REPORT

of the

ENVIRONMENTAL ENGINEERING COMMITTEE

SCIENCE ADVISORY BOARD

U. S. ENVIRONMENTAL PROTECTION AGENCY

on their review of the

"SAMPLE" RESEARCH PLANS OF THE

OFFICE OF ENVIRONMENTAL ENGINEERING AND TECHNOLOGY

October, 1985

INTRODUCTION AND HISTORY

At a meeting of the Environmental Engineering Committee of the Science Advisory Board on February 26, 1985, Mr. Carl Gerber, Director of the Office of Environmental Engineering and Technology (OEET) briefed the Committee on a program of research planning being undertaken by OEET. He explained that five-year research plans are being prepared for 27 topics currently under study by OEET. These include 10 topics on hazardous wastes and Superfund programs, 8 topics on water and toxics/ pesticides programs, and 9 topics on air/energy programs.

The purpose of these five-year research plans will be to describe the EPA/ORD programs to EPA program offices, the scientific and engineering community, and other interested groups. They will also serve as the basis for budget discussions and defense, although they are not intended to be budget documents themselves. They are intended to provide a technical perspective of what research is necessary to support the Agency's current and future mission and regulatory needs.

At his February 26, 1985 briefing session, Mr. Gerber presented drafts of three of these five-year research plans; namely, the Hazardous Waste - Land Disposal research plan, the Drinking Water research plan, and the Limestone Injection Multistage Burner (LIMB) research plan. He requested the Environmental Engineering Committee to review these as examples of the planning process and to provide comments, particularly on the overall approach.

The Environmental Engineering Committee has had a deep concern for the engineering and technology component of the EPA/ORD research program. The Committee prepared a resolution highlighting its concern about the reduced emphasis on control technology research in EPA and recommending to the Administrator that the trend be reversed. This resolution was approved by the SAB Executive Committee and submitted to Administrator William B. Ruckelshaus on October 13, 1983. The Committee was therefore quite ready to take on the review requested by OEET. A subcommittee of three committee members was asked to review the "sample" plans and report back to the full committee. Dr. Ben B. Ewing reviewed the Hazardous Waste - Land Disposal plan. Dr. Charles R. O'Melia reviewed the Drinking Water research plan. Mr. George P. Green reviewed the LIMB research plan. Their written comments were circulated to the full committee and the review was discussed at the Environmental Engineering Committee meeting on June 13 & 14, 1985. The following comments summarize the review.

GENERAL COMMENTS

The Committee applauds OEET for its development of these and the other five-year research plans. The three which were reviewed are sensitive to the Agency program offices' needs. They are well done and will be helpful

in describing the present and future research of OEET to the program offices and to the scientific and engineering community. The five-year planning period is appropriate in that it provides for some continuity which is compatible with the Federal budgeting cycle; yet it does not extend so far into the unseen future as to lose its reality.

The plans do focus on present activities and those studies needed in the next few years to continue progress in current directions. Perhaps they should also include a speculative component designed to identify potential new problems and their solutions.

HAZARDOUS WASTE RESEARCH PLAN

The draft of the Hazardous Waste Research Plan - Land Disposal is one of a series of plans dealing with hazardous waste research. Without the opportunity of reviewing the companion plans, it is difficult to determine just what is properly included in this plan an? what apparent omissions may actually be covered in other plans. Comments on the draft as it was presented will be made first, and then some comments will be made about additional topics which are important but which may be scheduled for some other part of the planning process.

HAZARDOUS WASTE-LAND DISPOSAL PLAN

1. It is not clear that the research plan has taken into consideration the provisions of the recent amendment to RCRA, and particularly the banning of land disposal of hazardous wastes.

2. Page 2, paragraph 4; "The program will also develop user friendly artificial intelligence systems that will standardize the review of applications submitted to the Agency. These systems will be based on the experience of experts and field proven techniques..." Also page 7, last paragraph; "Calculations to determine the adequacy are difficult for the uninitiated. As part of the overall program to provide permit writers with the latest technology, a user friendly interactive computer program with default values will be developed...." we recognize the practical importance of evaluating large numbers of permit applications with rapid turnaround, and the constraints on permit writers. We are leery of the complete dependence on a computer system or artificial intelligence when evaluating such complex systems of social importance. Great caution is necessary in this situation. Perhaps the computer program should be designed to throw out any unusual results and flag them so the permit application can be referred to an expert or a panel of experts. The computer program could screen permit

applications which are not routine so that they could be subject to special audit, in the manner of the IRS income tax return processing.

3. Page 5, paragraph 2; One other area with research potential is the use of genetic engineering to develop biological cultures for treatment of leachates or for detoxification of wastes, particularly liquid wastes, as part of the waste modification program.

4. Page 6, paragraph 3; Previous experience with attempts to correlate laboratory porosity or permeability with field conditions have not been encouraging. The research effort is important and justified, however.

5. Page 7, paragraph 2; Techniques for detection of leaks in liners and repair of damaged liners are very important to insuring the integrity of the containment and thus very important to maintenance of public confidence and acceptance of land disposal facilities. This program should be emphasized.

Additional Comments

There are some additional research needs which have not been covered in this research plan. OEET may intend to cover them in one or more of the related hazardous waste research plans. In that event discussion of them here may be superfluous. They are raised for discussion, however.

There is need for further research to define hazardous wastes, particularly in view of the RCRA amendments. Can the EP Toxicity extraction procedure be demonstrated to be reliable in predicting leachability of wastes under field conditions? Can bioassay techniques be developed to establish the toxicity for a wide range of systemic toxins, carcinogens, mutagens, teratogens, neurotoxins, immunotoxins, etc.?

2. Improved monitoring techniques are needed. Better sampling methods to avoid cross-contamination of samples should be developed. Improved analytical methods, particularly for part-per-trillion concentrations in complex mixtures, are needed, as are quality control/quality assurance procedures. Monitoring protocols for long-term surveillance need to be developed. The importance of being able to monitor for leaks in flexible membrane liners or clay liners without waiting for ground-water contamination to detect it has been commented on above.

3. Waste reduction, recycle, and recovery is not discussed in the research plan. There is need both for research to develop new technology and research to improve understanding of motivation for implementation of waste-reduction measures.

New incentives must be discovered and better understood in order that they can be used to enhance waste reduction opportunities.

4. The plan does not discuss alternative technologies for hazardous waste disposal, such as incineration, chemical oxidation, neutralization, or precipitation, demineralization, photolysis, or biological waste treatment. Again, comments above dealt with the application of genetic engineering to improve biological treatment processes for refractory compounds and mixtures.

5. The Hazardous Waste - Land Disposal plan does not discuss the need for research to develop siting criteria and methods of assessment of sites. It also seems to neglect the control of subsidence of the landfill site after closure. Finally it does not deal with the problems of synergism, antagonism, or interaction of mixtures of complex wastes.

6. The plan does not deal with research aimed at improved control of pits, ponds and lagoons. There are many of these existing throughout the country. Technology for solidification, or (even better) fixation, is needed.

DRINKING WATER TREATMENT RESEARCH PLAN

Four topics for future research are addressed. In approximate order of importance they are (1) trace organic substances, (2) disinfection byproducts, (3) microbiological contaminants, and (4) small water supplies. The report includes summaries of present work in these areas and also for inorganic substances and radionuclides in water. Future proposed studies emphasize organic chemicals, and include a significant amount of activity at field scale.

The plan is well formulated to continue present directions, with emphasis on moving to field scale testing. Without being comprehensive in suggesting additional directions, the following are suggested for consideration.

1. Field scale testing is as expensive as it is necessary. The problems addressed and the sites selected require considerable expertise and deliberation. If not presently available, criteria for these selections could be developed; participation in funding could be a factor.

2. A goal of the research plan is to contribute to the development of "a defensible basis for standards that apply to public water systems." More stringent standards for contaminants presently regulated (e.g., turbidity) and new standards for unregulated

contaminants can be expected. This suggests needs for new technology and adaptations of present practice. These are not addressed in sufficient depth in the plan.

3. Raw water supplies comprise a very diverse set of problems. Both water quantities (demands) and water qualities (types and concentrations of contaminants) vary widely from place to place and, at many locations, from time to time. The treatment systems appropriate to transform these diverse supplies into potable waters meeting a uniform set of standards can also be expected to differ appreciably among themselves. As our recognition of problems broadens and our regulatory standards expand to meet these newly perceived needs, it is reasonable to expect that treatment technology should diversify considerably. The proposed research plan does not appear to address this need adequately.

LIMB RESEARCH PLAN

The LIMB Research Plan is a very good plan and stands an excellent chance of having a definite impact on the commercial sector. Even though the Plan appears to be very good, some hopefully beneficial comments are presented.

Page 2, last paragraph. "The utility industry will not accept this technology, etc...."

There is generally the feeling that the industry would prefer to have technology fully demonstrated. However, when considering air quality control equipment, there is more the tendency to move ahead on new technologies prior to successful demonstration. It is unfair to proceed on the basis that the technology would not be used until LIMB has been successfully demonstrated. There is an opportunity at the present time for OEET to get directly involved with at least nine separate furnace injection demonstrations, and of these nine, six are in the category of 100 megawatts or larger. These utilities have already indicated their interest to the Department of Energy for participation in prototype tests of sorbent injection as a response to DOE's solicitation under the clean coal technology exercise. Each one of these utility proposals may represent a unique project that would enhance EPA's efforts and aid in the financial support of the technology. EPA support of some of these projects would be appropriate, either in conjunction with or in lieu of, DOE support and definitely has merit in cooperation with the utilities' coal and sorbent suppliers, boiler manufacturers, state agencies and the Electric Power Research Institute. Our understanding of the interest that has been expressed on behalf of the utilities is that the LIMB technology is feasible. The real' question to be resolved is one of economics. A part of EPA s long-range plan should be an expression of interest in the projects that have already been proposed as part of the DOE solicitation.

Page 3, first paragraph under Generic Research and Development.

The Plan has stated that the research required for achieving the desired levels of NOx removal has been completed. OEET should address this question further as NOx levels may have been demonstrated; however, for a long-term operation of utility boilers within the U.S., the question of slagging and fouling has not been properly resolved. The efforts to provide an understanding of the fly ash/sorbent mixtures should continue to be reviewed for both the continued work on SO2 and NOx.

Page 4, paragraph 1 "...tests have indicated that the LIMB SO2 removal goals can be met with at least two alternate approaches..."

Caution is merited here since other, apparently equally promising, approaches have not always lived up to their expectations.

Page 4, paragraph under Prototype Testing.

The benefit of extensive testing in large-scale laboratory furnaces is questionable. The chemical processes of sorbent activation, deactivation, and sulfation can be extrapolated from pilot to full-scale systems, so the primary remaining performance issue is mixing/dispersion. Some intermediatescale testing would be useful, but only if conducted on units with representative cross-sectional temperature and velocity distributions, not just correct temperature-time histories.

Page 5, paragraph under Technology Generalization.

The joint EPA/EPRI Conference on LIMB which was held in November 1984, was very successful and the Research Plan should contain some specific references to continuation of the technology transfer and future conferences.

Page 5, last paragraph, Level of effort to "...understand the enhancement mechanism..."

The projected budget seems high (over $1M for FY86-88 beyond the $3M spent in FY84-85, according to the second line item under PPA 01). A measured, phased approach may produce a more cost-effective R&D program (i.e., a building block approach instead of parallel activities), and losses would be minimized if the proposed enhancement mechanism is found to be less effective than expected or more difficult to apply.

Downplaying the concern about potential inhibition of the sorbent mechanism due to mineral matter interaction is also recommended. This apparent effect has not been observed by all researchers. Further, even if real, its importance is probably diminishing with the current trend towards injection into the upper furnace.

Page 6.

Significant conceptual process design and analysis is strongly urged before committing to experiments on sorbent recycle or utilization. The primary near-term emphasis should probably be on waste characterization and feasibility studies of the concept. Because of the commercial potential of the spent sorbent, industry should participate technically and financially in the utilization efforts as the market place now will accept the techrology and the need for the furnishing of these sorbents.

Prototype plans -- We agree in principle, but only if the test units have the same thermal profiles and aerodynamics as typical large boilers.

Demonstration plans -- It is not yet clear that the SO2 part of the LIMB process will behave differently in tangential- than wall-fired units. The differences may be no greater than the site-specific variations between different designs of wall-fired boilers (e.g., divided wall, front versus opposed firing, convective pass design, coal properties that affect the optimum location for injection). Hence, the decision to conduct full-scale demonstrations on both wall- and tangential-fired units should await the outcome of the field tests on smaller (e.g., 20 to 60 MW) units.

The technology generalization and process modeling is very good; however, a greater emphasis should be placed on the use of existing boilers rather than spending too much money in attempting to develop a purely predictive model based on early research. Models should be developed to be largely correlative.

The approach is very good and the program should be directed toward the acceptance and commercialization of the technology. Future efforts should be carried out in close cooperation with the ultimate users, namely, the utilities and EPRI. The time frame to achieve this work appears realistic; however, there is always the basic question "Are there sufficient funds available?"