isolate that part of the organism responsible for methane oxidation, and to determine the mechanism by which it operates. This knowledge could ultimately be used to design a catalytic system.

Research is also needed to elucidate the inherent thermodynamic constraints of any engineering system for methane-to-liquids conversion. Such research would identify the practical focal points on which further work could build, as well as the constraints on potential cost savings.

For gas cleaning, separation methods that could be implemented inexpensively on a large scale for removing hydrogen sulfide and carbon dioxide from natural gas might arise from advances in membrane technology. A pertinent research frontier in separations would be to search for ways to generate improved selectivity in a single separation step, so that the need to conduct a separation process in several stages, which often leads to higher capital costs, is avoided. Improved selectivity in a membrane might be achieved by using combinations of chemical and steric modifications to the structure of the membrane itself.

Proposed Research Thrust

The APT program should support research in the area of catalytic direct oxidations of methane to methanol, and the activation of the carbon-hydrogen bond in methane toward reactivity with various reagents. This new effort would seek to stimulate research in this area at academic and industrial organizations. This area has received more attention in the past than it does now, partly because catalytic carbon-hydrogen activation in methane has proved to be an elusive target. The potential benefits from major advances in this area, though, are so great that the panel believes that additional funding on new approaches to catalytic reaction of methane are warranted. Interdisciplinary approaches between chemists and chemical engineers, and significant industrial-academic collaboration in the areas of catalyst synthesis, characterization, selectivity, and reactivity should be encouraged.

The APT program should also consider supporting research on new approaches to selective removal of hydrogen sulfide and carbon dioxide from methane.

Potential Synergism with Other DOE Programs

The Division of Chemical Sciences in the Office of Energy Research funds many projects related to catalysis, including catalytic hydrogenation of carbon monoxide. This program has supported some elegant work on catalytic activation of carbon-hydrogen bonds in the past, and continues to support one of the leading groups performing such work. Clearly, the comments under the previous topic regarding the avoiding of overlap between any APT thrust in this area and research supported by BES apply here as well. For this topic, the potential for overlap is perceived to be less of a problem, since BES research in catalysis is largely not oriented toward carbon-hydrogen activation at this time. Nonetheless, this research thrust should be carefully designed to complement the existing funding by the Office of Energy Research in this area, and to stimulate wider interest in carbon-hydrogen activation, in developing fundamental understanding, and in developing non-syngas direct conversion routes.

MATERIALS

Introduction

The panel examined research opportunities in materials related to fossil fuel applications, focusing on new and modified construction materials for fossil fuel production, handling, refining, and subsequent processing to petrochemicals and ultimate fuel products. These materials include lightweight composites and new structural ceramics and castings that resist harsh or corrosive environments better than metals. Also considered was the impact of the use of new ceramic materials in automotive engines.

The science and engineering of materials is a research area of critical importance in fossil fuel production and conversion because these fuels are produced and processed in unusually harsh and corrosive environments. On-stream time in processing plants, lowered failure rates in equipment for exploration and production, minimization of undesirable contamination of process streams and final products, and construction of reaction vessels capable of withstanding elevated temperatures, pressures, and reactive or corrosive charges--all are directly dependent on material selection and capability.

The advent of ceramic automobile engines--predicted but not yet timed for appearance--may also have a profound impact on the chemical structure required for future fuels. This development would greatly affect refinery design and even the balance of fossil fuel sources that would be required (see section on Alternative Transportation Fuels, page 45).

In the last 10 years, materials researchers have made great strides toward ceramic materials with substantially improved structural capabilities, including increased toughness and better ability to withstand high temperatures. Organic and inorganic fiber-reinforced composites and alloys now offer combinations of chemical resistance and weight savings not previously available. These materials have tremendous potential application to the fossil fuel-processing industries.

Scientific and Technical Challenges

The following research areas and study topics should receive high priority in terms of support:

- Investigation of new ceramic and composite materials (inorganic and plastic) for usefulness in environments marked by severe chemical and thermal stress;
- Investigation of thermally rugged plastic and ceramic coatings for lining well casings, process vessels, and fuel conveying devices; and
- Development of high-temperature, tough, structural ceramics and economic forming processes that might be used in manufacturing important process equipment such as turbine blades and high-temperature pumps.

Proposed Research Thrust

Advanced materials programs relevant to these opportunities and needs are in progress in at least three dozen universities, several national laboratories, and several private research institutions. Organic, polymer, and inorganic chemistry; materials evaluation and engineering; surface science and characterization; and manufacturing engineering all are essential components of the most successful programs in existence. Rapid progress is being made in those programs with a good balance of academic and industrial participants.

The APT program should not attempt to start from scratch in developing collaborative groups and centers in this area, but should rather seek to capitalize on the infrastructure that already exists. What is needed is a program to target existing research that is most relevant to fossil fuel processing for additional necessary support, and to develop a mechanism to transfer the knowledge that would be gained to industrial settings where it could be developed further and put into practice. For example, several intensive academic/industrial collaborations are in progress at centers based in academic institutions. Modest seed funding from the APT program that would foster interest at these centers in needs of the oil and gas processing industries would be a very cost-effective step toward achieving the panel's goal for this area. If this process uncovered areas where voids in fundamental materials knowledge existed that would be directly relevant to the scientific challenges outlined above, special funding of specific projects at these existing centers by APT might be appropriate.

INSTRUMENTATION DEVELOPMENT

Introduction

Advances in analytical instrumentation have had a dramatic impact on adding to our knowledge base in many aspects of petroleum processing and related technologies. For example, the application of high-pressure liquid chromatography (HPLC) combined with field-ionization mass spectrometry has made it possible to identify the molecular weights of the nonvolatile components present in petroleum. Optical microscopy combined with both Fourier-transform infrared (FTIR) and Raman spectroscopies have led to a characterization of the spatial variations in chemical compositions of coal at the level of several micrometers. Exciting advances in solid state magic-angle spinning nuclear magnetic resonance spectrometry (MAS/NMR) have made it possible to identify the location and distribution of silicon and aluminum in zeolites, and carbon in nonvolatile petroleum fractions, shale kerogens, and solid coal. In light of these and many other contributions of instrumental analytical techniques, it is essential that further research on the development of instruments and their application to problems of petroleum processing be pursued. Further additions to our fundamental understanding of resource and feedstock structure (see previous section on Characterizing Feedstock Structure) are to be anticipated from new instrumentation advances.

Scientific and Technical Challenges

An activity that would have a significant impact on the economic vitality of the U.S. oil and gas industry is the development of miniaturized analytical instruments that could be used for on-line analysis of process streams. While gas chromotography is now in use on-line, other instrumentation, such as FTIR and NMR, are used predominantly in a laboratory environment. Research and development is needed to provide small rugged sensors and microprocessors capable of operation at high temperatures and pressures, and in the presence of corrosive gases or liquids. These rugged sensors might be in the form of specialized optical fibers or optrodes.

A second research area expected to make a contribution is the use of x-ray and NMR imaging. Current technology makes it possible to carry out tomography of three-dimensional objects with a resolution of about 1 μm. Using synchrotron radiation, it is even possible to obtain images of systems undergoing change on the time scale of approximately 1 s. NMR imaging provides not only spatial and temporal information, but also information on the elemental distribution and even the structural environment of each element. The application of imaging techniques to the characterization of kerogen in shale and oil in core samples is expected to provide an unprecedented level of information about the structure of the organic matter in its original environment. Information from such studies will contribute to an understanding of the content of organic material and could suggest novel ways for extracting it.

Successful instrumentation development will require highly interdisciplinary efforts. Measurement of pH can provide a simple example. To design an instrument capable of carrying out a pH measurement in a laboratory is a relatively simple undertaking. Performing the same measurement downhole at 300°C and at pressures of a few thousand psi is a far more complex problem, requiring materials expertise to produce a detector that can withstand the corrosive environment, analytical expertise, signal transmission expertise, and geological expertise to correlate the data with physical phenomena.

Proposed Research Thrust

The APT program should undertake a substantial effort in instrumentation development. A number of centers of excellence at national laboratories, universities, and industry already exist. There is every reason to believe that a pool of high-quality proposals would materialize fairly quickly in response to a solicitation.

Overlap With Existing APT Programs

Within the program activity on fundamental petroleum chemistry, the APT program is already supporting instrumentation development of a multielement detector for gas chromatography. This is a good example of the type of effort that should be expanded and extended to other types of instrumentation.

EXTRACTION OF OIL FROM SHALE AND TAR SANDS

Introduction

Oil shale and tar sands constitute important hydrocarbon reserves available within the continental United States. One principal drawback to the utilization of these reserves is the development of efficient and cost-effective means for separating organic matter from the inorganic matrix. In the case of shale, the organic material, called kerogen, is a three-dimensional polymeric material that must be broken down to oil. Current approaches involve pyrolysis of the shale rock, which is energy inefficient since one must heat and cool about 10 kg of rock in order to get about 1 kg of oil. Solids handling is a major challenge in process design, and shale particles have to be reduced somewhat in size for processing. For both shale and tar sands, an understanding of the factors controlling the interaction of oil and kerogen on the host matrix could contribute to the suggestion of alternative ways for disengaging these organic substances from the matrix.

Scientific and Technical Challenges

Current processing of shale above ground involves crushing and grinding of the rock. These processes are known to be highly energy inefficient; typically 5 percent or less of the total energy expended is used to accomplish rock fracture. Research aimed at significantly raising the energy efficiency of the grinding process would be desirable. Approaches to be investigated are wet grinding and the addition of small particles of high hardness (e.g., silicon carbide) that might act as grinding aids. Another approach to be explored is chemical comminution of shale. For eastern oil shales, which are true shales, this might involve the addition of salt solutions or other agents that could intercalate the clay in shale rock. For western oil shales, which are carbonate-based marlstone, the treatment of shale with acid solutions might effect comminution by inducing corrosive stress fracture of the rock.

A second area requiring attention is the elucidation of oil-rock and oil-sand interactions. To this end it would be useful to apply analytical techniques used traditionally in surface science and catalytic studies. Information gained from such investigations would reveal the minimal temperatures required for thermal release of oil from its host matrix. Insights might also be gained regarding the possibility of introducing chemical releasing agents that might weaken the oil-rock or oil-sand interaction. If the organic substances from shale or tar sands can be enriched, new processing technologies will be needed, particularly those that will reduce coke.

Proposed Research Thrust

A modest program activity within APT would be appropriate, perhaps seeking to combine expertise from mining departments in universities with that from chemistry, chemical engineering, or petroleum engineering departments. Pursuit of the problems described above will require appropriate instrumentation. In the case of shale comminution, access will be needed to grinding equipment and instrumentation for measuring particle size and size distribution. Observation of the shale particles by scanning electron microscopy following grinding or chemical comminution will also prove useful. For the studies of oil matrix interactions the most suitable techniques are photoacoustic FTIR and solid-state MAS/NMR.

ALTERNATIVE TRANSPORTATION FUELS

Introduction

Two-thirds of current annual consumption of petroleum is in the transportation sector. In the event of a sudden curtailment of imports, there are relatively few opportunities for this sector to switch to alternative fuels. A long-term effort to increase the technological options for other transportation fuels could significantly benefit national energy security. Of course, technological barriers are only one set of obstacles to the implementation of alternative fuel choices; economic forces, marketing considerations, and institutional and infrastructure barriers also play key roles. Nonetheless, an effort with a long-term orientation, focused on identifying and addressing the scientific and technological components of this problem, will at least help society to face alternative fuel choices from a more informed technical standpoint than is currently the case.

The range of possible alternative fuels is very broad. In terms of decreasing atomic ratios of hydrogen to carbon (see Figure 4-1), the principal possibilities of interest to the APT program are alcohol-based fuels and fuel blends containing alcohols such as methanol and ethanol, new fuels from different sources (e.g., oil shale, tar sands) with hydrogen-carbon atomic ratios approximating modern gasoline, new fuels with hydrogen-carbon ratios approximating today's diesel fuels, and fuels with low hydrogen-carbon ratios.

The fuels on this list do not all face the same technological barriers. For example, the obstacles to the introduction of methanol as a transportation fuel are not wholly technological or ecological, but rather are market barriers that relate to the introduction of any new fuel requiring a modified engine. In contrast, the introduction of fuels with low hydrogen-carbon atomic ratios will require the solution of a variety of difficult scientific and engineering problems before the generic economic problems of moving new fuels and engines into the market can even be considered.

Virtually all of the fuels on this list are currently under active, although fragmentary, investigation somewhere in the world. There have been attempts to probe the use of minimally processed petroleum fractions or lower quality liquids in engines, but this work has not been able to make significant progress on the problems of the fuel/engine interface. Some other recent work has aimed at building a diesel-type engine capable of running on powdered coal.

Scientific and Technical Challenges

A number of the potential candidates for alternative fuels present a rich array of scientific and technical challenges. If the fuel refining industry is to be able to respond flexibly to changing demands for fuels with different compositions, more fundamental research is needed to lay the ground for flexible plant design and processing technologies. For lower hydrogen/carbon fuels, solving the fuel/engine interface problem will require more fundamental work in combustion science to ensure complete combustion and fuel burn-out without forming soot or severe carbonaceous engine deposits. Higher combustion temperatures may be required, leading to needs for new construction materials for these engines. A number of the potential alternative fuels may present storage, compatibility, ignition, and perhaps health and safety problems, all of which must be investigated and resolved.

A related set of challenges derives from the impact on fuel requirements of new designs and materials now in the development stage for advanced engines. The panel is not aware of existing studies of the implications of these developments, and such studies might uncover some anticipatory types of research appropriate for federal involvement.

Proposed Research Thrust

Although end-use oriented, research on alternative fuels should be considered appropriate for federal funding because of the intrinsic long-range nature of any scenario involving fuels with markedly different hydrogen-carbon ratios than those in use today. Given the formidable institutional barriers to the introduction of new fuels and engines, significant attention to alternative fuels research from industry is only likely to occur when independent, high-risk research can demonstrate feasibility.

The panel believes that the APT program would be well placed to stimulate fundamental research on the most significant scientific and technological questions. For example, research aimed at developing combustion systems that can effectively utilize fuels of less than 12 to 13 weight-percent hydrogen (equivalent to a H/C atomic ratio of about 1.6) is of high priority, since such engines would facilitate the introduction of substitute fuels derived from minimally processed petroleum fractions and lower quality liquids from shale oil and coal. An appropriate APT thrust could be to concentrate on combustion properties and science of low-hydrogen fuels. Capabilities at NIPER in engine testing design might be applicable to these research frontiers.

A second activity for the APT program would be to commission a study of the transportation fuel implications of automotive-oriented ceramic programs in the United States and abroad that may give rise to higher temperature engines.

5

OTHER RESEARCH FRONTIERS

As described in the preface to this report, the panel began its deliberations by considering the broadest possible list of research frontiers related to oil and gas resources. The panel gave particular attention to potential research frontiers in two specific areas: environmental research and arctic and offshore research. No topic proposed in either area was judged to be of the high priority given to those topics described in the previous two chapters. Because the existing APT program contains a subprogram in arctic and offshore research, and because environmental problems are an important challenge in technologies for the extraction of unconventional oil resources (e.g., in situ retorting of oil shale), the panel felt that a description of its deliberations that led to the exclusion of these topics from the final list of high-priority topics would be appropriate as background information.

ARCTIC AND OFFSHORE RESEARCH

Arctic and offshore research (AOR) comprises one of the two organizational components of the APT program. This program had its genesis in a 1981 report from the National Petroleum Council entitled *U.S. Arctic Oil and Gas*[4] and its recommendation (page 8) as follows:

> Continued private and public Arctic research is important to the national interest and should be encouraged and supported where necessary. Research and development in Arctic technology for operations in hostile environments will lead to evolutionary improvements in operating systems. Efforts to enhance knowledge of ice conditions, ice properties, and ice forces should be stressed. Biological research and monitoring should be continued. Federally funded research programs should focus on collection and characterization of fundamental data and testing programs of broad issue. Timely and rapid dissemination of information obtained by federal agencies should be required.

In FY 1987, the AOR program focused mostly on a few projects in ice research, and on development of an Arctic and Offshore Research Information System (AORIS). As described in a presentation to the panel by DOE staff, the planned development of AORIS is reaching the stage where the system, if proved successful, can be placed on a self-supporting basis at the Morgantown Energy Technology Center, and future APT funding for this system will be eventually phased out.

Considerable background material on the existing subprogram in arctic and offshore research was made available to the panel prior to its meeting. This material included descriptions of each project supported in FY 1986, the FY 1987 APT Program Plan containing a detailed description of the AOR subprogram, and a broad list of potential research topics related to fossil energy resources in the Arctic. This latter list was taken from the proceedings of a 1984 workshop on arctic energy technologies,[5] and included research topics in areas such as arctic offshore structures, arctic offshore pipelines, subice development systems, and polar capable ice vessels. During the assembly of the initial list of research frontiers, panel members identified and discussed two research areas relevant to energy development in arctic regions: ice research and the impact of oil spills in arctic seas.

On-site studies of changes in thick ice sheets, resulting from regular climatic variations, were thought to be of interest. This opportunity, though, was not felt to be of intrinsically high priority, compared to the priorities discussed in the previous two chapters. In the case of arctic oil spills, more work may be needed to assure that oil spills, should they happen under ice or in rough seas, could be contained and disposed of safely. Yet the scientific and technical challenges in this area seemed to be largely in extending work that, by itself, was probably sufficient both to understand the fate and effects of oil exposed to typical underice and open sea environments and to adapt current oil-spill containment and mitigation technologies to the more severe conditions found in arctic seas. As a result, this research area was also considered to be of less importance compared to those in the previous two chapters.

A few other arctic and offshore research areas that are either too fundamental in scope or too large for industry to undertake alone should be mentioned. They are as follows:

- Characterization of the underside of ice with respect to its thickness, roughness, strength, and profile--including further extension of sensing techniques such as impulse radar for the aerial survey of ice keels;
- Mapping and characterization of the shelf edge soils--indications are that these are very unstable and that slumping is widespread;
- Development of power supplies for long-term underice needs, including scientific work and communications; and
- Tests to determine the phenomena involved when large-scale ice floes impact natural "structures"--here government participation with industry in such programs as the Canadian Hans Island test could produce fundamental data of long-term importance.

In view of the eventual completion of APT funding of AORIS, and the lesser relative priority given to other possible research topics for the Arctic and Offshore Program, the panel recommended that support for

AOR not be augmented over the level recommended in the FY 1988 administration budget request. As existing AOR projects come to their scheduled completion, the management of the Office of Technical Coordination should consider whether these funds would not be better used in the support of topics perceived by this panel to have higher priority.

ENVIRONMENTAL RESEARCH

One topic in environmental research has already been discussed in the previous section. The panel identified and gave consideration to another environmental topic: groundwater contamination from in situ oil shale processing. Toxic products from underground shale heating and combustion might eventually contaminate nearby aquifers serving as present or potential sources of drinking water. Research, if successful, could at a minimum define the probability and consequences of such hazards and could conceivably offer ways to mitigate, contain, or eliminate them.

The attendant environmental risks of in situ oil shale processes have already received some attention from DOE. Researchers using survey wells have obtained some information on the extent of contamination around operating in situ retorts and constructed worst-case scenarios using limited data. Limited research has been done on containment techniques to seal abandoned retorts. To some extent the same concerns about groundwater contamination are involved in aboveground shale processing, but the focus is largely on surface water contamination. Current industrial-scale plant operations have demonstrated that these aboveground risks can be mitigated.

Future research challenges in this area largely involve extension of existing technologies. Analytical studies to identify potential contaminants, and estimates of their fates and rate of migration are needed. Further studies of ways to seal off or contain contaminants are called for. These considerations should be addressed, though, with the same priority as research on oil shale in general. As noted in the previous chapter, the panel suggests that research on shale be deferred in favor of research on oil and gas resources.

In addition to these specific considerations, the panel felt that high-priority environmental research related to oil and gas production and utilization is already receiving attention in other programs--both within and external to DOE--that have more funding available to them and a specific mission in environmental science and technology. Given the other high-priority areas in this report that are appropriate only for the APT program, the panel feels that a specific environmental thrust is not required, and that environmental issues related to fossil fuel production, processing, and utilization should be dealt with in these other, more targeted programs.

6

RECOMMENDED LEVEL OF EFFORT AND FUNDING

INTRODUCTION

The present Advanced Process Technology Program in the Office of Fossil Energy is budgeted in the FY 1988 Administration Request at approximately $2 million. Its average size in recent years has been closer to $5 million. This panel has concluded that, to be effective, this vital program should support the high-priority research topics outlined in this report at a minimum level of $10 million. A budget totaling $20 million, with $10 million allocated for the geoscience-related topics presented in this report and $10 million devoted to the other topics, is well justified. The panel's recommendations for supporting high-priority advanced exploratory research through the APT program, under a number of potential budget scenarios, are summarized in Table 1-1 (see page 12) and are described briefly in the following sections.

In addition to increased funding, it would be desirable for APT program managers to consider ways in which they could maximize the transfer of results gained from the research recommended in this report to a wide user community that would include smaller and independent drilling companies, as well as the major oil and service companies. While the panel did not have the opportunity to consider at length the best mechanism for such transfer of information, this issue emerged as an important need during the peer review of this report.

The panel noted with interest the current proposal for new mechanisms of cooperative research and development funding by the DOE. Such mechanisms, or extensions and modifications of them, might be useful to increase the leverage of the funding levels recommended below. They should <u>not</u> be viewed, though, as an alternative to increasing the level of DOE support to the amounts needed for effectively addressing the critical needs and research opportunities outlined in preceding chapters.

SCENARIO 1: MINIMUM EFFECTIVE LEVEL OF SUPPORT

After identifying the highest-priority research topics, the panel tried to quantify the minimum level of support needed to sustain an effective research program encompassing these topics. Several assumptions were built into this estimate. First, since these topics are all fairly fundamental in nature, it was assumed that additional costs of conducting any research in the field, as opposed to the laboratory, would be minimal (or leveraged with funds from other sources). Second, it was assumed that research applicants would already have at their disposal adequate facilities and instrumentation to support their research, and that additional costs for facilities and expensive instrumentation would also be minimal at first. As frontier research in these areas continues to progress, though, the APT program will eventually have to bear its share of instrumentation and facility costs for its researchers. Third, since cost-sharing is often present in programs supported by the Office of Fossil Energy, the panel made a rough assumption that the amount of funds added to DOE's research support by this mechanism, summed over the entire APT program, would approximately balance the total amount of APT research funds consumed by overhead and similar charges. Thus, the dollar figures given below represent, to the panel, levels of support that actually reach the principal investigator on a given research project.

With these assumptions in mind, the panel estimates that the minimum effective level of effort needed to pursue the areas outlined in this report is equivalent to a budget level of about $10 million per year. This budget level is about twice the average funding level for the APT program in recent years. In the panel's view, increasing the total budget for the APT program from its traditional level to about $10 million represents an increment in support that has a realistic chance of success in the budget process.

Even at this minimum effective level, the depth of treatment possible for a number of high-priority research topics would be far from ideal. Several of these topics encompass a number of subareas (for example, three subareas are identified in this report under Extraction of Oil from Shale and Tar Sands). An APT program with a budget total of $10 million could probably build a critical mass of projects in only one of several possible subareas for these research topics. In other words, the research frontiers outlined in this report are sufficiently promising that they could be the basis of a high-quality program with a multiyear pattern of budgetary growth beyond the $10 million level.

SCENARIO 2: ENHANCED SUPPORT FOR GEOSCIENCE RESEARCH FRONTIERS

The Energy Research Advisory Board (ERAB), which reports to the Secretary of Energy on research matters, has recently issued a report entitled Geoscience Research for Energy Security.[1] The ERAB report strongly supports additional emphasis on a number of research frontiers in the geosciences, several of which have been identified independently by this panel in Chapter 3. In the event that the ERAB report becomes the basis for a special initiative to fund geosciences research in DOE, this panel recommends that the APT program receive sufficient funding to pursue the research opportunities described in Chapter 3 at a level well above the minimum target proposed in the preceding section. The high-priority research frontiers related to the geosciences that have been described for exploration and production are sufficiently promising that a high-quality research program could easily require a budget of $10 million for these topics alone.

Funding of these topics by the APT program under a departmentwide geosciences initiative could also have the benefit of allowing enhanced support of other high-priority topics described in this report. In such a case, the panel recommends that the nongeoscience topics be funded at a level of $10 million, thus yielding an APT program with a total budget of $20 million.

SCENARIO 3: FUNDING AT THE AVERAGE RECENT BUDGET LEVEL

In making its recommendations for topics that might be supported at a budget level near $5 million (see column B of Table 1-1), the panel adopted three criteria:

- Scientific and technical importance of the research topic;
- Lack of support or grossly insufficient support from other sources, both private and public; and
- Prospects for a success that would allow development to proceed in an intermediate timeframe.

The topics that would be recommended for funding under this scenario would be the following:

- Physical and chemical properties of rocks;
- Predicting reservoir internal structure and heterogeneity;
- Dynamics of dispersed systems;
- Novel methods for enhanced oil recovery;
- Seismic methods for determining lithology and fluid content;
- Characterization of feedstock structure and structure-property relationships; and
- Chemical processing of methane.

An APT program funded at merely the average budget it received before its reorganization and redirection in late 1986 seems not to take advantage of the opportunities afforded by such a change. While such a scenario is certainly better than the worst-case scenario described in the following section, most of the exciting and potentially rewarding research frontiers identified in this report would be left unsupported, including some high-risk research opportunities with a high potential payoff if successful. Also unsupported would be a number of opportunities in conversion and processing where research advances could provide synergism to coal research supported by the existing Advanced Research and Technology Development Program.

SCENARIO 4: FUNDING AT THE LEVEL IN THE FY 1988 BUDGET REQUEST

At the funding level requested in the President's FY 1988 budget, little progress can be made on the critical research needs spelled out in this report. The recommendation by the panel in this worst-case scenario would be to maintain those research projects in the APT program that currently address the highest priority needs identified in this report (see column C in Table 1-1). This is a different philosophy of setting priorities than that reflected in the previous scenarios, which is reflected in a slight difference in topic selection. The existing projects that correspond to the high-priority needs in this report are in the following areas:

- Predicting reservoir structure and heterogeneity;
- Characterization of feedstock structure and structure-property relationships; and
- Instrumentation development.

The combined current budgets for these projects are approximately at the FY 1988 requested level. It must be emphasized, though, that this level of effort is far short of meeting the national need for fundamental research related to oil and gas resources. This worst-case scenario would require the APT program to forego funding most of the research areas that hold great promise for significant advances.

CONCLUSION

Fundamental research to facilitate the efficient detection, recovery, and processing of domestic oil and gas resources is of critical importance to a national strategy of preparedness for a future of uncertain supplies and prices for liquid fossil fuels. A program structured along the high-priority research frontiers identified in this report promises significant benefits to national security and economic competitiveness. In a political environment dominated by fiscal and budgetary constraints, it is perhaps tempting to think of postponing investments in energy research with a long-term payoff. Such a policy would be extremely shortsighted, as it would leave the United States vulnerable to disruptions in supplies and prices for fossil fuels. The opportunities for fundamental research related to oil and gas resources are intellectually tantalizing and remarkably diverse. A timely response is required to address these opportunities, so that the nation will be able to face its long-term energy future with confidence.

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