

**A REPORT OF THE BASIC ENERGY
SCIENCES ADVISORY COMMITTEE**

**1989 REVIEW OF THE BASIC ENERGY
SCIENCES PROGRAM OF THE
DEPARTMENT OF ENERGY**

February 1990



U.S. Department of Energy
Office of Energy Research
Washington, D.C. 20545

MASTER

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I. INTRODUCTION

During 1989 we continued our review of OBES research programs. The BESAC subcommittee reports (Appendices 7 -14) are summarized in Section III. Considerations of the adequacy of these programs, in regard to scope and balance, in supporting the Department's mission needs was given special emphasis in our deliberations.

The 1989 Terms of Reference (Appendix 1) charged the Committee with several new tasks. BESAC comments were submitted on the OBES x-ray lithography initiative (Appendix 5); comments on the human-resource issue appear in Appendix 6.

In response to a separate request (Appendix 2) for BESAC review of the July 1989 Draft Environmental-Restoration Research Plan, comments were provided (Appendix 3); a detailed report of the findings and recommendations appears in Appendix 4.

The report of the joint BESAC / HERAC Global Change Subcommittee (organized in 1988) is reproduced in Appendix 15. The report has been approved by BESAC, but not as of this date (January, 1990) by HERAC.

During all Committee and Subcommittee activities BESAC members remained conscious of the importance of avoiding real as well as perceived conflicts of interest , recusing themselves from the deliberations where necessary.

II. OVERVIEW

The Office of Basic Energy Sciences (OBES) continues to perform its essential, multiple national roles in science and technology programs in the face of nominally level but actually deflating annual budgets. It supports major user facilities such as synchrotron light sources and research reactors for DOE and for the national academic and industrial research communities. It funds and manages basic research programs in materials sciences, chemical sciences, computational and mathematical sciences, biosciences, geosciences and engineering, both in the national laboratories and in universities. It has attracted and maintained a distinguished cadre of research scientists in these research

operations who serve as a major resource to DOE and the nation in addressing the diverse aspects of national energy programs.

At a time when DOE is challenged to utilize all of these capabilities in solving vast problems of energy supplies and conservation, global change, the environment, and national economic and defense security, the functional elasticity of OBES has become strained by the continued funding stretch out and by the mandated allocation of funds to activities outside of energy research. Yet, the continuing commitments to operate and upgrade for safety the existing major user facilities, to plan and construct new ones, and to prepare to close down others, will inevitably require significant growth rates for the facilities budget. At the same time, research progress in all of the areas supported by OBES justifies appropriate selective expansion to provide a nationally essential flow of new concepts, data and technology. Adding to these growing requirements the special needs dictated by accelerated concerns over domestic energy supplies, global change, and the environment, leads us to conclude that the OBES budget is not in balance with the Department's mission demands. Inadequacies are apparent, for example, in emerging thrust areas such as the geosciences and the environment. Moreover, these shortfalls cannot be addressed effectively by reprogramming resources currently committed to traditional OBES strengths, such as materials and chemical sciences, since these remain critically important to DOE and are experiencing serious budget pressures as well.

The current coverage of physics-related topics illustrates the imbalance problem. In response to strong encouragement from the physics community, BESAC organized an *ad hoc* subcommittee during 1989 to examine the scope of OBES physics research. The subcommittee identified critical DOE mission-related requirements for basic research in plasma and optical physics which are currently missing in the OBES program, as well as persuasive needs for expanded research efforts in atomic physics. The subcommittee report, which is reproduced in Appendix 8, offers detailed recommendations.

The Department is currently developing a long-term national energy strategy. This is, in our opinion, as important to the nation as any other national security issue. It is important, we believe, to build into this plan a concept for the long-term growth in activities of OBES on behalf of, and in support of, all major energy issues.

We are forced to conclude that significant expansion in the OBES scope, accompanied by growth in its budget and research management function, will be necessary if OBES is to provide the needed research support, and to strengthen the essential bridge to DOE's applied activities. Maintaining the presently prevailing constraints would very rapidly necessitate: (1) elimination of some of the currently operating or planned facilities, thereby reducing our national research competitiveness in many different disciplines; (2) the loss of leading individual research activities for which there are no adequate substitutes in the national research program; (3) the failure to initiate in timely fashion diverse new basic research directions essential to the Departmental mission, where the lead time for productive yield may be five to ten years; (4) the failure to mount basic research programs matched to the foreseeable resource and environmental challenges of the twenty-first century.

We specifically recommend that OBES develop a long-range plan for achieving the desired program balance. The plan should identify needed research initiatives, as well as the associated costs and time lines.

III. RESEARCH PROGRAMS

(1) Major Facilities

■ **Because of chronic concerns over inadequate operating funds for major facilities, we continued to monitor the status of DOE's photon and neutron facilities in some detail in 1989.**

■ **We note with approval that the recommended funding increases were included in the FY90 OBES budget. Unfortunately, the combined effects of the partial Gramm-Rudman-Hollings sequester, Congressionally mandated initiatives, and new initiatives directed by DOE/ER are likely to reduce these increases significantly.**

■ **In regard to selected individual facilities, we note the following:**

1. HFIR; HFBR

Continuing problems with both facilities, and the resulting delays in restarting the reactors, are having a serious impact on the U.S. neutron-scattering community and are therefore of great concern. In the meantime, the recommended upgrade of the HFBR beamlines and instruments has not been implemented.

2. IPNS; LANSCE

In the absence of any DOE steady-state neutron sources, it is essential that operation of the pulsed neutron sources be fully funded. However, anticipated funding levels during the current fiscal year will force reduced operations.

3. ANS

DOE's commitment to this facility remains uncertain. The recommended line-item funding for R&D in support of the conceptual design activity has not been approved. Affordability limits imposed by the OBES operating budget appear inadequate to support the original completion date.

4. SSRL

Agreement has been reached for full dedicated use of the SPEAR ring. However, the required funding may not be available.

5. APS

It is encouraging that FY90 construction funds for this synchrotron facility have been approved, and that this project is moving forward.

RECOMMENDATIONS

Although OBES responded positively to most of our recommendations, progress has been limited and our 1988 recommendations must be repeated:

- o **Sufficient funds should be provided to assure that existing facilities are optimally operated.**
- o **In view of the national need for new research-reactor facilities, the pace of the ANS R&D activity should be accelerated, preferably by means of line-item funding.**

- o **The existing reactors should be restarted at the earliest possible date, consistent with safety requirements, and the HFBR upgrade initiated.**

(2) Materials Sciences

■ This program remained essentially unchanged in 1989. It is broad in scope, and well-structured to meet DOE's materials-science needs. The program is a critical component of U.S. basic research in materials, with significant impacts on national security in areas of energy, defense, and economic competitiveness. Budgetary constraints have prevented all but modest evolution of the program over the past year. Those changes that have taken place are consistent with established priorities, and are consistent with the views expressed in earlier BESAC reports.

■ Responding to our 1988 recommendations, OBES has initiated a number of important new synthesis and processing activities.

RECOMMENDATIONS

- o **Consistent with national needs, the materials sciences funding levels should be increased above the inflation rate.**

(3) Chemical Sciences

■ During 1989 we concentrated our CSD efforts on funding issues, the status of last year's recommendations, and new proposals and initiatives. In general, BESAC continues to be favorably impressed by the excellent quality of the CSD research program. Its scope is closely related to, and in many ways complementary to, that of the materials sciences program.

■ The increased funding level recommended in our 1988 report were not realized in the FY90 CSD operating budget. As a result, the recommended program initiatives have been adversely impacted :

1. Combustion Research Facility (CRF)

The planned phase II initiative for the CRF at SNL (Livermore) has still not been funded. After several consecutive years without funding, this initiative has now been supplemented by a new joint proposal for FY92 from SNL and LBL for a Combustion Dynamics Facility. The new proposal will emphasize LBL's effort on fundamental aspects of combustion chemistry including molecular level understanding of combustion reactions and SNL's effort on complementary basic and applied research, including the development of optical diagnostic techniques.

2. Large Einsteinium Activation Project (LEAP)

The LEAP initiative recommended in the 1988 report has not been started. This program is dependent on HFIR at ORNL. BESAC is very concerned about the long delay in the restart of HFIR.

3. Molecular Science Research Center

The Environmental Molecular Sciences Laboratory at PNL will house the Molecular Sciences Research Center. Over the past few months several prominent scientists have joined the Center. The \$6M of new funding which was requested in the FY90 budget for this initiative has been eroded by the requirement to fund other mandated projects.

4. Atomic Physics Storage Ring

The atomic physics storage ring initiative has been given thorough review and endorsement by the scientific community. Competing proposals have been reviewed.

RECOMMENDATIONS

We repeat the following recommendations from last year:

- o **The CSD budget, in real terms, should be restored to its FY81 level.**

- o Support for combustion research, which is central to DOE's mission, should be increased.
- o Additional research support should be made available for atomic physics.
- o Research initiatives based on the Pimentel and other National Academy reports should be supported.
- o The CSD Subcommittee reiterates its 1988 observation that capital equipment allocations for single investigators are inadequate.

We also urge that :

- o Every effort be made to restore HFIR to full operational status as soon as possible.

(4) Environmental Sciences

■ The environmental restoration effort envisioned by DOE will involve staggering costs unless improved clean-up technologies are developed. A focussed, long-range research program is essential if this requirement is to be realized.

■ Although OHER has defined an appropriate research program, we believe that OBES has much to contribute to that effort.

RECOMMENDATIONS

o OBES should define and implement a strategy of basic environmental research in close collaboration with OHER. In prioritizing research objectives, strong consideration should be given to the relative magnitudes of health effects associated with waste sites being targeted for restoration.

(5) Geosciences

■ The Geosciences program continues to be inadequately funded in relation to the fundamental geophysical and geochemical challenges facing DOE in all of its mission areas. Current research sponsored by OBES is of high quality, but the FY90 budget of \$16.4M is not sufficient to address all of the time-urgent fundamental issues. The program also suffers a serious imbalance, with a disproportionately large fraction (nearly 40%) devoted to acoustic imaging research.

■ OBES has been unable to implement earlier BESAC recommendations for new basic research on underground fluids. A critical need continues to exist for an expanded effort in this area.

RECOMMENDATIONS

o The Geosciences budget should be increased to provide an adequate level of support for DOE's efforts in environmental restoration, energy, and other topics of national importance.

o Research on underground fluids should be expanded (by \$5M, initially), with increasing emphasis on mass transport, energy transport and chemical reactions in the deep and shallow subsurface.

o The physics and geology of fractured rock should receive special attention, including porous sediment, and the identification and kinematics of chemical reactions, especially those that effect permeability and porosity.

o Studies of the scaling problem of fluid dispersivity should be supported in the field by monitoring existing wells and by new scientific drilling.

- o **Research on advanced imaging techniques should place greater emphasis on the relatively shallow subsurface.**

(6) Applied Mathematical Sciences

■ During the past 40 years, DOE and predecessor agencies have played a dominant role in the development of super computing. However, in spite of enormous increases in computing power, some problems of national importance still cannot be effectively simulated. DOE programs are playing an important role in studies of advanced parallel architectures (both hardware and software), and in development of associated application codes.

■ We judge the OBES program in advanced computing to be of excellent quality. A number of significant accomplishments are attributable to this program. The recently published OSTP implementation plan for the "Federal High-Performance Computing Program" offers DOE an opportunity for greatly accelerating its rate of progress. The plan has four components:

- o High-performance computing systems
- o Advanced software technology and algorithms
- o The National Research and Education Network
- o Basic research and human resources.

Many of the proposals in the implementation plan build upon the expertise and research efforts of the DOE program.

RECOMMENDATIONS

- o **OBES should aggressively pursue a leading role in the High-Performance Computing (HPC) initiative. A funding level of at least \$30M during the first year appears appropriate for the recommended DOE role.**

- o **OBES should take advantage of existing computational resources, both conventional and experimental, to solve relevant Grand Challenge problems in suitable OBES disciplines.**
- o **The AMS program should consider increased capital equipment funding for new, robust parallel-architecture computers for research and development on software, algorithms, and applications.**
- o **Supporting the aims of the 1989 OSTP Report, the DOE should develop an educational and training initiative aimed at all aspects of computational science.**

(7) Basic Engineering Research
&
Advanced Energy Projects

■ These programs were reviewed by a single subcommittee. BESAC continues to be impressed with the high quality of the individual projects, and with successes in transferring important results to industry and other users. Both programs are very well managed, with impressive results.

■ The Engineering Research program supports a key part of the OBES mission, creating generic engineering results that are essential to the design of future systems and for the safe and efficient operation of existing systems. A particularly pressing need exists, over the next several years, for basic engineering research relevant to the minimization, processing and cost-effective clean up of hazardous waste. Bioprocessing is a potentially attractive approach to many environmental remediation problems, but is still relatively undeveloped.

■ We are pleased to see that the initiative in Multiphase and Porous Media Flow is proceeding within the current budget recommendations. Outstanding proposals exist, but additional funding is needed.

■ The Advanced Energy Projects program has acted on the 1988 BESAC recommendations to concentrate on high-risk, high-potential-payoff ideas evolving from basic research, and to enforce a three-year funding cycle.

RECOMMENDATIONS

o OBES should mount a research initiative in bioprocessing of fuels and energy-related wastes. The effort should be coordinated with other relevant DOE offices, such as OHER. New funds should be identified for this purpose beginning in FY92. We estimate that the OBES portion of initiative will require approximately \$5M.

o Additional funding is recommended for multiphase and porous media flow studies as resources become available.

o The core effort in the Advanced Energy Projects program should be continued at its present level of funding, adjusted for inflation. The three-year funding limitation should continue, except in very exceptional cases (e.g. to permit some continuity of effort where a subsequent source of funding has already been identified).

(8) Energy Biosciences

■ The research supported by this program is of uniformly high quality. There exists a serious disparity, however, between the number of new proposals that can be funded under the prevailing tight budget constraints and the biological research opportunities which exist for supporting energy conservation and renewable energy resource development.

RECOMMENDATIONS

o The annual funding level should be increased to at least \$40 million in the near term.

- o Priority should be given to support of individual investigator-initiated projects, rather than to research centers and fellowship programs.
- o In funding new work, projects fundamental to bioremediation should be emphasized.

(9) Physics

■ As already discussed in the Overview section of this report, BESAC organized an *ad hoc* effort during 1989 to examine the balance and overall adequacy of physics research supported by OBES. Substantial omissions in DOE mission-related areas were identified.

RECOMMENDATIONS

o OBES should expand the scope of its physics-related research by organizing an appropriate basic research program in plasma and optical sciences (at a steady-state level of approximately \$10M, each), and by increasing its support of atomic physics. These efforts should be well integrated.



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Department of Energy
Washington, DC 20585

APPENDIX 1

MAR 27 1989

Dr. Albert Narath
AT&T Bell Laboratories
Whippany Road
Whippany, NJ 07981

Dear Dr. Narath:

I appreciate your willingness to assist the Department by Chairing the Basic Energy Sciences Advisory Committee (BESAC) on an interim basis. I want to outline for you and the Committee the charge for this year's activities.

For the first two years of the committee's existence, the committee has taken a broad look at BES and pointed out its strengths and weaknesses. BESAC should continue to review the overall quality and direction of the BES program and bring to my attention noteworthy observations--either negative or positive. However, we also need your advice on new initiatives or thrusts within BES and within the various subprograms. Please examine carefully the activities underway in each of the subprograms and suggest improvements in the focus of each. Some of the new emphases or activities underway or being considered on which I would especially like to hear from you include: multiphase flow in engineering research, high resolution underground imaging in geosciences, materials synthesis and processing in materials sciences, molecular sciences in chemical sciences, and x-ray lithography in advanced energy projects. These are some specific examples, but you may wish to add others if appropriate.

In your deliberations, a specific activity on which I request your advice and recommendations is the Historically Black Colleges and Universities (HBCU) research program support provided in response to the President's Executive Order. I would like to know how to ensure that the research is meritorious and competitive and also if alternative approaches could be used to obtain the highest quality research and to fully implement the Executive Order with regard to HBCU's.

We recognize the need to obtain additional funding which the BESAC has recommended. Nevertheless, we may be limited in funding over the next several years because of the generally tight Federal budget. Therefore, we must try to keep the BES program at the forefront of research despite the budget situation. Your opinion on the worthiness of various new initiatives would greatly assist us in planning the BES program.

I would like to have your report by December 15, 1989. I look forward to further discussion with you and the committee at your meeting on April 10.

Sincerely,

original signed by

Robert O. Hunter, Jr.
Director
Office of Energy Research

JUN 29 1989

Dr. Albert Narath
President
Sandia National Laboratory
P. O. Box 5800
Albuquerque, New Mexico 87185

Dear Dr. Narath:

The Department of Energy (DOE) is faced with major problems associated with nuclear and hazardous waste management and cleanup. The Secretary of Energy, Admiral James Watkins, has identified the strengthening of waste management activities, including environmental compliance and cleanup as one of his first priorities. In a letter to Congressional Committees (copy enclosed), Admiral Watkins stated that a five year cleanup plan would be developed to address problems in the near term. However, he also stated that a research program would be developed to address longer term improvements to waste handling and cleanup.

At my request a report is being prepared to define the problems and issues to address mid and long-range improvements to waste management and environmental restoration through advances in technology for remediation and cleanup. A draft planning document will be ready for distribution July 7, 1989. I would like the Basic Energy Sciences Advisory Committee to review this plan and provide recommendations and comments to me by August 4. The report will be distributed to the Committee on July 10 at the Basic Energy Sciences Advisory Committee meeting in Albuquerque.

The review of the problem definition for mid to long term research should:

1. Address the adequacy of the proposed problems to meet waste management and cleanup requirements, especially directed at environmental restoration.
2. Recommend any additions or modifications to the proposed problem definition including new or innovative approaches, that would significantly increase the value of the plan.
3. Determine whether the proposed priorities are appropriate and realistic for mid to long term needs of the Department.
4. Examine whether the proposed plan appropriately complements the Department's near-term cleanup, demonstration, and research and development activities.

I recognize that the time frame for the review of this plan is very short but the demands of the Department are such that a finalized draft is needed in August. Staff of OER are available to assist you in any way to facilitate the review.

This plan is important to the mission of the Department and your help in making this a scientifically strong document is very much appreciated.

Sincerely,

A handwritten signature in black ink, appearing to read "R. Hunter, Jr.", written in a cursive style.

Robert O. Hunter, Jr.
Director
Office of Energy Research

Enclosure

A. Narath

President

Sandia National Laboratories

Albuquerque, New Mexico 87185

August 2, 1989

Dr. Robert O. Hunter, Jr.
Director, Energy Research
Department of Energy
1000 Independence Avenue, SW
Washington, D.C. 20585

~~TO~~
Dear ~~Dr.~~ Hunter:

This letter is a preliminary response to your request to BESAC for comments on the draft ER plan for intermediate and long-term research on environmental restoration. The Environmental Subcommittee of BESAC is holding a meeting in Menlo Park, California, Thursday, August 3, to consider the plan in more detail, and to respond to a request from Don Stevens as to what is an appropriate role for BES in research on environmental restoration. A more carefully considered comment from BESAC will be forthcoming.

BESAC finds the draft research plan carefully thought out and well organized. The categories identified for research are, we believe, appropriate. However, these categories are only broadly defined. Further elaboration will be necessary to identify actual research topics.

Only two very general attempts to prioritize the research were made in the draft plan. Figure 2.4 is an attempt to prioritize on the basis of potential restoration cost reductions; figure 3.1 is an attempt to prioritize on the basis of major site problems. No comparison of the two prioritization schemes represented by these figures was made. More effort is needed in arriving at research priorities, unless sufficient funds are available to make such prioritization unnecessary.

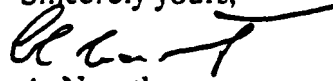
It is not clear to what extent DOE's environmental restoration problems are unique. Many of the contaminants may contain organic solvents and hydrocarbons which resemble the wastes encountered in other industries. It is our perception that hydrocarbons and solvents were underemphasized as waste forms in the present draft.

There are problems in utilizing new and novel technologies for waste restoration since the regulatory environment discourages innovation. For example, the EPA Inspector General has been critical of the EPA Superfund program for the use of SITE

technology in the cleanup of sites. Within DOE, pressure to take the safe route and utilize proven technology, rather than to employ any higher risk methods, is also present. Consideration must be given to how innovative, high risk technology, can be introduced into an activity where all concerned want to be "safe".

It is our judgement that environmental restoration is a major concern for DOE which will persist well into the next century. The DOE estimate of the cost of "environmental remediation" exceeds \$60 billion; past experience with such efforts suggest that this is an underestimate. BESAC is confident that intermediate and long-term research can improve the remediation technology and reduce the ultimate costs. In BESAC's considered judgement, Basic Energy Science (BES) has much to contribute to this research effort and its participation in the proposed program is therefore essential.

Sincerely yours,



A. Narath
BESAC Chairman

Distribution:
BESAC
Don Stevens, DOE

APPENDIX 4**REPORT OF BESAC ENVIRONMENTAL SUBCOMMITTEE ON :
DRAFT ENVIRONMENTAL RESTORATION RESEARCH PLAN**

J. D. Bredehoeft, Chairperson

G. D. Hegeman

D. C. Hoffman

L. T. Silver

W. B. Travers

The Environmental Subcommittee of BESAC met in Menlo Park, California, August 3, 1989, to consider mid- and long-term DOE research for environmental restoration. In addition to select BESAC members the group included invited experts, the OBES program managers and Frank Wobber from OHER (see Attachment I). The meeting started with a briefing on contamination at Hanford; the problem at Hanford is thought to typify problems at other DOE facilities.

A decision was made at Hanford to utilize the thick unsaturated soil zone above the water table as a medium for the disposal of chemical wastes. The thinking in the 1940's and 50's, when this practice was initiated, was that the "field capacity" of the dry soil in the area would hold and immobilize the dissolved wastes. Today we find that both moisture and the wastes gradually move downward to the water table where they are transported laterally by the moving ground water toward the Columbia River, where it ultimately discharges into the river. A long-term stream pollution problem has been created. Twenty million gallons of waste are produced daily. Something of the order of 100 cubic miles of soil are now contaminated. This results in an enormous and expensive clean-up problem.

I. **BACKGROUND**

The Nature of the Problem

Earth materials are extremely heterogeneous in their physical properties. This heterogeneity exists at all scales from centimeters to tens of kilometers. The permeability is especially heterogeneous; in a single ten centimeter rock core it can vary by several orders of

magnitude. Typical reservoir rocks (aquifers, in ground water parlance) have a permeability variation of three to perhaps five orders of magnitude. Less permeable cap rocks (or confining layers) typically have permeabilities five, six or more orders of magnitude lower than the reservoir rocks. It is not unusual to see eight or ten orders of magnitude variation in permeability within several meters of typical geologic materials.

This natural heterogeneity introduces a variety of complexities into fluid transport within the earth. A natural mixing, called hydrodynamic dispersion, is introduced into the movement of chemical constituents whether they are tracers, contaminants or hydrocarbons. The dispersion grows as the contaminant encounters more of the natural heterogeneity. Complex short circuits are formed; some of the tracer gets out in front of the centroid of mass and some trails well behind the centroid. Commonly the dispersion in a natural deposit is several orders of magnitude larger than that observed in the laboratory.

The fact that earth materials vary widely also impacts their chemistry. For example, fine grained deposits generally contain silts and clays which have a much larger exchange capacity than do the coarser grained deposits. Sorption occurs preferentially in the fine grained deposits. The mineralogy of the rocks controls the chemical interaction with chemicals in the fluids.

The unsaturated soil zone is even more complex, in that the hydraulic conductivity of the soil is a strong function of the moisture content. Moisture transport in an unsaturated soil is a highly non-linear process.

Implications of Heterogeneity

Earth materials, because of their large variation in physical properties, pose a special problem in analysis. One problem is that the dispersion grows as heterogeneities at varying scales are encountered. In the currently accepted theory, the parameter which describes the ability of the rock to disperse, the dispersivity, also grows as the transport distance increases. For example, if one is interested in 10 meters of transport, the dispersivity has one value; if one is interested in 10 kilometers of transport the dispersivity almost always has a much larger value. One is not certain how to extend the values from the small to the larger scale. Thus, if we are interested in large scale transport, in order to obtain appropriate values for dispersivity we must measure the dispersivity at the large scale; we cannot run a small scale test and determine a meaningful value for the large scale. This leads to questions

concerning basic theory. The saving fact is that reservoir engineers and hydrologists have examined enough large scale transport problems to know the expected bounds of the dispersivity.

There are perhaps two widely different approaches to handling the geologic heterogeneity; these are:

- 1) to describe the heterogeneity at the particular site in question in detail using geophysics as the principal tool, or
- 2) to describe the heterogeneity in statistical terms, which in turn leads to predictions with statistical distributions, or confidence bounds, about the predictions.

It is the expectation of many investigators that new geophysical techniques, either borehole to borehole, or surface methods could lead to a mapping resolution of physical earth properties of the order of a meter or so at depths of perhaps a kilometer. The problem of shallow geophysical imaging is made more complex above the water table in soils of varying moisture content.

Chemistry

Transport in the earth of multiple reacting chemical components is complex. Existing theory has been borrowed from chemical engineering. The theory, especially for multi-components, needs additional development and checking against field performance. The mobility of many chemical species depends upon their ionization state; this is particularly true for many of the heavy metals contained in the wastes. In many instances the basic chemistry of many of these metals as they exist in in the natural environment is not well understood. In addition there is the problem that the chemical state may be altered by the process of sampling and removal from the rock. Ideally, one would like the capability to do *in-situ* chemical analysis by some form of borehole logging; such techniques are possible, given sufficient research. In most contaminant problems, the *in-situ* rates of chemical reaction are important; the kinetics of many of the reactions of interest, however, are poorly understood.

Biochemistry

Many of the contaminants of concern are organic compounds. We are finding that most of these are degraded in-situ by natural micro-organisms which exist both in unsaturated soil and in the saturated zone below the water table. Natural micro-organisms have been isolated in groundwater to depths approaching a kilometer. These organisms appear to be able to adapt their metabolism to a variety of potential food sources. For example, creosote and pentachlorophenol have been observed to be degraded by colonies of several species of organisms. One species provides a degraded product which the next can live on, etc. Hopefully, in the end the ultimate product is benign.

If we are to make a major breakthrough in the cost of environmental restoration it will most likely be a technology for in-situ treatment. The microbiology approach looks very promising. One can stimulate the natural organisms through inoculation followed by fertilization. In the case of aerobic organisms one may also need to provide oxygen. The biologists say that organisms can be safely bio-engineered to specifically degrade the wastes of concern.

Heavy metals can also be concentrated biologically. For example, selenium at Kesterson Reservoir was concentrated in the bottom of the ponds biologically. It may be possible to utilize micro-organisms to either mobilize or stabilize heavy metals in-situ.

Research Strategy

Research into the fundamental processes involved in environmental contamination must be closely tied to actual field investigations. The earth is so different in its makeup that in the final analysis the earth itself is the laboratory. This changes the nature of basic research.

The consultants at the meeting with clean-up experience made the point that test site restoration is an integral part of the characterization process. Stated another way, often much more is learned about site characterization during the process of restoration than was learned during the entire characterization phase. One needs the actual stress on the system to see how it responds in order to do an adequate job of characterization.

This statement regarding stressing the system carries over to the research. The research needs a close tie to the field and the actual process of restoration. This will make basic research more difficult to define and manage; the line between basic and applied is not nearly so clearly defined. The experts at Menlo Park were unanimous, as well as insistent, that good research will require this close collaboration.

The earth is expensive to investigate because it ultimately requires some drilling. For the geologist or reservoir engineer (or hydrologist) the drilling rig is the one key tool of his investigation. One way to maximize the research effort is to coordinate site investigations at sites that are being restored. Drilling for research can often be "piggy-backed" with drilling for restoration. Provided there is good coordination, research and restoration can go on hand-in-hand.

The waste contamination problem involves numerous traditional disciplines: geology, physics, chemistry, biology, mathematics and engineering. The interesting research areas are truly interdisciplinary (not multidisciplinary) in the areas between traditional disciplines. It is a problem and a challenge, how to create true interdisciplinary collaboration.

II. COMMENTS ON THE DRAFT REPORT:

EVALUATION OF MID- TO LONG-TERM BASIC RESEARCH FOR ENVIRONMENTAL RESTORATION

The group agreed that the outline of research needs contained in the report was a useful way to organize the research and captured the essence of the problem. Five major areas for research are identified in the report:

1. **Fundamental knowledge of natural systems;**
2. **Advanced sampling, characterization, and monitoring methods;**
3. **New remediation technologies;**
4. **Performance assessment;**
5. **Health and environmental effects of technology application.**

In each of these categories, research objectives and specific research needs are identified. The group was favorably impressed with both the objectives and the needs. The list of research needs must be fleshed out, however, before it is a research plan. The Menlo Park group had some ideas about specific research topics; these ideas are more appropriate for future documents in which the specific areas of research are elaborated in more detail.

Priorities

There are two attempts in the draft report to prioritize the research. Figure 2.4 prioritizes the research based upon its potential for cost savings. Table 3.1 prioritizes on the basis of major site problems. No attempt is made in the report to compare the two prioritization schemes. Our analysis of these two prioritizations suggests that they compare favorably, an observation which should be included in the report. Contaminated environments are the area of great concern where the potential cost savings are highest. This, we believe, translates into better *in situ* technologies for restoration. If wastes can be treated in place, it will greatly enhance the restoration possibilities. The biologically based technologies have great promise in this area.

Health Effects

In both prioritization attempts, health effects are either intermediate or low in priority. We believe this is a mistake. We are ultimately concerned about the effects of environmental contamination on humans. This is an area in which our knowledge is certainly incomplete. Little is known about the effects of low-level dosages of many toxic chemicals for long periods of time. The health effects of many of the ubiquitous organic compounds have not been studied in detail. We believe health effects should have a high priority in the final research effort.

It was the group's perception that many of the environmental standards are set on the basis of detection limits rather than any real impact on human beings. The current limits are further driven by a philosophy of zero risk. There must be some effort, given high priority, to change this philosophy. A better knowledge of the real risk to humans would certainly help.

Cross-Cutting Technology : Isotope Geochemistry

There are a few areas of research with sufficiently broad interest that they do not easily fit into the 5 categories outlined in the mid- to long-term research document. One of these is isotope geochemistry. In some sense it is a supporting technology. However, its utilization in the research can be so powerful, with sufficiently wide ramifications, that we believe it is worth highlighting separately.

The Department of Energy has a powerful and unique resource in the form of isotope mass spectrometry facilities. These are unmatched in capability and support infrastructure by anything available in academia, private institutions, or other government agencies. At present, many of these facilities are directed toward, and supported by the nuclear-energy and nuclear-weapons test programs. These capabilities represent opportunities for substantial new contributions to basic research.

Isotope geochemistry will contribute observational support for studies of active fluid-rock systems. Understanding fluid-rock systems involves obtaining answers to questions such as:

- (a) What is the permeability, in space and time, of the rock matrix?
- (b) How fast does the fluid move through the rock?
- (c) How fast do dissolved contaminants move through the system?
- (d) What are the sources and flow directions of the fluid?
- (e) Does the fluid react with the rock medium, and at what rate?

Isotopic measurements of rock materials and fluids can yield answers to these questions in many natural situations. For example, oxygen isotopes can be used to determine the sources of fluids; isotopes of Sr and Pb can be used to determine the sources of dissolved constituents and the rate of chemical reaction between the rock medium and the fluids; and short-lived cosmogenic isotopes can yield the age of the waters and therefore the fluid transit time in the reservoir. In complex hydrological systems these determinations might require very high precision isotopic ratio measurements or difficult large dynamic-range ratio measurements, and relatively large numbers of analyses. The combination of specialized equipment and the capability for rapid analytical throughput, both of which are or could be available, make the isotope geochemistry facilities of the DOE laboratories a unique national resource that can be focused on these difficult but timely problems.

The combined facilities of the laboratories will allow the use of a wide range of isotopes as tracers in an active fluid/rock system. Immediate contributions would be to better understand the hydrologic processes operating at the DOE sites earmarked for waste isolation and/or cleanup. The particular value of isotopic measurements lies in using rare isotopes in the environment to field-test models of contaminant transport *in situ*.

The DOE facilities provide unique opportunities to study contaminants. In many instances radio-isotopes would provide ideal tracers for *in-situ* experiments; however, they are almost never utilized because of excessive environmental concerns. Many of the DOE wastes, especially the radioactive components, provide ideal tracers, which can provide a time history as well as the paths of contaminants. The OBES Geoscience Program has initiated a new effort in the use of isotopes for geologic problems; this initiative has great promise for research into environmental restoration.

Management Concerns

BESAC unanimously finds that if DOE is to mount a mid- to long-term research program in environmental restoration it is best done in Energy Research (ER) with its long-

standing tradition of research. It was obvious at the Menlo Park meeting, however, that there are some concerns within OBES about their own participation. The OBES managers were very cautious in identifying environmental research opportunities, presumably for fear that it might cause mandated reductions in other areas, as may occur in FY90 with \$9 million of Materials Sciences and Chemical Sciences funding being targeted for reprogramming for "environmental" activities. It is obvious that OBES should get more involved in research on environmental problems. OBES must be convinced that there is, within DOE, long-term, balanced support for such an effort.

Within OBES, some of the ongoing research has high priority for the environmental restoration. Among the research areas are:

- o Underground imaging,
- o Isotope geochemistry,
- o Microbiology applied to *in-situ* restoration,
- o Two phase transport in the subsurface,
- o Heavy element chemistry, at conditions expected in the natural environment,
- o Surface chemistry,
- o Use of computers to solve problems of subsurface transport.

The list could be made longer; however, it is adequate for making the point that OBES has much to contribute to the research effort in this area. We have outlined our suggested research agenda for OBES in Attachment II.

Frank Wobber, in OHER, has nurtured and built a program of research into problems of groundwater contamination. He now sees an opportunity for his program to increase, perhaps dramatically. As a good manager he has made plans for how to increase his program meaningfully. We applaud OHER's efforts.

It is not clear whether OHER's plans include additional support for activities in OBES. We believe some fraction of any significant increase of effort in environmental restoration within the Department should go to OBES. The challenge for management is how to bring the two groups together to build a meaningful joint program.

Summary

DOE has a long-term environmental restoration problem. Even with expenditures as large as \$600 million per year, it will require a century to complete the \$60 billion effort the Department envisions. Long-term research aimed at a better understanding of the relevant fundamental issues, leading to improved technologies for clean-up, is imperative.

The research is different in nature than traditional basic-research investigations. For research to be useful to environmental restoration, it must be closely tied to real field problems, especially at sites where restoration is going on. One needs the actual stress of the restoration process to ensure that the characterization is correct. Some of the contaminants make good tracers. Much of the actual contaminant movement may be viewed in terms of tracer experiments. The contaminated sites become field laboratories in which to test hypotheses. One idea is to piggy-back basic research with actual restoration. This approach of coupling the field to the basic research blurs the distinction between basic and applied research and may require a change in philosophy of research management.

It is obvious that OHER has planned a useful research program. Given its background and experience, OBES could contribute significantly to the effort in selected areas. There needs to be closer collaboration between OBES and OHER if ER is to make a maximum impact on the enormous restoration task that lies ahead.

Attachment I : Attendees at DOE Environmental Cleanup Meeting, August 3, 1989

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Attachment II : Recommended Environmental Research Agenda for OBES**BIOSCIENCE**

- o Methods of *in-situ* biologic treatment of a variety of organic and inorganic wastes.

CHEMISTRY

- o Separation techniques and analytical methods for environmental contaminants including methods for *in-situ* analysis
- o Heavy-element chemistry under environmental conditions
- o Surface chemistry

ENGINEERING

- o Two-phase subsurface transport of multicomponent fluids
- o Theory of systems far from equilibrium applied to multicomponent transport

GEOSCIENCE

- o Isotope geochemistry applied to problems of contaminant movement
- o Studies of permeability distribution including underground imaging of contaminated sites
- o Increased field investigations
- o Expanded program of organic and inorganic geochemistry

MATERIALS SCIENCE

- o Increased effort in stochastic partial differential equations applied to problems of contaminant transport

MATHEMATICS

- o Improved numerical methods for the solution of the multicomponent transport equations, including parallel processing

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October 23, 1989

Dr. Robert O. Hunter, Jr.
Director
Office of Energy Research
U.S. Department of Energy
Washington, D.C. 20585

Dear Dr. Hunter:

In your charge to BESAC dated March 27, 1989, you asked the Committee specifically to address various net initiatives and thrusts within BES, one of which was the x-ray lithography initiative in advanced energy projects.

This task was assigned by BESAC to its subcommittee on Engineering and Advanced Energy Projects, which I chair. The subcommittee established a panel on x-ray lithography which met in Washington on October 5, 1989. Three members of BESAC participated in the meeting. In addition, members of the subcommittee, the program director of advanced energy projects, representatives of DARPA, IBM, Los Alamos, LLNL, BNL, ANL, SEMATECH, NRL, Princeton University and Louisiana State University were present. Dr. Stevens also attended.

Based on the presentations and discussions held during this meeting, together with further inquiries, the panel has established certain findings and is in the process of incorporating them into its report. These findings were reviewed by BESAC at its October meeting and endorsed pending review of the written report. These findings are summarized in the attachment to this letter.

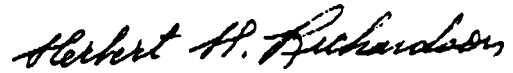
In light of pressures that appear to exist in the DOE to quickly issue an RFP for the design and construction of two competitive synchrotron x-ray sources to be utilized by U.S. manufacturing firms for submicron chip manufacture, BESAC has directed that we advise you not to proceed with an RFP at this time without additional planning or coordination with other U.S. efforts.

As a result of our findings, we recommend that the DOE first engage in a planning effort to define a program that considers all elements of the manufacturing system, includes DARPA and the semiconductor industry in the planning process, takes into account the various workshops that have been held on this topic, and recognizes that the light source is only one part of a much larger investment that must be made to realize competitive U.S. manufacture of chips with feature size of less than 0.35 microns.

Efforts by DOE and other federal agencies, as well as by industry to accelerate the U.S. development of x-ray lithography for manufacture of submicron chips are strongly endorsed by BESAC. However, proceeding with the manufacture of light sources alone, without integration into a total system is not likely to produce the desired results.

The BESAC is also concerned that the proposed initiative will place additional stress on the BES budget, especially on the programs for advanced light source development that made the proposed initiative possible.

Sincerely yours,



Herbert H. Richardson
Chairman
Subcommittee on Engineering
Research and Advanced
Energy Projects

BESAC PANEL ON ENGINEERING RESEARCH AND ADVANCED ENERGY PROJECTS

Panel on X-Ray Lithography

FINDINGS

1. To encourage acceleration of the U.S. effort in submicron chip manufacture is important to the nation. Synchrotron radiation offers the most attractive short term options for a commercially practical light source for production of electronic chips with linewidths less than 0.35 microns.

2. The light source, although important, represents only a small portion of the total investment necessary for commercial chip manufacture. In the U.S. only IBM, as a single organization, has the R&D program and financial resources to undertake the total development process, and is doing so with its own funds. Perhaps SEMATECH or other industry consortia could mount a program to utilize a DOE-developed light source, but the panel suspects this is not likely.

3. The panel believes that a program to develop a small industrial synchrotron would demonstrate DOE technology transfer. Synchrotrons have been used as x-ray sources for applications in microelectronics, microsensors, and biological studies. Practical, small synchrotrons are essential for such applications to be industrially feasible, and might enable small-scale synchrotron research to be undertaken in many more university laboratories. Japan has realized this and is building a host of such machines. If the U.S. is to be competitive in these areas it is important that an industrial synchrotron be developed expeditiously in this country.

4. The panel believes that the concept of developing two machines for \$33 million is unrealistic, but is reasonable for the development of a single "warm" machine. There are several organizations and consortia fully capable of responding and successfully producing a "warm" machine.

5. The panel found no coordination of the proposed machine development with other major activities underway at BNL, IBM, DARPA, LSU and others. Before proceeding with development of an RFP for development of an industrial machine, a national plan for its utilization should be prepared by DOE in close coordination with DARPA. The initiative should proceed as follows:

- * Establish with certainty the ultimate home of the machine to result from the current Brookhaven effort; call this the BNL machine.

- * Ascertain which companies wish to utilize the BNL machine at a substantial level when it is in its "home" location.

- * Get U.S. industrial representatives, DOE lithography people and DARPA together to secure a projection for the need for a DOE-developed

industrial source for use in chip manufacture. This should include the prospects for securing industry matching funds for this development and for achieving the industrial investment needed for one or more complete chip fabrication facilities.

6. This program should not be funded at the expense of the BES budget, since it is basically a development project. The BES budget is already severely strained, both in terms of research support and research facilities operation and development. DOE should consider securing matching funds from state and industrial sources, and look to the more development-oriented parts of the Department for the needed federal share.

7. The panel feels that the practical solution for an industrial synchrotron in the short term may lie in a "warm" rather than a cryogenic machine. While the cryogenic machine has advantages in footprint and overall size, these advantages are more than offset by the difficulty in development and in operation of the cryogenic system. The "warm" machine is well developed and an industrial machine development is entirely feasible within the present state of the art.

APPENDIX 6**REPORT ON
HUMAN RESOURCES****H.H.Richardson****I. BACKGROUND**

The Committee was asked to provide advice on how to improve the role of minorities and Historically Black Colleges and Universities (HBCU) through interactions with OBES and other related DOE programs. Of particular concern was identifying strategies to ensure that research proposed by minorities and HBCU's is meritorious and competitive. This task was initiated, but could not be completed within the time frame of the 1989 report. Preliminary observations and several recommendations have been made based on the study to date.

II. FINDINGS

The DOE sponsors a number of programs to strengthen HBCU's and to encourage minorities to enter and remain in science and engineering careers. For example, several of the National Laboratories have collaborative programs with single or multiple (i.e. through consortia) black colleges, and sponsor a variety of programs through which research staff of the laboratories host minority students, faculty and other professionals. Within OBES, the success rate of black colleges and minorities in peer-reviewed competitive programs has been notably low. Thus, there is no significant flow of OBES competitive research funding to minorities or to HBCU's.

The Committee found that programs in minority science and engineering were decentralized and uncoordinated within DOE. In its investigation to date, the Committee did not find a coordinated activity addressing the need to increase the participation of minorities and HBCU's in basic energy science and engineering research and education. Within the Department as a whole, a coherent strategy for increasing and strengthening the pool of

minority professionals at all levels does not seem to exist. This is a need that transcends the issues of HBCU's alone.

The Committee found that the National Science Foundation (NSF) and the National Aeronautics and Space Administration (NASA) have aggressive, well-coordinated programs to help strengthen minority and HBCU participation in education and research in science and engineering. The NASA Minority University Program, administered from NASA Headquarters, affords one of the premier examples of a well-coordinated federal minority program. Its goal is to increase the ability of HBCU's to participate in research programs, and also to increase the numbers of non-HBCU minority students and faculty participating in NASA research and education programs in science and engineering. A related goal is to enhance alliances with private sector institutions in order to enhance minority participation in education and research in science and engineering.

III. RECOMMENDATIONS

Based on our investigation to date, we recommend that:

1. The Office of Energy Research (or the Office of the Secretary) establish a Minority University Research and Education Program, with a manager reporting to the Director of Energy Research (or appropriate official in the Office of the Secretary). This program should be supported with seed and operating funds, as well as funds to match and stimulate funding from the DOE Laboratories, and given responsibility for developing and coordinating a DOE-wide activity aimed at increasing the amount and quality of minority representation in the research community relevant to energy...including research faculty at HBCU's and non-HBCU institutions. Further, the program should address all under-represented minorities, not only blacks.
2. DOE study and adopt, where appropriate, the models afforded by NASA and the NSF. Further, DOE should participate actively in cooperative and collaborative efforts among other federal agencies, the President's Commission on Historically Black Colleges, state and local entities, and private sector institutions.
3. The BESAC be requested to continue its study, and recommend strategies for increasing the quality and supply of qualified engineers, scientists and educators in basic energy

research and education at all levels, including women, minorities, and the cohort as a whole.

4. The Office of Energy Research should consider establishing a formal partnership program involving major research universities and smaller institutions that have primarily (or at least significant) minority student populations in science and engineering. These partnerships should be funded at least in part by DOE and have access to the resources of the DOE laboratories. The objectives of such a program should include establishing working relationships between the educational institutions that encourage and mentor minority students and minority institution faculty, thus providing encouragement and "on-the-job" training for young students and research faculty.

APPENDIX 7

REPORT OF THE BESAC *AD HOC* SUBCOMMITTEE ON PHYSICS IN OBES

F. Y. Fradin
J. Macek, Chairperson

I. BACKGROUND

The BESAC ad hoc Subcommittee on Physics in OBES was augmented in May and the membership list is attached. It held a two-day meeting at the Germantown DOE site to review the scope of physics-related research in OBES and to prepare its recommendations to BESAC. A copy of the agenda for this meeting is attached. The committee also studied research summaries of the relevant OBES programs. The particular charge of the subcommittee was to identify physics-related research areas relevant to the DOE mission that are under-represented in, or effectively precluded by, the present organizational structure. The committee did not examine optics-related research at major facilities since that work is machine-specific, although the committee recognizes that some of this work may have broader significance.

II. FINDINGS

Physics-related research is supported in a variety of OBES divisions. Condensed matter physics is supported in the Materials Science Division; atomic and chemical physics, in Chemical Sciences; laser research, in Advanced Energy Projects; and Geophysics, in Geosciences. In addition, an initiative in plasma physics has been proposed, but not yet implemented, in the Atomic Physics Program of Chemical Sciences.

The subcommittee found that two areas of basic research pertinent to the DOE mission, namely plasma physics and optical and laser physics, were badly under-represented. Basic plasma physics, which is pertinent to advanced energy production and particularly to magnetic fusion, has traditionally been supported by the magnetic fusion division of ER. However, this program has moved from an emphasis on basic research to machine specific engineering design. In the words of Dr. David Crandall, Director of

Applied Plasma Physics Division, Office of Fusion Energy, "Support for plasma physics research and atomic physics that is not projected to affect machine design within a time frame of three years is precluded." This concentration inevitably means that the basic physics relevant to alternative concepts is in grave danger of drying up. The proposed initiative in the Atomic Physics Program seems to reflect an awareness in OBES of this crisis.

Plasma research relevant to fusion concepts deals mainly with the subject of fully ionized plasmas. Inertial confinement fusion and other projects in Defense Programs deal with the interaction of intense, short-duration pulses of electromagnetic and ion-beam radiation with matter. Here, it is the physics of partially ionized plasmas and the atomic physics of ionic species that underlie these technological developments. The present atomic physics program emphasizes the physics of ionic species but basic research in partially ionized plasmas *per se* is not presently represented.

Research in optical physics outside of major facilities is also badly under-represented in OBES. Projects in materials research deal with the response of materials to electromagnetic radiation but do not address basic research in the broader aspects of the subject. Four projects totalling \$250K in the atomic physics program address some issues of quantum optics fundamental to laser operation. Optical physics projects totaling \$3M in areas such as x-ray laser devices and x-ray imaging are supported in the Advanced Energy Projects Division under the "infants and orphans" umbrella. The committee finds that these projects, taken collectively, fall far short of adequately addressing the scientific needs of DOE in the optical physics area. The short-term project-oriented structure of the present AEP Division is not an adequate framework for basic long-term research.

III. RECOMMENDATIONS

OBES should expand the scope of its physics-related research by organizing an appropriate basic research program in atomic, optical and plasma sciences, as outlined below:

1. Basic research on fully ionized plasmas relevant to magnetic fusion, and on partially ionized plasmas relevant to the interaction of intense laser and ion-beam radiation with matter, should be put in place. Plasma physics is of

unquestionable importance to DOE. Some DOE missions in which plasma physics is of central importance include:

- o Nuclear weapon design
- o Nuclear weapon effects simulation technology
- o Magnetic fusion
- o Inertial fusion
- o Discharge-based lighting
- o Semiconductor device manufacture
- o Surface cleaning
- o Deposition of wear- and corrosion-resistant coatings
- o Laser excitation
- o Pulse power components

A funding target of \$10M is appropriate to initiate this program.

2. A new program to provide for basic optical physics research should be undertaken. This research will have a major impact upon the drive towards development of:

- o Short wavelength lasers
- o X-ray imaging
- o Characterization of intense coherent radiation
- o New spectroscopic techniques for plasma diagnostics and sensitive detection of atoms
- o Nonlinear optics in solids and vapors (new ways to generate coherent light)
- o Adaptive and holographic optics (new capabilities in the manipulation of light)

These areas of optical physics are central to the DOE mission, technology transfer, and industrial competitiveness. A funding goal of \$10M is appropriate for this initiative.

The present OBES programs should be encouraged to continue development of innovative optics-related tools needed in their research.

3. The subcommittee recommends expanding the present Atomic Physics Program. The three areas, plasma, atomic, and optical physics, are closely linked and would form a well-integrated unit within OBES. The atomic

physics program should continue its current emphasis, but should also address the needs of Defense Programs and short wave-length laser research for an initiative in atomic theory. Such an initiative is needed to provide both an adequate base of expert knowledge and a supply of trained personnel proficient in the most advanced theoretical modelling of strongly interacting atomic species and ions in unusual environments. In addition, the subcommittee believes that the atomic physics program has been slow to take advantage of new technologies that have opened up new areas of research. This is true in the case of exotic species such as positrons, muons, and anti-hydrogen; in the use of synchrotron light, and in the development of ion-storage rings. Finally, the subcommittee also notes that funding in magnetic fusion for basic atomic physics has been nearly zeroed out, as research in that program has moved to engineering design. A funding increase to insure an adequate science base in this area is appropriate.

4. Basic research that would underlie innovative physics-based technology, specifically to enhance industrial competitiveness and technology transfer, constitutes another area that could be integrated into the proposed atomic, optical and plasma physics initiative.
5. The atomic, optical and plasma physics program should be managed as a well-integrated activity within OBES. Organizational changes to facilitate this objective should be explored.

ATTACHMENT:MEMBERS OF BESAC *ad hoc* SUBCOMMITTEE ON PHYSICS

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APPENDIX 8**REPORT OF BESAC SUBCOMMITTEE ON
FACILITIES - 1989**

F. Y. Fradin
D. E. Moncton
J. M. Rowe, Chairperson

I. BACKGROUND

For this year, the subcommittee was given no major new tasks, and has chosen to follow up on the recommendations made last year, and to observe the results. Although no special meetings were held, the subcommittee met in conjunction with two of the regular BESAC meetings to discuss issues and priorities. In addition, a letter was sent to the directors of all major DOE neutron facilities requesting that they organize a national coordinating committee.

II. FINDINGS

Our first recommendation from last year was that the Department of Energy provide sufficient operating funds to existing facilities so that they could be fully exploited, thus maximizing the capital invested in them. We note with approval that the Department did request such funds, and that OMB and the Congress did approve these increased funds for facility operation. However, the current highly uncertain budget situation may undo the good work, and make the situation even worse. The combination of a Gramm-Rudman sequester, along with congressionally mandated initiatives and the compact synchrotron project favored by the former Director of Energy Research now seem likely to reduce these increases, possibly even to zero. This is occurring at a time when the Department philosophy for research reactor operation is in turmoil, with the transfer of operating responsibility to Nuclear Energy (the mechanism for funding is not certain as this is written), and continuing problems at HFIR and new ones at HFBR. In these circumstances, it is imperative that the pulsed neutron sources, IPNS and LANSCE, be fully funded so as to provide neutron scattering for U.S. researchers. However, under the current funding plan,

both sources will be required to reduce operations. SSRL has now reached an agreement for full dedicated use of the SPEAR ring, only to see that the necessary funds may not be available.

In our second recommendation, we recommended that the Department proceed to build a new generation of synchrotron and neutron facilities. We note with approval that the construction funds for the APS have been approved for FY 90, so that this project can move forward. We are extremely disappointed that the Department did not give approval to the initiation of line item funding for the R&D for the ANS. This is a national goal, and has been recommended with very high priority by every panel that has looked at it. However, the R&D needs are simply too large to be taken from the normal operating budget of OBES. The National Academy of Sciences, in their report entitled "Safety Issues at the DOE Test and Research Reactors" has stated that DOE should formalize plans for the replacement of its Class A research reactors. However, without the R&D proposed for the ANS project, it is not possible to define a project for such replacement. If the ANS R&D does not proceed expeditiously, it is quite certain that DOE will eventually be in a position where it will no longer operate any research reactors. We therefore strengthen our recommendation from last year.

In our third recommendation, we supported the upgrade of the HFBR beam lines and instruments, as an interim measure until ANS came on line, and to help develop concepts for the ANS instrumentation. We note that this recommendation was not implemented, and further, that the HFBR is now shut down for safety reviews.

We note that the PSR, a storage ring that serves LANSCE, has shown marked improvement in operation this past year, having achieved 65 microamps at one stage, and 80% reliability, while working to achieve full design current. We note again that LANSCE is a parasitic operation, which can only be cost-effective with the continued support of the DOE Nuclear Physics and Weapons programs. Finally, we urged that a National Coordinating Committee for Neutron Scattering be established to provide continuing advice on priorities for the field. The first steps in setting up this committee have been taken, and the committee should be in place by early 1990. We urge the synchrotron community to take a similar step in the coming year.

In summary, we are pleased that OBES has responded positively to most of our recommendations. However, for various reasons, virtually all of our recommendations need to be reiterated for this year, and even strengthened.

III. RECOMMENDATIONS

1. We recommend that the Department provide sufficient funds to OBES to ensure that existing facilities are optimally operated.
2. We recommend that the Department respond to the national need for new research reactor facilities, and establish the ANS R&D program as a line item immediately.
3. We recommend that the existing reactors be restarted as soon as is consistent with safety, and that the HFBR upgrade be initiated, in order to ensure that the Department will continue to have a role in research reactor facilities.

APPENDIX 9**REPORT OF BESAC SUBCOMMITTEE ON
MATERIALS SCIENCES - 1989**

C. P. Flynn, Chairperson

F. Y. Fradin

D. E. Moncton

K. C. Taylor

I. BACKGROUND

During this year, the Subcommittee did not hold formal meetings. Discussions took place mainly at full BESAC meetings. The observations of the Subcommittee follow.

II. FINDINGS

This program remains much as outlined in the 1988 BESAC report. It is broad and well-managed in its efforts to underpin the diverse and rapidly-advancing science of materials. The DOE Materials Program is a critical component of U.S. basic research in materials, which impacts not only on the energy mission of OBES, but also on the nation's defense and its economic health. Budgetary constraints have prevented all but modest evolution of the program over the past year. Those changes that have taken place are consistent with the priorities of the division and the community, which also conform with the views expressed in earlier BESAC reports, e.g., the enhanced programs on high T_c superconductors is noted. In particular, a continued and growing emphasis is placed on initiatives in the synthesis of novel materials and new configurations of materials, and on the processing of materials to achieve useful properties. The recent report of the NAS and NAE study on Materials Science and Engineering was also strongly supportive of synthesis and processing initiatives to ensure a strong effort to improve this important enterprise for the future competitiveness of the U.S. Synthesis and processing is of clear importance for many technologies, e.g. integrated circuits, computer memories, high temperature superconductors, airframes, etc. This is an area where foreign competitors are very active.

The OBES Division of Materials Sciences has initiated new synthesis and processing programs in the following areas:

Major New Synthesis and Processing Projects

(Note: Each is about 500K/year)

FY 1988

High temperature superconductors--Ames, ANL, BNL, LANL, ORNL, AND SNL. About half this effort is synthesis and processing (6 projects).

FY 1989

Ceramic Epitaxial Films and Composites - ANL

Structural Ceramics - University of Illinois

Enzymatic Synthesis of Materials - LBL

Ceramic Composite Synthesis Using Biological Processes - PNL

FY 1990

Field Responsive Polymeric and Organometallic Materials - University of Illinois

Effect of Impurities, Flaws, and Inclusions on Adhesion and Bonding at Internal Interfaces - LLNL

FY 1991

Growth Mechanisms at heterointerfaces - LBL

Synthesis and Properties of Novel Ceramic Nanocomposite and Macromolecular Thin Films - ORNL

III. RECOMMENDATIONS

Recognizing the importance of materials research to the nation's economic health and the missions of DOE, we recommend that Materials Science funding levels should be increased generally above inflation.

APPENDIX 10**REPORT OF BESAC SUBCOMMITTEE ON
CHEMICAL SCIENCES - 1989**

D. C. Hoffman
J. Macek
K. C. Taylor
M. S. Wrighton, Chairperson

I. BACKGROUND

The Chemical Sciences Division (CSD) Subcommittee met as a group to discuss the report on October 20, 1989, in Washington, D.C. Members absent were contacted by phone for input.

The BESAC Chemical Sciences Subcommittee did a major review of the overall Chemical Sciences program in 1988. This year the Subcommittee chose to limit their review to the general funding picture for Chemical Sciences, progress and response to last year's report, and new proposals and initiatives.

The CSD program was reviewed for BESAC by Robert Marianelli at its April 10, 1989, meeting. The review included research directions, selected accomplishments, budget considerations, and proposed initiatives. A major objective of the Chemical Sciences Program is building the chemical knowledge base for advanced energy technologies. Chemical Sciences also serves the scientific community through its support for certain of DOE's unique user facilities. A long-standing objective of CSD, which is not being met due to the shutdown of HFIR, is to prepare and separate transuranium isotopes for research. The primary research areas funded by CSD include the following:

- o chemical reactivity
- o chemistry of materials precursors
- o chemical physics
- o atomic physics

- o chemical catalysis
- o organic coal chemistry
- o separations and analysis
- o chemical engineering sciences
- o photochemistry and radiation science
- o heavy element (actinide) chemistry
- o separations and analysis
- o chemical engineering sciences

II. FINDINGS

We were very impressed with the excellent quality of the CSD program and planned initiatives. The support of high quality frontier research should be vigorously maintained as the primary objective of the supported research program.

Status of Funding for Chemical Sciences

In its 1988 report the CSD Subcommittee recommended a \$12M increase in the FY90 CSD operating budget in order to restore its funding level to the 1981 research effort. The Subcommittee also recommended an additional \$13M for new initiatives. The total increase (\$25M) would have placed CSD's budget in line with the overall OBES operating budget increase during 1981-1989. These increases were not realized in the FY90 operating budget.

The FY90 CSD operating budget is not sufficient to fund the recommended program initiatives outlined in the 1988 BESAC report. Funds targeted for special programs have been seriously eroded by activities mandated by Congress which were not matched by increases to the budget.

Progress on Initiatives Recommended in 1988 Report by CSD

1. Combustion Research Facility (CRF)

The planned phase II initiative for the CRF at Sandia Livermore has still not been funded. After several consecutive years without funding, this initiative has now been supplemented by a new joint proposal for 1992 from SNL and LBL for a Combustion Dynamics Facility. The new proposal will emphasize LBL's effort on

fundamental aspects of combustion chemistry, including molecular level understanding of combustion reactions and SNL's effort on complementary basic and applied research, including the development of optical diagnostic techniques.

2. Large Einsteinium Activation Project (LEAP)

The LEAP initiative recommended in the 1988 report has not been started. This program is dependent on the restart of HFIR. The subcommittee is very concerned about the long delay in the restart of HFIR. We stand to lose critical expertise when an area of research is inoperable for so long. The transfer of responsibility for reactors to Nuclear Energy should not be allowed to further delay the restart of HFIR.

3. Molecular Science Research Center

The Environmental Molecular Sciences Laboratory at PNL will house the Molecular Sciences Research Center and support PNL as an Environmental Center of Excellence. Over the past few months several prominent scientists have joined the Center. Future success of the Center will depend on their continued ability to acquire superior staff and the commensurate development of a focused research program which builds on the talents of these people. The \$6M of new money which was requested in the 1990 budget for this initiative has been eroded by the requirement to fund other mandated projects.

4. Atomic Physics Storage Ring

The atomic physics storage ring initiative has been given thorough review and endorsement by the scientific community. Competing proposals have been reviewed. Two workshops are being held to examine future opportunities for research in atomic, molecular and optical sciences, one in November, 1989, at UCB and another in March, 1990, in Washington, D.C.

III. RECOMMENDATIONS

The CSD Subcommittee reiterates the following recommendations from last year:

1. Even in a level funding scenario for OBES as a whole, the CSD budget should be increased in real terms to restore it to the FY81 research effort.

2. New money should be added to the budget to support combustion research, which is central to DOE's mission.
3. Additional research support should be made available for atomic physics.
4. The CSD Subcommittee supports the research initiatives based on the Pimentel report and National Academy reports.
5. The CSD Subcommittee reiterates its 1988 observation that capital equipment allocations for single investigators are inadequate.

In addition, the CSD Subcommittee urges that every effort be made to restore HFIR to full operational status as soon as possible, consistent with all applicable safety requirements.

APPENDIX 11**REPORT OF BESAC SUBCOMMITTEE ON
GEOSCIENCES - 1989**

J. D. Bredehoeft
L. T. Silver
W. B. Travers, Chairperson

I. BACKGROUND

In FY90 the geosciences budget is \$16.4 million, of which \$3.6 million (22%) is for the new acoustical imaging initiative. This brings the total for underground imaging research to \$6.5 million or nearly 40% of the Geosciences/OBES FY90 budget.

II. FINDINGS

In previous BESAC annual reports, and by letter to the director of DOE/ER, we strongly recommended that the Geosciences budget be increased by \$5 million for new basic research on underground fluids. This recommendation was not implemented, and the geosciences program, as a whole, remains seriously underfunded in relation to its strategic importance to DOE's mission.

As in the past, most future domestic energy will come from petroleum, and most U.S. petroleum will come from enhanced oil recovery (EOR) from old oil fields. Basic geosciences research on subsurface fluids and fluid pathways will be equally useful for waste clean-up, proper waste disposal, and for EOR projects. It is difficult to determine the portion of the total geosciences budget now being spent on basic problems of mass and energy transport by subsurface fluids. For FY90 we estimate that about \$3.7 million (23%) is now being spent on fluid/rock research, excluding research on imaging the subsurface. It seems clear that in the future more funds should be devoted to fluid/rock research. This finding is not a criticism of the excellent research in imaging now being supported by OBES.

Environmental Restoration

Environmental restoration of DOE lab waste disposal sites is dealt with specifically in the report by the Environmental Subcommittee of BESAC. However, much of the clean-up problem is geological with long-term consequences and requires basic geosciences research. Clean-up of all natural lab disposal sites will require hundreds of millions of dollars if present understanding and techniques are used. Breakthroughs in science and engineering are required to reduce costs. The principal geologic problems are characterization of (1) rock permeability and (2) rock chemistry.

Perhaps the most difficult geological problem is the mixing of underground fluids, called hydrodynamic dispersion. Dispersion is caused as fluids move through heterogeneous rock. In typical reservoir rock, permeability can vary by five or six orders of magnitude, and permeabilities in the less-permeable cap rock (confining strata) can vary by eight to ten orders of magnitude. In addition, chemical variability in rock minerals and reservoir fluids influence dispersion. The result is that complex short circuits of contaminants are created and these change through time. Geologists find that the fine-grained clay minerals can react strongly with chemicals in the fluids, while coarser-grained sediments are generally less reactive.

Dispersivity changes with scale; the larger the scale, the larger the dispersivity. Thus lab experiments on permeability and chemical reactivity of rock and fluids often are very valuable, but are extrapolated to large subsurface reservoirs with considerable uncertainty. Therefore, we believe that experiments should be carried out in the field by monitoring existing wells and by new scientific drilling. Many crucial observations can be made by drilling a series of inexpensive shallow wells supplemented by a few deep wells. Some observations need to be made over a few years, hence the need to begin scientific drilling near contaminated groundwater reservoirs as soon as possible.

Geophysical measurements made from ground surface provide important data and augment data gained by drilling. Reflection and transmission acoustical (seismic) data can give details of rock permeability and porosity. The later give hope that, in some cases, contaminant "fronts" or boundaries can be located without drilling or will permit fewer observations wells.

Rock/Fluid Research

Study of individual waste disposal sites alone, however, is insufficient. Fundamental questions of the physics and chemistry of fluid/rock interactions are needed, to avoid bad surprises at local sites where regional influences are strong, such as a high rate of fluid flow or unpredicted direction of movement of subsurface fluids. Natural occurrence of toxic substances in the subsurface, such as mercury, can complicate clean-up, particularly where reactions with certain organic acids occur. Thus, the need for large scale, long-term geosciences understanding suggests research into the coupled system properties of fluids and rocks. These include:

- 1) Basic geometry (structure and stratigraphy) of porous rock
- 2) Boundary conditions of topography
- 3) Sources and sinks of fluids
- 4) History and present distribution of permeability over a wide variety of scales in porous and fractured rock
- 5) History and present states of fluid properties including density, viscosity, and chemistry
- 6) Identification, kinetics, and history of chemical reactions
- 7) Dispersivity of mass and heat
- 8) Thermal conductivity of rocks and fluids
- 9) Elastic constants and plastic behavior of rock

The greatest emphasis should be on research into physics and geology of fractured rock, including porous sediment and the identification and kinematics of chemical reactions, especially those that effect permeability and porosity.

The results of research on the above listed items will provide data that are related by a set of coupled differential equations that describe conservation statements for mass energy, and momentum. The coupled equations must be solved simultaneously. The equations are difficult to solve analytically; however, with modern computers, solutions to realistic problems of great interest are now readily possible.

As examples of the complexity of the fluid/rock system, it is now recognized that in the vicinity of magmatic intrusions, aqueous solutions that transport ore minerals generate permeability by extensively fracturing rock. Later, permeability is decreased as these

moving solutions deposit minerals as fracture fillings. Similarly, newly generated petroleum may cause extensive fracturing of the petroleum source rock, usually shale, permitting migration of oil to reservoir rock of limestone and sandstone. Thus such seemingly diverse problems as ore mineral deposition and petroleum migration are closely related because two parameters critical to the fluid transport process, (1) permeability, and (2) chemical reactions and their kinetics, are related.

At crustal levels below where open fractures are usually found (usually about 2-3 km, although fractures may exist to depths greater than 10 km), the mechanism of flow becomes ambiguous. Is temporary fracturing important, or do fluids move by diffusion or along grain boundaries? Do fluids induce temporary changes in rock character that disappear after fluid migration has ceased? What mineral reactions are reversible and what are not, and under what conditions? The equations for pressure, chemical composition, temperature, and fluid flow are coupled and nonlinear. It is significant that few of these linkages have been directly observed in the field. Yet their interaction controls not only ore deposition and initial petroleum migration, but also groundwater contaminants including toxic and radioactive wastes, enhanced oil recovery, and geothermal heat. It is important also to recognize the cyclicity of fluid migration events for waste site monitoring. Also, episodic fluid migration near active faults may be important for earthquake prediction.

The Acoustical Imaging Initiative

Funding for the OBES acoustical imaging initiative of FY90 is \$2.85 million. Continuing research on underground imaging is at a level of \$3.62 million for a total FY90 budget of \$6.47 million for all underground geophysical imaging research. This is nearly 40% of the total OBES Geosciences budget of \$16.4 million. This is not too much to spend on underground imaging research, but is too large a fraction of the geosciences budget. The underground acoustical imaging research initiative funds excellent science, complements more extensive research by the petroleum industry, but appears to pay too little attention to the shallow subsurface (less than 1000 feet) where most toxic wastes are stored.

Research on acoustical imaging is done mostly by oil companies and most of their work seeks to improve seismic imaging using ground-surface energy sources and receivers. Downhole receivers have been used for velocity control to improve data processing, and recently downhole energy sources have been used. Lately, oil companies

have experimented with a moving downhole energy source in one well and an array of receivers in an adjacent well, in attempts to produce cross-well tomograms for fluid saturations, porosity, and permeability. The quality of cross-well tomography will soon be good enough to be crucial to the success of many enhanced oil recovery (EOR) projects. Transmitted acoustical energy is also sensitive to temperature. Hence, the EOR techniques of steam and fire floods can be closely monitored.

During our conferences with oil company researchers, they reported the need for research into better tools for downhole energy sources. It appears that Geosciences/OBES could play a useful role by providing a small amount of crucial research funding, and by coordinating work being done in universities and at national laboratories with industrial work.

Downhole imaging by electromagnetic sources has not gotten as much attention from researchers as has acoustical imaging, but has promise where wells are close together and where contaminants have electrochemical properties quite different than groundwater.

III. RECOMMENDATIONS

1. We strongly recommend that the Geosciences budget be increased to provide an adequate level of support for DOE's efforts in environmental restoration, energy, and other topics of national importance.
2. We recommend basic research on underground fluids with increasing emphasis on mass transport, energy transport, and chemical reactions in the deep and shallow subsurface. Current research is of high quality, but is insufficient to enable DOE to clean up waste disposal sites, to select safe long-term sites, and to guarantee adequate domestic energy supplies.
3. The greatest emphasis should be on research into the physics and geology of fractured rock, including porous sediment, and the identification and kinematics of chemical reactions, especially those that effect permeability and porosity.
4. In regard to the critical scaling problem of fluid dispersivity, we recommend that experiments be carried out in the field by monitoring existing wells and by new scientific drilling.

5. **Research on advanced imaging techniques should place greater emphasis on the relatively shallow subsurface.**

APPENDIX 12**REPORT OF THE SUBCOMMITTEE ON
APPLIED MATHEMATICAL SCIENCES - 1989**

F. Y. Fradin
I. M. Singer
A. B. White, Chairperson

I. BACKGROUND

On October 11, 1989, the Applied Mathematics Subcommittee of BESAC met in Cambridge at MIT to review aspects of the AMS program in High-performance Computing and to consider recommendations to the full committee in this area. In particular, we were concerned with the recently published OSTP report, "The Federal High-performance Computing Program". Presenting a short discussion of activities ongoing at individual sites were:

- o Bob Ward - Oak Ridge National Laboratory
- o Rick Stevens - Argonne National laboratory
- o Jim McGraw - Lawrence Livermore National Laboratory
- o Paul Messina - California Institute of Technology
- o Dennis Duke - Florida State University
- o Bill Camp - Sandia National Laboratories
- o Jeff Saltzman - Los Alamos National Laboratory

II. FINDINGS

Computational science has become an indispensable tool for understanding and thus manipulating our physical environment. This tool - high-performance computers, visualization techniques, high-speed networking, reliable and robust software - has augmented in important and essential ways both theoretical and experimental science. Mathematical models can be interrogated computationally in much greater detail than is usually feasible with purely theoretical (analytical) techniques; computational experiments

have provided insight into physical regimes that are expensive, dangerous, or even impossible to examine directly. In this latter category, we find simulations of events such as nuclear winter, nuclear reactor accidents, and environmental consequences of hazardous waste disposal.

The Department of Energy and its predecessor agencies (e.g., ERDA) have played a dominant role in the development of computational science. Beginning with the “Los Alamos” problem run on the ENIAC and MANIAC machines in the 1940s and extending to the CRI supercomputers of this decade, the DOE complex has driven the development of supercomputers and their peripherals. Over this forty-year span, some applications have enjoyed an increase in computing speed of ten orders of magnitude. However, in spite of this enormous increase, we still cannot effectively simulate some problems which are important to our national interests. For example, in a recent presentation to the FCCSET Committee on Earth Sciences (CES), DOE’s initiative on Global Warming included as a major ingredient a goal “to effectively increase computer speeds in climate modeling by 10000”. These increases will only come through application of parallel processing to major application problems, barring major advances in component technology (e.g., optical or quantum tunneling devices).

In the area of advanced computing, the Department of Energy programs have taken the lead in evaluation of early hardware prototypes of advanced architectures, in research on algorithms for high-performance architectures, and in development of applications codes running on experimental production systems. The AMS program in advanced computing is excellent and building toward an effort integrated across both disciplines and organizations. There have been a number of significant accomplishments attributable to this program in advanced computing; among those accomplishments are:

- o the development of the hypercube architecture - presently the dominant architecture for highly parallel machines
- o design of cycle prefix networks for interprocessor communication
- o the DOE Grand Challenge computing program.

In 1987, OSTP published “A Research and Development Strategy for High-performance Computing” which described a five-year plan to develop computational science into a tool applicable to even the most challenging problems - the Grand Challenges. In September of this year, OSTP published the implementation plan for the previous strategy

document: "The Federal High-performance Computing Program". This plan has four components:

- o high-performance computing systems
- o advanced software technology and algorithms
- o the National Research and Education Network
- o basic research and human resources.

Many of the proposals in the implementation plan build upon the expertise and research efforts of the DOE program.

III. RECOMMENDATIONS

1. The Department of Energy should aggressively pursue a leading role in the Federal High-performance Computing Program based on its historical influence, its present programs in high-performance computing, and the importance of DOE Grand Challenges in fields such as condensed matter physics, chemistry, environmental studies, and national defense. In the first year, additional funding on the order of \$30M should be sought for the DOE role in the HPC program.
2. The Office of Basic Energy Sciences should take advantage of the computational resources, both conventional and experimental computers, to solve relevant Grand Challenge problems in suitable OBES disciplines.
3. The Applied Mathematical Sciences program should consider increased capital equipment funding for new, robust parallel architecture computers for research and development on software, algorithms, and applications.
4. Supporting the aims of the 1989 OSTP Report, the Department of Energy should develop an educational and training initiative aimed at all aspects of computational science.

APPENDIX 13**REPORT OF BESAC SUBCOMMITTEE ON
BASIC ENGINEERING RESEARCH AND ADVANCED ENERGY PROJECTS - 1989**

J. Macek
R. M. Osgood, Jr.
H. H. Richardson, Chairperson

The membership of the augmented subcommittee for 1989 is shown in Attachment (1) to the Subcommittee report. In addition to its assignment to review the Advanced Energy Projects (AEP) and Engineering Research (ER) Programs within OBES, the subcommittee was asked to review the proposed new initiative in x-ray lithography in response to Dr. Hunter's request to BESAC.

The subcommittee held three meetings during 1989, all in Washington, D.C. The first meeting, on August 11, focused exclusively on the ER Program, and the second, on October 5, dealt with the AEP Program and the x-ray lithography initiative. A final meeting was held on November 27, to formulate the subcommittee's findings and recommendations as presented in this report.

(A) Basic Engineering Research**BACKGROUND**

Basic engineering research generates the knowledge base, techniques, and tools necessary for the design of new and the effective operation of existing energy systems. The reservoir of generic results produced is essential to the design, optimization, and safe and efficient operation of energy systems..whatever they may be..in an uncertain future environment. As DOE mobilizes major efforts to address the national problems of environmental restoration, engineering research has an especially urgent role to play in making possible the development of new, cost-effective and safe systems for restoration.

In its 1988 report, the subcommittee commented on the exceptional quality of the program management, of the investigators and projects supported, and on the success in transferring results to the user community. The subcommittee continues to be impressed with the leadership, performance and high quality of this program.

Four major new initiatives, requiring additional funding, were recommended last year: Large Scale Energy Systems, Multi-Phase and Porous Media Flows, Dynamics of Highly Stressed Structures, and Bioprocessing of Fuels and Related Wastes. The BESAC recommendation for a major research thrust in permeability and flow in porous media, with increased funds in engineering and in geosciences, was endorsed. We also recommended that a DOE-wide coordinating mechanism for basic engineering research be established, under the chairmanship of the OBES basic engineering research program. We recommended continuing efforts in strategic planning, and increased emphasis on supporting energy research that involves electrical engineering and computer science.

FINDINGS

The Basic Engineering Research program supports a key part of the OBES mission, creating generic engineering results, applicable to a wide range of energy systems, that are essential to the design of future energy systems and for the safe and efficient operation of existing systems. A particularly pressing need exists, over the next several years, for basic engineering research relevant to the prevention, processing and cost effective cleanup (i.e., environmental restoration) of hazardous waste.

Within the substantially level funding available, the Engineering Research program has been responsive to the subcommittee's 1988 recommendations. A DOE-wide Energy Engineering Research Coordinating Committee has been formed and has met twice. Significant redirection of effort has taken place to address the recommended initiative in multiphase and porous media flows. Of the 14 new projects funded in FY89, eight were in this area. Total funding for the area is now about \$2.75 million, far short of what is needed, but all that could be reasonably devoted without seriously damaging the core engineering research program. A workshop "Bioprocessing Research for Energy Applications" [ORNL/TM-11054, April 1989] was completed, thus providing a blueprint for the recommended bioprocessing initiative. New funding must be provided in order to begin implementation. A workshop in the area of highly stressed structures is under

consideration. No action has been taken on the large scale systems initiative due to the absence of needed new funds.

Bioprocessing offers an extremely attractive approach to many of the problems faced in environmental engineering and restoration, yet is relatively underdeveloped, especially in the area of bioprocess engineering. Bioprocess engineering research is needed in order to enable the practical utilization of new results in the biosciences in a range of energy-related bioprocessing systems. Among the more promising applications are enhanced petroleum recovery, oil spill cleanup, removal of hazardous materials from liquids, biomass conversion, microbial and enzymatic processes for solar hydrolysis of water, and biological treatment of nuclear and chemical wastes. Bioremediation technology was identified as one of the principal environmental restoration research requirements in the DOE Office of Energy Research Workshop held in April, 1989 ["Basic Research for Environmental Restoration,;" DOE/ER-0419, September 1989]. The subcommittee views engineering research in bioprocess engineering as the top priority initiative for new funding in the Basic Engineering Research budget for FY90 and 91.

The initiative in multiphase and porous media flows is considered to be of major importance. Basic engineering research in this area is critical to applications in such fields as power plant design and optimization, crystal growth, chemical and biochemical processing, slurry and pneumatic transport, mineral separation, refrigeration, high-power-density electronic cooling, combustion and emissions control, and the flow and dispersion of toxic substances in the earth. A flow of outstanding proposals exists, and additional funds are needed to carry this research further.

The subcommittee reviewed the energy engineering research conducted in the National Science Foundation (NSF). Of the approximately \$250 million engineering research program, about \$19 million was identified as energy-related. Several important gaps in energy engineering research were noted in the areas of petroleum/reservoir engineering, power systems (especially electric power systems), and nuclear engineering. No formal mechanism for coordination of NSF and DOE engineering research was found.

The question of appropriate funding levels for the DOE Basic Engineering Research program was discussed at length. No quantitative value for appropriate levels could be identified; however, the subcommittee feels that the program is currently underfunded, perhaps by a factor of two or three. In NSF, the engineering research budget has

traditionally been about 10% of the total research budget. Applying this historical figure to DOE would suggest a basic engineering budget of about \$50 million. Overall, the subcommittee believes that the proposed \$50 million level contained in the BER long range plan for FY95 is reasonable.

A review of the portfolio of the core BER program revealed that a significant portion of the effort currently supported addresses the mid-to-long term research needs identified in the above-referenced DOE report on Basic Research for Environmental Restoration. The most relevant areas are:

Plasma Processing (for de-toxifying wastes)	\$1.695 M
Robotics (for site survey and in-situ sampling)	\$1.625 M
Transport in Porous Media (to understand transport phenomena)	\$0.490 M

The increasing demands in environmental engineering, both in new systems design and in environmental restoration, will require an increased supply of engineers and engineering faculty at a time when severe shortages are projected for engineering as a whole. The BER program, through support of university research, can make an important contribution to meeting this need for professional personnel.

RECOMMENDATIONS

1. We recommend that DOE mount a coordinated research initiative in bioprocessing of fuels and energy related wastes, led by BER and involving as a minimum Energy Biosciences (EB) and the Office of Health and Environmental Research (OHER). The program should be coordinated with the Engineering Directorate of NSF and should involve industry participation. We believe that the environmental area is suitable for cooperative research with industry, and can attract significant industry interest and financial support. This initiative should be given a high priority by OBES and DOE.

2. We recommend that new funds be added to the OBES and OHER budgets beginning in FY92 (\$3M in BER, \$2M in EB and \$2M in OHER) to fund this initiative. A larger fraction of the funding is suggested in BER, because little work is now underway in bioprocessing in BER compared with that in OHER and EB. In the meantime, planning and seed funding should proceed within available resources. We recommend a FY95 target level of about \$11M in engineering bioprocessing research.
3. The 1988 BESAC recommendation that \$5M be added to the Geosciences and \$2.5M to the Engineering budget for research in flows through underground permeable structures should be implemented. In addition, the BER initiative in multiphase and porous media flows should receive increased funding, with an FY95 target level of approximately \$11M in engineering bioprocessing research.
4. In light of near-term budget stringencies, we recommend that the initiative in highly stressed structures be deferred indefinitely, and research in this area be incorporated into the core program to the extent feasible. We recommend that the initiative in large scale systems be deferred until FY92. A target level for this program of \$5.5M is suggested for FY95.
5. The core Engineering Research program, currently underfunded, should be increased over the next five years, if funds are available, to approximately twice its FY90 level. Together with the above recommendations, this would suggest an FY95 target funding for BER of about \$50M.

(B) Advanced Energy Projects

FINDINGS

The subcommittee continues to support strongly the "Core" concept of the AEP; that is, to encourage and support high-risk, high-potential-payoff ideas evolving from basic research. The subcommittee continues to be impressed with the high quality of the projects and investigators supported, and with the success in transferring important results to industry and other users. This is a very well-managed program with impressive results.

Of the approximately \$9 million budget in the core program, about one-third is available for new projects, permitting about eight new starts per year. This program size and rate of turnover seems adequate.

The subcommittee is pleased to see that the continuing cluster of muon-catalized fusion projects is being phased out, as suggested in the 1988 BESAC report. It appears that the core program is now normally operating with a typical three-year funding cycle supported in the range of \$250K/yr., as recommended.

The subcommittee did not review the Heavy Ion Fusion Accelerator Research Program or the SBIR Program administered by AEP. Both of these programs have been placed in OBES/AEP for convenience, and should not be allowed to compete with other OBES programs or with the important core program of the AEP.

RECOMMENDATION

We recommend that the AEP core program be continued at its present level, with modest growth, at least to offset inflation, in the future.

(C) X-Ray Lithography

BACKGROUND

The subcommittee established a panel on x-ray lithography [Attachment (2)]. The panel met in Washington on October 5, 1989 with Dr. Ryszard Gajewski, Director of the AEP, and representatives invited by DOE from DARPA, LANL, BNL, ANL, NRL, SEMATECH, IBM, Princeton and Louisiana State University. Dr. Don Stevens also attended. Unfortunately, the University of Wisconsin, which has an operating beamline for industrial x-ray lithography developments in microfabrication, was not represented. However, information on that program was obtained later by mail and telephone.

Subsequent to this meeting, the x-ray lithography initiative was discussed by BESAC at its October meeting, and a subset of the panel met following this meeting for further discussion. A final discussion of the panel's findings and recommendations was held during the subcommittee meeting in Washington on November 27. The full BESAC

reviewed the results on November 29 in Dallas and approved the report with minor changes.

FINDINGS

1. To encourage acceleration of the U.S. effort in submicron chip manufacture is important to the nation. Current efforts lag considerably behind those of Japan and Europe, and U.S. industry capacity for producing such chips is in serious jeopardy. Synchrotron radiation offers an attractive short-term option for a commercially practical light source for production of electronic chips with feature sizes less than 0.35 microns. DOE should also conduct research into alternative sources and systems to achieve even smaller feature sizes.
2. The light source, although important, represents only a small portion of the total investment necessary for commercial chip manufacture. In the U.S., only IBM, as a single organization, has the R&D program and financial resources to undertake the total development process, and is doing so with its own funds. Perhaps SEMATECH or other industry consortia could mount a program to utilize a DOE-developed light source, but the panel suspects this is not likely.
3. The panel believes that a program to develop a small industrial synchrotron would demonstrate DOE technology transfer. Synchrotrons have been used as x-ray sources for applications in microelectronics, microsensors, and biological studies. Practical, small synchrotrons are essential for such applications to be industrially feasible, and might enable small-scale synchrotron research to be undertaken in many more university laboratories. Japan has realized this, and is building a host of such machines. If the U.S. is to be competitive in these areas, it is essential that an industrial synchrotron be developed expeditiously in this country.
4. The panel believes that the concept of developing two machines for \$33 million is unrealistic, but the development of a single "warm" (i.e., non-cryogenic) machine is reasonable. There are several organizations and consortia fully capable of responding and successfully producing a "warm" machine.

5. The panel found no coordination of the proposed machine development with other major activities underway at BNL, IBM, DARPA, University of Wisconsin, Louisiana State University, and others.

RECOMMENDATIONS

1. DOE should proceed expeditiously to explore the feasibility and potential benefits of developing an industrial x-ray lithography machine for the commercial production of microchips. This initiative should begin with the formulation of a national plan for the utilization of an industrial machine. We recommend the following steps:

- o Establish with certainty the ultimate home of the machine to result from the current Brookhaven effort (the BNL machine).

- o Ascertain which companies wish to utilize the BNL machine at a substantial level when it is in its home location.

- o Convene U.S. industrial representatives, DOE lithography experts and DARPA representatives to secure a projection for the need for an additional DOE-developed light source for use in chip manufacture. This should include the prospects for securing industry matching funds for this development, and for achieving the additional industry investment needed for one or more complete chip fabrication facility. Until such need and commitments can be demonstrated, no RFP should be issued for the construction of an additional x-ray source.

- o If it is determined that there exist one or more viable customers for an industrial machine, then an RFP should be issued for the design of ONE machine, requiring that cost and construction schedules be included. At least two competing designs should be sought.

2. This program should not be funded at the expense of the OBES budget, since it is basically a development project. The OBES budget is already severely strained, both in terms of research support and research facilities operation and development. DOE should consider securing matching funds from state and industrial sources, and look to the more development-oriented parts of the Department for the needed federal share.

3. The panel feels that the practical solution for an industrial synchrotron in the short term lies in a “warm” rather than a cryogenic machine. While the cryogenic machine has advantages in footprint and overall size, these advantages are more than offset by the difficulty in development and in operation of the cryogenic system. The “warm” machine is well developed and an industrial machine development is entirely feasible within the present state of the art.

4. OBES should support research into alternative light sources to achieve even smaller feature sizes than those evidently feasible with state-of-the-art synchrotrons.

5. OBES should limit its participation in microchip production research to the development of light sources. This is the area where OBES has special competence and extensive experience. DOE should not become involved in other aspects of chip manufacture such as mask and stepper development.

Attachment 1**BESAC SUBCOMMITTEE ON BASIC ENGINEERING RESEARCH
AND ADVANCED ENERGY PROJECTS**

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Attachment 2BESAC PANEL ON X-RAY LITHOGRAPHY

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APPENDIX 14**REPORT OF BESAC SUBCOMMITTEE ON
ENERGY BIOSCIENCES - 1989**

G. D. Hegeman, Chairperson
D. C. Hoffmann
M. S. Wrighton

I. BACKGROUND

The Energy Biosciences Program ("Biological Energy Research Program") began ten years ago. It was given the goal of supporting biological research efforts basic to energy conservation and renewable resource development. This mission contrasts with those of other DOE biological programs, which concern themselves with the biological effects of energy production and use, rather than the biology of energy conversion and conservation. Energy Biosciences has undergone slow growth from its modest start (\$6 million in FY80) but has recently suffered both a relative and absolute decline in support. As a result, the number of new proposals being funded has reached a very low level of less than 5%.

The FY89 Energy Biosciences budget was \$20.78 million, of which 56% was devoted to support of 139 investigator-initiated projects in the areas of biology basic to energy trapping, conversion and conservation. The balance was distributed approximately equally (*ca.* 17% each) to i) the University of Michigan Plant Research Laboratory, ii) three National Laboratories (Brookhaven, Lawrence-Berkeley and Los Alamos) and iii) a collection of centers (e.g. Oregon Graduate Center), SERI, the SBIR mandated allocation and other minor commitments (databases, conferences, workshops, etc.).

The proposed level of the Energy Biosciences budget for FY90 will affect most adversely investigator-initiated grants, since other commitments are fixed (long-term or mandated by legislation). The EB budget will fall both in actual dollars and by the effects of inflation on the value of the dollar. Ironically, applications to EB have almost doubled in number over the past three years. The actual number of proposals funded dropped for the

first time in FY89, and is estimated to drop further in FY90 to a discouraging 5% funding level.

II. FINDINGS

Energy Biosciences has done an excellent job of allocating the 89% of EB's budget over which it has control among a number of appropriate areas divided roughly equally between plant biology and microbiology (see pp. ii and iii, Annual Report and Summaries of FY89 Activities, Division of Energy Biosciences, September 1989, DOE/ER-0424). The projects supported are a nice mix of mature efforts, now yielding good results in satisfying quantity, and more speculative projects in emerging areas where new techniques or recent findings suggest that exciting developments are imminent.

Criticized for maintaining too low a profile on the horizon of funding sources available to the biological community in FY85, EB has done a stellar job of advertising itself. Its success is reflected by a program of very high quality, although the funding rate remains discouragingly low. Of signal importance in raising the profile of EB programs and in seeking out areas of emerging importance for funding, has been the effort to mount meetings, conferences and workshops. This program, although relatively small in cost, has had great impact and is valuable in planning and identifying new areas for support.

The recently announced effort to support study of "energy genomes" (the DNA complements of mitochondria and chloroplasts) has some merit. The mitochondrial genome, in many cases, is small even in comparison with the bacterial genome, and seems a relatively opportune target for total sequence determination. Selected chloroplast genomes, while larger, should also be worthwhile to study. Tight focus on the energy genomes of one (e.g. *Zea mays*) or a few related plants could yield significant comparative results in a relatively short time.

The recent crystallization and determination of the structure of the *Rhodobacter viridis* photosynthetic reaction center suggests the maturing character of some structural research in photosynthesis. Perhaps some reallocation of increasingly tight funds from these areas into new, underfunded areas is in order.

III. RECOMMENDATIONS

In what follows we lean heavily on the Summary Report of the Workshop on Research Directions for the Division of Energy Biosciences held at the ACS Belmont Conference Center, Elkridge, MD, 2-4 December, 1989 (Report compiled and edited by George E. Stapleton).

Individual investigator-initiated projects have been the core of EB's program and this should continue. Research Centers and fellowship programs tend to develop costly administrative baggage, with notable exceptions, and take on a form which becomes its own excuse for existence. State-of-the art science should be supported by EB, and to attract younger scientists is important, but in a time of shrinking support these goals can probably be best and most economically met by supporting good young investigators, postdoctoral fellows and students on individual research projects. Priority should be given to these younger investigators, since established workers are likely to have resources that allow them to survive times of stringency. Other responses to short funds could include use of cost sharing with institutions, particularly for expensive capital equipment items of general use equipment. Similarly, matching funds for grantees who can bring in industrial partners could leverage scarce funds and also aid in technology transfer in areas of interest to EB.

One important way that EB might improve its funding position is to emphasize projects fundamental to bioremediation. Cleanup efforts fit nicely within the general mission of energy conservation, - particularly *in situ* bioremediation. The Environmental Subcommittee of BESAC pointed out in the report of its August 3, 1989 meeting that the first of five areas identified in environmental restoration is to obtain fundamental knowledge of natural systems (e.g. microbiology applied to *in situ* restoration), and that EB is the logical lead division within DOE to give such efforts a home.

Specific areas of emerging interest, in addition to those currently represented on the EB list of projects, are:

- o Plant and microbial secondary metabolism
- o Neglected (under exploited) microorganisms
- o Plant cell and subcellular biology
- o Plant plastid genomes

- o Microbial potential for solid waste reduction and *in situ* bioremediation
- o Microbially induced biofouling and corrosion

The disparity between the number of pre-screened proposals being submitted and the number funded, taken together with the declining level of individual awards, the high quality of the current program and the large number of promising areas in which work should be done, lead us to recommend an increase in funding for Energy Biosciences in FY90 to at least \$40 million. This will, at least to some extent, redress the declining morale and declining research facilities now encountered in the Energy Biosciences area. It may also help to retain and attract the crop of young investigators on which future EB research will depend.

REPORT OF THE SUBCOMMITTEE
FOR GLOBAL CHANGE

BASIC ENERGY SCIENCE ADVISORY COMMITTEE (BESAC)
HEALTH AND ENVIRONMENTAL RESEARCH ADVISORY COMMITTEE (HERAC)

Vera Alexander
John Bredehoeft (Co-Chairman)
Todd Crawford
Simon Levin
James Mahoney
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Harold Mooney (Co-Chairman)
Leon Silver
William Travers
Karl Turekian

November 1989

EXECUTIVE SUMMARY

This Subcommittee for Global Change was formed in response to a request from James Decker, Acting Director of Energy Research, to review DOE's major global change research programs, which include (1) extensive CO₂ studies, (2) the Continental Ocean Margin Flux Study (COMFS), and (3) Terrestrial Ecology studies. We found the research generally to be of high quality, and relevant to the major issues of global change. However we were concerned about the coordination among the research program elements, as well as the integration of the research activities into the broader responsibilities of DOE relevant to global change.

The input into the atmosphere of CO₂ and trace gases from energy production is the principal driving force of concern in global climate change. World-wide concern for the effects of climate change can seriously constrain future energy systems and may have a major negative economic impact on the United States as well as other developed nations. The Department of Energy needs to be involved in both (1) research investigating global change processes and effects, and (2) technology development of environmentally acceptable and economically feasible alternative energy sources and energy conservation measures. Research on global change must be closely integrated with technology development activities within DOE.

Global change due to emissions of greenhouse gases has environmental, societal and economic impacts that transcend the scientific research. The Subcommittee's principal concern is the need for wider participation in the issues of global change by the entire Department of Energy. We have identified two levels of needed coordination: (1) a coordinated global change research effort within Energy Research (ER), and (2) a coordinated response to global change issues by the entire Department.

The Department of Energy has the principal responsibility for the

development of U.S. energy strategy. Potential global climate changes resulting from an increase in greenhouse gases will be major constraints on future energy system development, and must be a serious consideration for the entire Department. **We believe DOE should take a holistic view of global change such as that illustrated in Figure 1.** The possible need to mitigate global environmental impacts has become an important feedback mechanism, which will influence virtually all future energy technology developments, and the priorities for change in the national energy system. Energy Research (ER) has important responsibilities within this feedback system; its own activities must be well coordinated, and must be closely integrated with the Department's broader responsibilities for technology development and energy strategy planning.

The Subcommittee has recommendations in three areas:

- I. A Global Change Coordinator should be established in the Secretary's Office.** This individual would coordinate global change activities throughout DOE. The principal responsibilities for the Coordinator would be to ensure that (1) the analysis capability implied in Figure 1 is achieved, and (2) research and technology development are continuously integrated. The Departmental Coordinator should ensure that the Department addresses two specific issues at an early date:
 - o A Department-wide Strategy for DOE Global Change Research should be formally established.**
 - o DOE should assure that the data sets needed for long-term global change studies are being systematically collected, evaluated, and archived.** The data necessary to assess global change come from diverse sources. Data collection in the FY 90 Federal Research Program constitutes only 8 percent of the total research effort; this seems small, given the nature of the problem.

Responsibility of Department of Energy

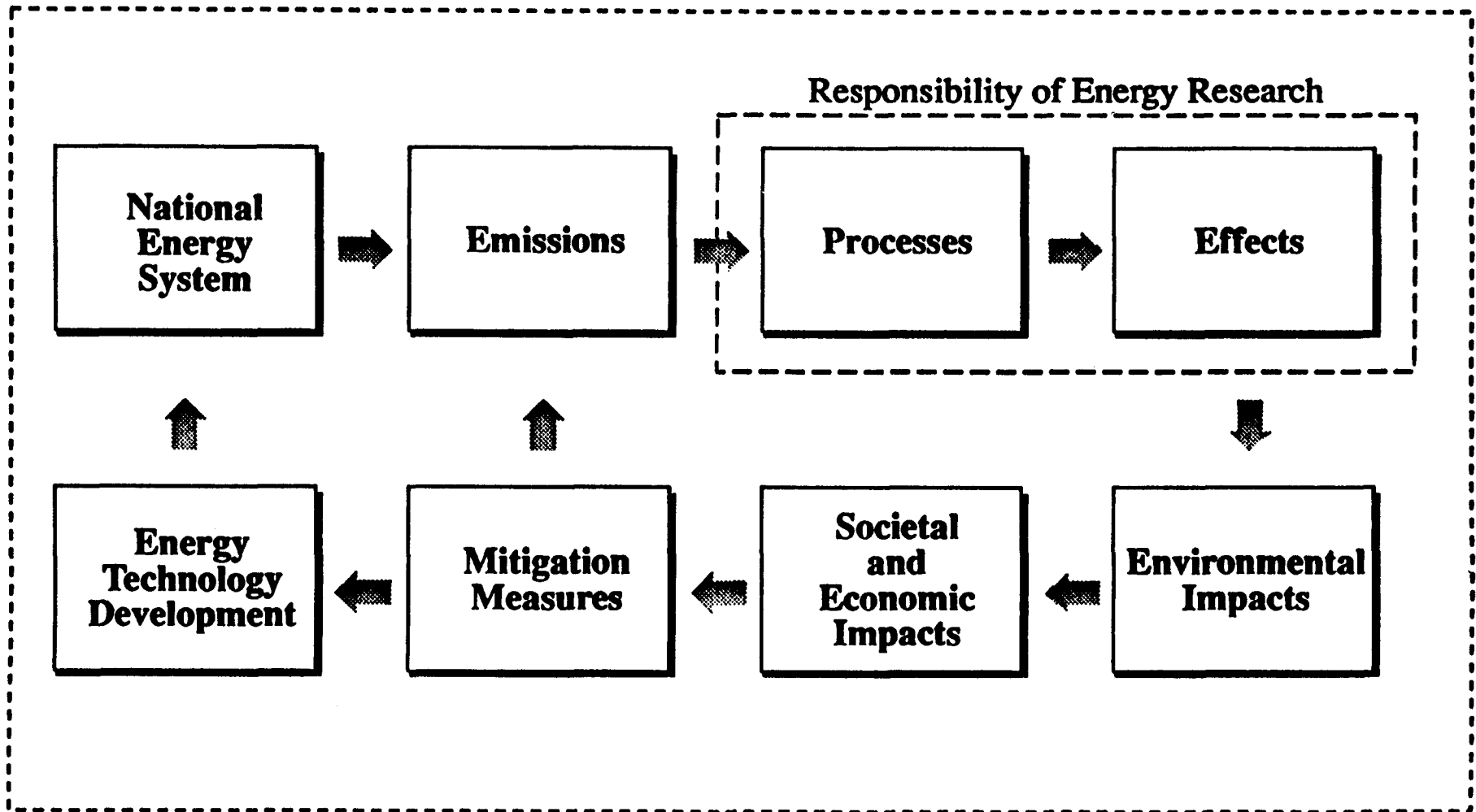


FIGURE 1.

Feedback model of the relationship between energy systems, global environmental impacts and mitigation requirements

- II. DOE should play a key interagency role in coordinating U.S. global change activities. In some activities, such as CO₂ research, the Department already has the major role. It should assume a broad leadership role in interagency coordination, especially in the Committee on Earth Sciences (CES). The subcommittee believes that DOE has not been viewed as having a leadership role in CES, because of time limitations of senior staff, and lack of a specific Department strategy.
- III. DOE's global change research should be more intensively coordinated, and expanded in certain areas.

The Subcommittee recommends that a coordinator for all research related to global change processes and effects be appointed, reporting to the Director of Energy Research. This coordinator would have the responsibility to assure that the Department's research is prioritized and fully coordinated, and that research planning is periodically updated to reflect the priorities of the entire Department, as well as the interagency Committee on Earth Sciences.

It is the Subcommittee's view that each of the current DOE research program elements relevant to global change (CO₂, Continental Margin studies, and Terrestrial Ecology) is doing important and productive research and fills a major need in the U.S. Global Change Program. After reviewing the ER global change research program, we have identified areas of research which we believe should continue to have high priority for continued support, and expansion as appropriate. These are:

- o Geochemical observations of CO₂, other nutrients, and trace gases in the deep ocean.

- o Experiments to better understand the important processes that cause global change that are included in General Circulation Models (GCM), such as the fluxes of radiatively sensitive gases to and from the earth's surface, the role of aerosols on cloud formation, cloud distribution as affected by climate change, etc.
- o Comparison of the structures of various GCMs to understand differences in model results.
- o Large-scale ecosystem experiments to understand their response to environmental stress.
- o Study of total nutrient movement across the Continental Margin.
- o Continued investigation of the carbon cycle.
- o Integration of ecological models with the General Circulation Models.

INTRODUCTION

The Subcommittee for Global Change was formed in response to a request from James Decker, Acting Director of Energy Research, to the Health and Environmental Research Advisory Committee (HERAC) and the Basic Energy Science Advisory Committee (BESAC), for a broad evaluation of the role of DOE in global change. The Subcommittee was appointed by the HERAC and BESAC Chairmen from among the committee memberships, for a one-year duration. The Subcommittee has completed its investigation; this is its final report.

The United States is the largest energy consuming nation in the world, producing approximately 20 percent of the anthropogenic CO₂ input to the atmosphere. The buildup of greenhouse gases in the atmosphere is of particular concern to DOE, since much of this buildup is the result of burning carbon-based fuels. The world-wide concern for climate change could lead to the world-wide adoption of control measures which in turn would constrain the mix of future energy systems. DOE is involved with both global change research and the development of environmentally acceptable and economically feasible alternative energy technologies, including conservation.

The Subcommittee for Global Change has responded to its charge in a broad context. There is an obvious need for participation on global change research, technology development, assessment and planning issues across a broad spectrum of DOE activities. We are concerned that there be an integrated program within the Department to address global change. Our report includes broad recommendations for the Department, as well as recommendations on coordination and research programs within Energy Research (ER).

Interest in global change issues has developed substantially during the past twelve months, both nationally and internationally, as well as within DOE. There is a growing international awareness of global change as a

significant world-wide problem. Several heads of state have expressed concern; there have been numerous international meetings to discuss the subject. Within the United States the National Research Council Committee on Global Change has published its first report, in which it has suggested an initial research agenda for the U.S.

Within the Federal Government the Committee on Earth Science (CES), established by the Federal Coordinating Council for Science, Engineering and Technology (FCCSET) is coordinating the Federal Program of Global Change Research. The CES research coordinating effort has been successful to date. Numerous federal agencies are coming to a consensus about the role of each agency in mounting a large and diverse government-wide research effort. There is an awareness that the problem is so large that the scientific community will not make significant progress without genuine cooperation among the participating agencies. The CES has drafted its first report outlining a federal global change research program for FY 90; those who have reviewed the draft document, including this subcommittee, are favorably impressed. DOE, because it has such a large stake in these matters, must be an active participant in the CES deliberations. DOE has indeed become more active during the past year, but further increases in activity by the Department are needed. The Federal Research effort will begin turning toward assessment and energy strategy evaluation in coming years; DOE must be a leader in these activities.

Holistic Model

The Subcommittee's concept of the role of the Department of Energy in the issue of global change is illustrated in Figure 1. Energy-producing technologies create CO₂ and other trace gases; when introduced into the environment, these gases have the potential for changing the global climate. Mitigation of these environmental changes will have major societal and economical consequences. There are substantial time lags in the system. A buildup of greenhouse gases produces changes on a time

frame of several decades. The oceans provide enormous buffering, both in their heat storage capacity and their ability to store CO₂, which further adds to the time lag.

We recommend that DOE develop an analysis methodology, or model, for all of the elements shown in Figure 1. This model would provide a tool to structure thinking, and to assess the environmental and economical ramifications of various policies. The model would need input from a wide spectrum of DOE activities. Within the holistic model we have included those activities which we believe are the purview of Energy Research in the boxes marked "processes" and "effects"; the remaining boxes are activities which would be supported in other parts of DOE. Alternative technology development, including consideration of conservation as an option, should be an important aspect of an integrated DOE global change program.

RECOMMENDATIONS:

I. A Global Change Coordinator for the Entire DOE

The Subcommittee is concerned with how the total global change effort is integrated throughout the Department. We believe integration is best accomplished by establishing a Coordinator for Global Change within the Office of the Secretary. A precedent was the establishment of a Secretarial Special Assistant for Coordination of Defense Waste Management; we envision a similar Coordinator for Global Change. One of the principal duties of the Global Change Coordinator would be to ensure that all of the elements contained in the holistic model (Figure 1) are implemented and continuously monitored. This can only be done in full cooperation with other federal agencies working on the problems of global change. A second principal duty of the Coordinator would be to ensure that energy technology development proceed hand in hand with the research on global change.

We have identified several additional activities that the DOE Coordinator should ensure the Department addresses promptly:

Strategy for Global Change Research

Global change involves a new vision of the earth's environment; the international scientific community is viewing the earth's global environment as a single system with various complex subsystems, including human activities. **The scientific community is asserting that it is not enough to look at a small set of subsystems; we must look at the total global environment if we are to understand it.** This new construct should be considered in the development of long-term research plans. DOE should develop and periodically update a specific strategy for investigating global change, i.e., a strategy for how the Department intends to participate in multiple sponsor (domestic and international) global change research.

Assurance of Adequate Long-Term Data Records for Global Change Research

A number of anthropogenic inputs are believed to cause global change. Those most frequently mentioned in their order of priority are: energy use and associated atmospheric gas generation; use of halocarbons, of which the most important are the chlorofluorocarbons; agricultural practices; land-use modifications; other industrial practices. In order to assess the causes and effects of global change, the scientific community needs a reliable, long-term data base which measures changes in the driving inputs. DOE needs good data on all of the driving forces for change, especially the greenhouse gases. It is not necessary that DOE collect all the needed data, nor finance such an effort. DOE does, however, need to ensure that adequate, reliable data sets are being acquired and maintained by someone in the community. A key example of needed data is the carbon budget. Table 1 is a current estimate of the global carbon reservoir.

TABLE 1. ESTIMATED TOTAL CARBON RESERVOIR

	TOTAL CARBON (10 ²¹ grams)
ATMOSPHERE	0.036
OCEANS (Inclusive of Biomass)	6.4
CONTINENTS (Inclusive of Biomass)	7.3
CARBONATE ROCKS	
OCEANS	28
CONTINENTS	54
METAMORPHIC ROCKS	<u>10</u>
TOTAL	106

The carbon fluxes to and from each of the reservoir elements, rather than the absolute size of the reservoirs, are important in assessing human impacts. The active carbon reservoirs, in both the continental biomass and the oceans, are each approximately 200 times larger than the carbon in the atmosphere. It is obvious from Table 1 that the carbon in the oceans and on the continents is very important in the CO₂ problem. A large time lag is associated with the mixing and release of carbon dioxide from the deep ocean. In order to assess the fluxes in and out of these reservoirs, studies of temporal changes are necessary. These studies require long sequences of synoptic data. Such data, especially from the oceans, are difficult and expensive to obtain.

It will take leadership on the part of DOE to ensure that appropriate data are currently collected and that data acquisition continues into the future. Data collection in the FY 90 Federal Research program constitutes only 8 percent of the total research effort; this seems small, given the nature of the problem.

II. DOE Role in Interagency Coordination of Global Change Research

The Committee on Earth Sciences is playing a key role in coordinating the Federal Government Research endeavor. DOE should actively participate with CES, and it should take a leadership role on CES initiatives. In order to fulfill its responsibilities for energy strategy development and updating, DOE must assign senior staff capable of providing leadership in the coordinating activities currently underway, especially within CES. As one example, DOE has been designated the lead agency in CO₂ research. As the leader, the Department should work actively to coordinate and cooperate in the research endeavor.

DOE should also assume a leadership role in the analysis and implementation of compatible alternative energy sources. We note that a number of bills have been introduced in Congress which would limit CO₂ in the atmosphere, thereby constraining energy production. Unless there is constructive, thoughtful leadership in this area the country may have inefficient or impractical constraints imposed on energy production.

The Subcommittee was especially concerned about the institutional barriers which appeared frequently in our review of the DOE programs. There were obvious considerations of turf among some DOE managers. We are not trying to place blame, nor to point fingers at particular individuals; "turf" seems to be a fact of life, especially when resources are scarce. The Subcommittee notes that DOE should give priority to the elimination of such intramural institutional barriers.

III. RESEARCH SCOPE AND COORDINATION NEEDS WITHIN ENERGY RESEARCH (ER)

The Global Change Research Program at DOE is centered in three principal programs:

1. **CO₂ Studies**
2. **Continental Ocean Margin Flux Study (COMFS)**
3. **Terrestrial Ecology (in which we include Theoretical Ecology).**

Each of these programs was reviewed by the Subcommittee, and is discussed briefly below.

It was apparent to the Subcommittee that the CO₂ program was considered by ER as its principal global change research effort. It was our judgment that both COMFS and the ecology research efforts will also provide needed inputs to long-term global change research. We perceived that these three research efforts are proceeding more or less in isolation from one another without integration into a total DOE global change research effort. In our judgment, **there is a need for better coordination of the global change research within ER. We believe a global change research coordinator should be appointed within ER, responsible to the Director of ER.**

CO₂ Studies

The CO₂ program is the flagship of DOE activities in global change. DOE has the largest federal program in CO₂ research. The research is subdivided into:

- 1. Carbon Cycle**
- 2. Climate Studies**
- 3. Ocean CO₂**
- 4. Vegetation Studies**
- 5. Resource Analysis**
- 6. Experimental Investigations**

Recent decisions have been made to phase out the Resource Analysis area and to scale back the Carbon Cycle research in order to initiate the **6th area of Experimental Investigations designed to quantify the link between the radiative balance and atmospheric temperature.** Comments on each of the current research elements, and the decisions to change emphasis among them, follow.

Carbon Cycle

The scientific community is still unable to balance the annual global carbon cycle. This indicates obvious holes in our fundamental understanding of carbon and how it is cycled globally. The Subcommittee therefore regrets that it appears necessary to scale back carbon cycle research in order to initiate the Experimental Investigations.

Climate Studies

The focus of the climate studies is an intercomparison of the major General Circulation Models (GCMs) being led by a group at Lawrence Livermore National Laboratory (LLNL). The GCMs are central tools in examining global climate change. They attempt to capture mathematically all of the important processes underlying global atmospheric processes, and ultimately climate. They incorporate several feedback mechanisms, which have the potential to amplify, retard or reverse the responses to anthropogenic stresses. There are four major U.S. groups actively conducting research in GCMs; these are:

1. NOAA Geophysical Fluid Dynamics Laboratory in Princeton (NCAR)
2. National Center for Atmospheric Research (NCAR)
3. NASA Goddard Institute for Space Studies (GISS)
4. Oregon State University (OSU)

In general, the models project global average temperature increases in response to CO₂ and trace gas increases; however, the outputs of the various models are quite different when viewed on continental or smaller spatial scales. The models differ in how they incorporate the major processes, such as cloud and precipitation formation.

The LLNL group is engaged in a systematic comparison of GCMs. This comparison has led to a reexamination of the fundamental processes incorporated in the models. By means of these studies, the scientific community is beginning to gain insight into why the different GCMs produce differing results.

It is the Subcommittee's judgment that GCM analysis is a key tool to understanding global change. The computer models are the only viable tools for integrating and analyzing the entire global climate system. We believe the intercomparison underway at LLNL is productive and should continue to be adequately supported.

Ocean CO₂

The enormity of the oceanic reservoir (200 times larger than the atmosphere; see Table 1) shows the importance of the ocean in buffering CO₂ in the atmosphere. The transport of dissolved and particulate carbon into the deep waters of the oceans is a major factor in the long-term removal of carbon from the atmospheric pool. Furthermore, the distribution of carbon in the ocean shows great global variation. Periodic synoptic global-ocean samples of carbon as well as other nutrients are desirable; these could be integrated to assemble a time history of carbon and CO₂ in the oceans. This is expensive undertaking, especially were it to be done on a stand-alone basis. At the moment, the Ocean Circulation Experiment (WOCE) is being initiated. It is important that the appropriate geochemical measurements in the ocean be integrated with WOCE. The Global Ocean Flux Study (GOFs) is designed to address this problem of the flux of carbon and nutrients in the world oceans. We believe it is important that DOE supports GOFs. DOE's marine program, COMFS (discussed in the next section), can supply a critical near-shore continental margin component to this effort.

Vegetation Studies

The terrestrial (vegetative) carbon reservoir is approximately equal to the oceanic carbon reservoir (see Table 1). There are numerous uncertainties concerning the vegetative carbon reservoir. For example, there is a debate about the net carbon fluxes when equatorial rain forests are cut down. There are also significant questions about vegetative responses to increases in atmospheric CO₂. These are important areas for further research.

Resource Analysis

Resource analysis has focused on the economic and societal impacts and on potential substitution of non-carbon based energy sources. These are important considerations for DOE. In the past these were included in the CO₂ program because they were not being addressed elsewhere within the Department. We can understand the phasing out of these activities in ER, assuming that they are being picked up somewhere else within the Department. The Subcommittee's judgment, as expressed by Figure 1, is that the resource analyses and all its ramifications are critical Departmental functions and must be accommodated.

Experimental CO₂ Investigations

A set of experimental investigations is to be started within the CO₂ program initiated by the Director of Energy Research. This activity has been described as the search for a "smoking gun" experiment, an experiment that would prove a direct link between the buildup in greenhouse gases and an increase in global temperature. The Subcommittee is skeptical that a single experiment, or even a set of experiments, can be designed that will be conclusive. On the other hand, a number of the processes

that link inputs to effects are not well understood; **experiments to define better the basic processes would certainly be worthwhile.**

A workshop at Germantown, Maryland, April 24-25, 1989, attempted to outline a set of worthwhile experiments focused on radiation-climate linkages. These experiments fell into three broad categories:

- I Measurement of Atmospheric Conditions
- II Radiation Theory and Measurements
- III Climate System Feedback

Out of the workshop came an outline for a set of narrowly focused experiments which the Subcommittee also felt were worthwhile.

CONTINENTAL OCEAN MARGIN FLUX STUDY

The continental margins of the world's oceans are the most biologically productive regions; it is estimated that half of the productivity of the oceans occurs near the continental margins. A large fraction of the CO₂ dissolved in the oceans is sequestered biologically; a debate continues about how large that fraction is. In addition, most of the nutrients available in the deep oceans are transported across the continental margins. Understanding the processes that occur at the margins is an important component in understanding the global oceans, especially in understanding the flux of nutrients to the ocean.

The oceanographic community has traditionally focused its research on the deep ocean. The COMFS program is DOE's continued commitment to the understanding of the transport and circulation of carbon at the continental margin.

COMFS is currently investigating the transport of particles, especially carbon, on the North Atlantic Margin. This work complements the first phase of the WOCE and GOFs, which are also slated for the North Atlantic Ocean. The other current investigation under COMFS is an area of coastal basins on the margin of Southern California.

The Subcommittee feels that the Atlantic COMFS investigation is important. This DOE research is filling a hole in our understanding of nutrient cycling, especially particulate carbon in the oceans. A great deal of emphasis has been placed upon studying the carbon cycle. The cycling of other nutrients, including nitrogen, phosphorus, potassium, sulfur and iron, is also important. The COMFS geochemistry should be expanded to sample the total suite of nutrients in the water column.

The Subcommittee would assign a lower priority to the Southern California work, since it does not appear to be directly linked to studies in the deep ocean. If the COMFS program could be expanded, we would suggest studies of the Bering and Chukchi Seas. This region contains the largest continental margin area in U.S. waters; it is among the most biologically productive in the world; and there is a potential for a substantial transport of the biogenic carbon northward from the Chukchi Sea margin into the Arctic Ocean basin. The coupling between this margin and the deep Arctic Ocean waters through thermohaline convection affords a mechanism for the removal of biogenic and inorganic carbon into the deep ocean.

The COMFS program, as indicated above, fills an important gap by providing an understanding of the productivity of and the transport across the continental margins. Such information is critical to an overall understanding of the chemistry and biology of the oceans. Clearly, the COE COMFS program should be closely integrated with WOCE and GOFs, which are focused on the deep ocean.

TERRESTRIAL ECOLOGY

Changes in climate will impact terrestrial ecosystems, and it is important that the possible responses of terrestrial ecosystems be well understood. For example, the amount of carbon fixed by photosynthesis may potentially change due to an increase in atmospheric CO₂. Some experiments suggest that plant communities would fix more CO₂ in an enhanced CO₂ environment. To date most of the experimental results are on single plants, or on a small number of plants. These results must be scaled up to the level of entire ecosystems.

The ecological studies can be subdivided into:

1. **Observations** of natural ecosystems in a variety of environments for the purpose of detecting change, and
2. **Process studies** to determine ecosystem responses to stress, including increased levels of atmospheric trace gases. (We would include with the process studies the necessary theory -- Theoretical Ecology, in DOE's terms).

The Subcommittee was briefed on a proposal to establish ecosystem observations at a number of DOE facilities that are distributed in a transect from the humid southeast (Savannah River), to the arid northwest (Hanford). The advantage of such ecosystem observations at these facilities is that there are large tracts of relatively secure land. Some systematic observation records dating back to the establishment of the facilities are available; these could provide a basis to detect ecological responses to global change. The Subcommittee was impressed with the potential for such ecosystem observations at existing DOE facilities. However, the Subcommittee is not prepared to recommend this program as a higher priority than other possible approaches to the development of long-term ecosystem observations.

RECOMMENDATIONS FOR RESEARCH EMPHASIS -- SUMMARY

The Subcommittee was favorably impressed by the research projects currently being supported by DOE. We did observe a lack of communication, along with a lack of an overall strategy, for the research effort as it contributes to Global Change issues.

Some research areas should be considered for special emphasis; these include:

- o **Geochemical observations of CO₂, other nutrients, and trace gases in the deep oceans, as part of GOFs.**
- o **Experiments to enhance understanding of the important processes which produce global change and that are critically important to the GCMs such as the fluxes of radiatively sensitive gases to and from the earth's surface, the role of aerosols on cloud formation, distribution and optical properties of clouds, etc.**
- o **Comparison of the structures of GCMs to understand better the differences between models.**
- o **Large-scale ecosystem experiments to understand their response to large-scale environmental stresses.**
- o **Studies of nutrient cycling on the continental margin, including both carbon and other major nutrients.**
- o **Continued investigation of the carbon cycle. Current understanding of the global carbon budget and its annual variability is insufficient.**
- o **Theoretical studies directed at developing the conceptual basis for global change research.**
- o **Coupling of ecological response models to the outputs of the GCMs.**

APPENDICES

1. Subcommittee Membership

2. Initial Meeting, October 3-4, 1988, Menlo Park, California
Minutes

3. Meeting, January 27, 1989, Reston, Virginia
Agenda
List of Attendees

4. Meeting, May 22-24, 1989, Washington, D.C.
Agenda
List of Attendees

DEPARTMENT OF ENERGY
CHARTER
BASIC ENERGY SCIENCES ADVISORY COMMITTEE

1. Committee's Official Designation:

Basic Energy Sciences Advisory Committee (BESAC)

2. Committee's Objectives and Scope of Activities and Duties:

The activities of the Basic Energy Sciences Advisory Committee include:

- a. Periodic reviews of elements of the Basic Energy Sciences (BES) program and recommendations based thereon;
- b. Advice on long-range plans, priorities, and strategies to address more effectively the scientific aspects of energy-related basic energy sciences issues of departmental policies and programs;
- c. Advice on appropriate levels of funding to develop those plans, priorities and strategies and to help maintain appropriate balance between competing elements of the BES program;
- d. Advice on scientific aspects of basic energy sciences issues of concern to DOE as requested by the Secretary or the Director of Energy Research.

3. Time Period Necessary for the Committee to Carry Out Its Purpose:

In view of the goals and purposes of the Committee, it is expected to be continuing in nature.

4. Official to Whom This Committee Reports:

The Committee will report to the Secretary of Energy through the Director of Energy Research.

5. Agency Responsible for Providing Necessary Support for This Committee:

The Department of Energy. Within the Department, primary support shall be furnished by the Office of Energy Research.

6. Description of Duties for Which the Committee is Responsible:

The duties of the Committee are solely advisory and are stated in Paragraph 2, above.

7. Estimated Annual Operating Costs in Dollars and Person-Years:

\$80,000; 1/2 person-year; plus meeting and travel costs of \$50,000.

8. Estimated Number and Frequency of Meetings:

The Committee will meet periodically, approximately 4 times a year.

9. Committee's Termination Date (If Less Than 2 Years from Date of Establishment or Renewal):

Not applicable.

10. Subcommittee(s):

To facilitate the functioning of BESAC, subcommittees may be formed. The objectives of the subcommittees are to make recommendations to the parent committee with respect to particular matters related to the responsibilities of the parent committee. Ad hoc members from outside BESAC may be appointed by the subcommittee chairperson to ensure the expertise necessary to conduct the subcommittee's business.

11. Members:

- a. Committee members shall be appointed by the Secretary of Energy.
- b. Approximate number of Committee members: 17
- c. Committee members may not serve for more than 4 consecutive years.

12. Chairperson:

The Chairperson shall be appointed by the Secretary of Energy.

This Charter for the Advisory Committee named above is hereby approved on:

OCT 11 1988

Date

A handwritten signature in black ink, appearing to read "H. Raiken", written over a horizontal line.

Howard H. Raiken
Advisory Committee Management Officer

OCT 11 1988

Date Filed