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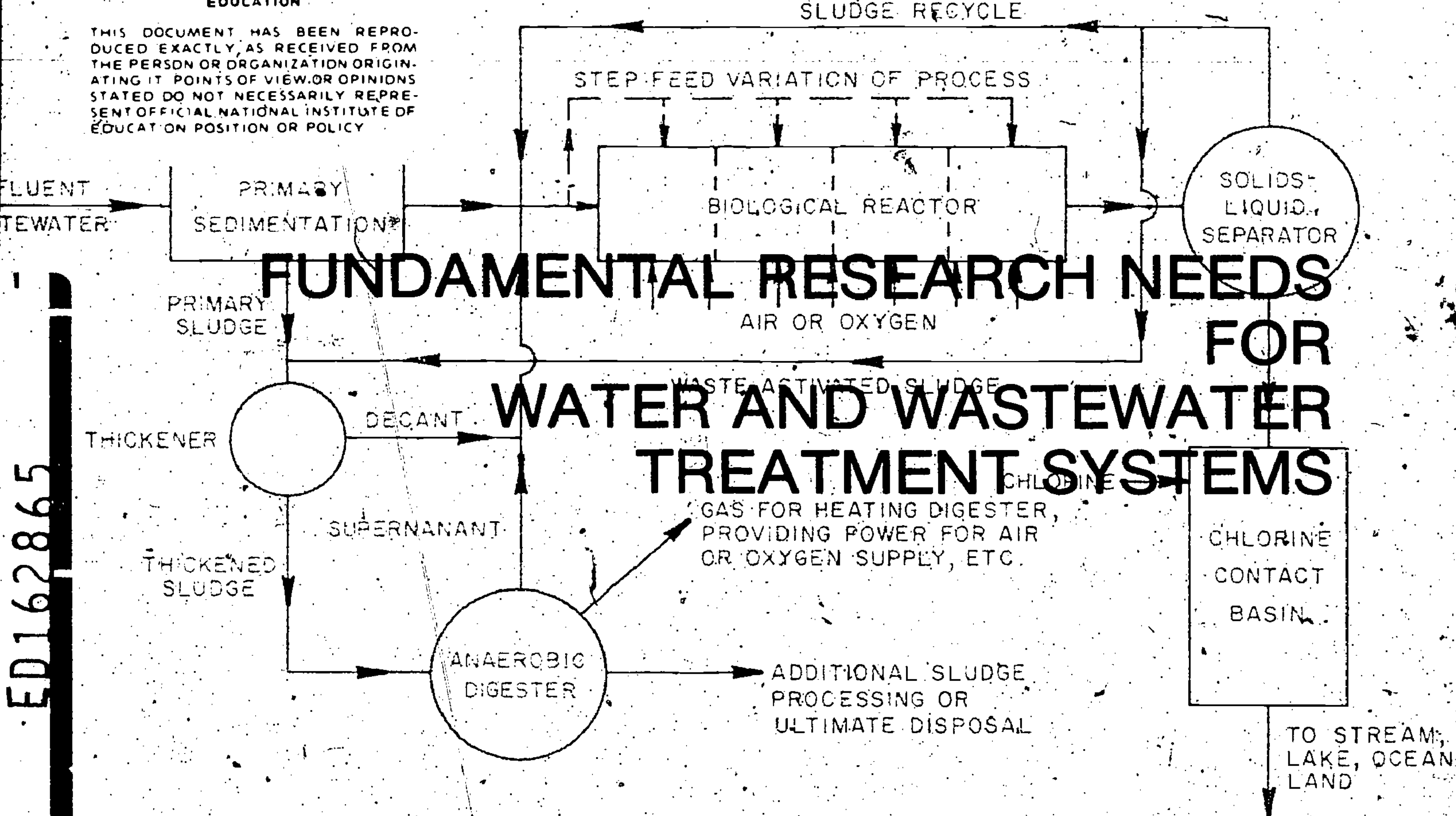
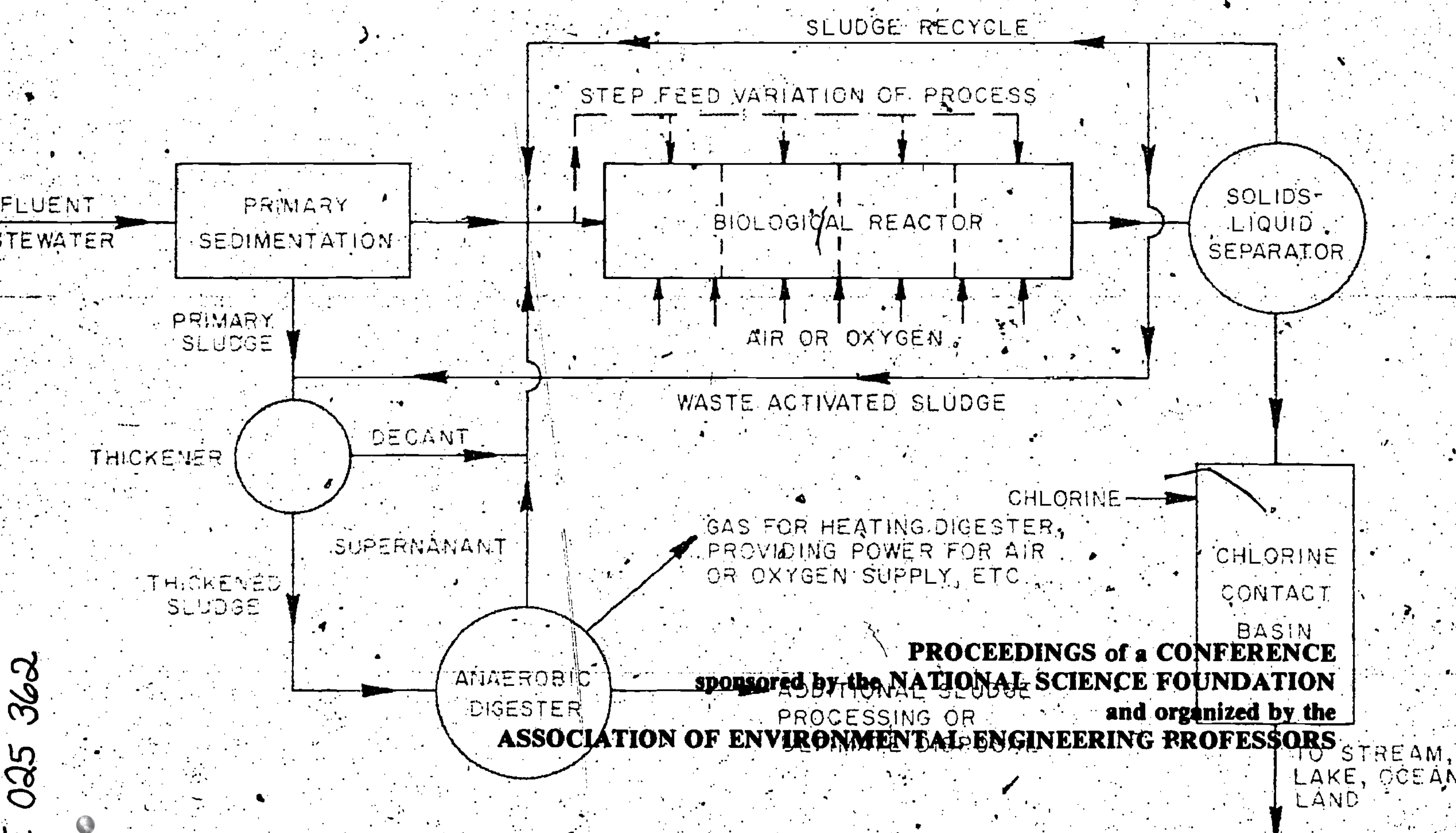
ABSTRACT

Papers are presented identifying fundamental research needs in water and wastewater treatment by industrial users of technology, industrial users of research, a municipal water department, a consulting engineer, Congress, and the EPA. Areas of research needs addressed include: (1) microbial, viral, and organic contaminants; (2) biological processes; (3) physiochemical processes; (4) control of wastewater treatment plants; (5) sludge disposal systems; and (6) water quality. In addition, research activities in environmental engineering being conducted by the National Science Foundation are identified. (BB)

FUNDAMENTAL RESEARCH NEEDS FOR WATER AND WASTEWATER TREATMENT SYSTEMS





PROCEEDINGS of a CONFERENCE
sponsored by the NATIONAL SCIENCE FOUNDATION
and organized by the
ASSOCIATION OF ENVIRONMENTAL ENGINEERING PROFESSORS

FUNDAMENTAL RESEARCH NEEDS FOR WATER AND WASTEWATER TREATMENT SYSTEMS

PROCEEDINGS of a CONFERENCE
held at Arlington, VA, December 15, 1977

Sponsored by the NATIONAL SCIENCE FOUNDATION
and organized by the
ASSOCIATION OF ENVIRONMENTAL ENGINEERING PROFESSORS

J. F. Andrews and T.M. Keinath
Conference Co-Chairmen

J.H. Sherrard
Editor of Proceedings

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FOREWORD

The need for additional water and wastewater treatment facilities and for the improvement of the design and operation of such facilities is well known and extensively documented in the popular press, the scientific and engineering literature, and governmental publications. Congress has responded to this need through legislation which has resulted in the appropriation of several billions of dollars and it is expected that high expenditures will be required for several years. However, it is not so well known that research efforts on water and wastewater treatment have lagged far behind efforts related to treatment plant construction. One for the major reasons for this discrepancy is inadequate financial support for research, as documented in the 1974 Comptroller General's Report[1] to the Congress on "Research and Demonstration Programs to Achieve Water Quality Goals: What the Federal Government Needs to Do." A quote from this report is as follows:

> "Municipal technology R&D funding has decreased 64% over the last 7 years, while funding of EPA's construction grant program has increased about 3,200 percent."

Although declining financial support for long-term, fundamental research is a problem throughout the scientific and engineering community, the problem is even more serious in the environmental engineering area. Traditionally, this discipline has received research support from the Environmental Protection Agency and its predecessor agencies. However, since the creation of the EPA in 1970, funding has been primarily for short-term projects designed to support the mission of a regulatory agency thus leading to a neglect of long-term, fundamental research. The current position of the EPA with respect to the type of research supported is best illustrated by quoting from the 1976 EPA Office of Research and Development Report to Congress[2] on "Environmental Research Outlook: FY 1976-1980."

> "EPA's overall research program must support the mission of a regulatory agency. Specific research objectives and priorities derive from objectives and priorities that EPA establishes in fulfilling its total legislative mandate. Accordingly, the research program is "mission oriented" with emphasis on production of timely and quality outputs—i.e., research results that are directly useful to environmental decision-makers, regulatory officials and polluters."

This Conference was organized to direct the attention of those in government, in academia, and in the private sector to the need for increased efforts in long-term, fundamental research on water and wastewater treatment systems. Moreover, it is hoped that through the definition of specific research needs, a larger portion of the research talents in the environmental engineering profession will be diverted to fundamental research thus reversing the trend toward short-term, immediate-application research which has come to dominate the field. Although no one questions the need for short-term research, a better balance between the two types is necessary if the problems of water pollution control are to be solved.

Accordingly, the primary objectives of the Conference were to: (1) provide a forum and focal point for the exchange of information on fundamental research needs between the academic community, users of research, and granting agencies; (2) define and establish priorities for these research needs; and (3) disseminate the Conference proceedings for the purpose of stimulating research in high-priority areas. Of course, the ultimate objective of the Conference is to improve the quality of the nation's water resources. Increased attention to fundamental research on water and wastewater treatment represents one of the major avenues by which this objective can be accomplished.

These proceedings represent the integrated best judgement of the experts gathered at the Conference as to long-term, fundamental research needs on water and wastewater treatment systems. It should provide a firm foundation for the development of a national research program in this important area.

John F. Andrews, Cochairman
AEEP/NSF Conference on Fundamental
Research Needs for Water and
Wastewater Treatment Systems

February 1978

[1]Comptroller General of the United States, Report to the Congress—*Research and Demonstration Programs to Achieve Water Quality Goals: What the Federal Government Needs to Do.* Vol. 1, p. 13, January, 1974.

[2]Office of Research & Development, U.S. Environmental Protection Agency Report to Congress—*Environmental Research Outlook FY 1976-1980*, p. 8, February, 1976.

NATIONAL SCIENCE FOUNDATION ACTIVITIES IN ENVIRONMENTAL ENGINEERING

Conference on Fundamental Research Needs
Water and Wastewater Treatment Systems

BASIC RESEARCH IN ENVIRONMENTAL ENGINEERING AT THE NATIONAL SCIENCE FOUNDATION

A.A. Ezra
National Science Foundation
Washington, D.C.

The Engineering Division at the National Science Foundation supports basic research in environmental engineering. As a rough guide to what we mean by basic research, I shall provide the following definition: exploration of natural phenomena of interest to environmental engineering, the attempt to explain them or describe them quantitatively in terms of first principles. It is recognized that engineering research, both basic and applied, is generally problem oriented, and that it is often not obvious whether a particular project is more basic in nature or more applied. When these cases arise, they are discussed by Dr. Ed Bryan and myself to make this determination. I should emphasize that it really doesn't make too much difference to which directorate of the Foundation a proposal is submitted, or whether the proposer thinks his work is basic or applied. What is important is whether his proposal is reviewed favorably. If it is, support will be found somehow, even if it means carrying it over to the next fiscal year, or funding it at a much lower level than originally requested.

Proposals for support of basic research in Environmental Engineering are unsolicited. We allow the research community to tell us what they think is important. However, a very thorough review of the relevant literature must be presented in the introduction to the proposal in order to provide the necessary justification for the proposed research project. Gaps in existing knowledge must be identified, and the relationship of the proposed project to filling or bridging these gaps must be clearly presented. The references cited in the literature review should be alphabetically arranged by author.

The next most important chapter is the technical approach. While we recognize that what appears to be a reasonable technical approach at the beginning of a research project may soon be abandoned in favor of another, we need evidence that enough thought has been given to the subject to make it possible for the proposer to present a plausible technical approach.

The review process is ad hoc. We solicit by mail the services of experts in the field, who review the proposals on a voluntary basis—there is no financial reward for their services. It is wise to remember that a reviewer is generally an author of one or more papers in the field. A less than thorough review of the literature in the proposal may omit reference to one of his (or her) publications—something that he (or she) often regards as evidence of poor scholarship. On the other hand, an unduly critical review of a publication may accidentally wind up in the hands of a reviewer who authored that publication. An unduly harsh review of a proposal is usually checked out for a possible bias due to this sort of accidental occurrence, and the reviewers comments are weighed accordingly.

There is no specified deadline for basic research proposals in Environmental Engineering. They are sent out for review as they come in, and the reviewers are the pacing element in the decision process. Proposals have to be acted on, one way or another, within nine months of being received by the Foundation. Those of you who are requested to be reviewers should keep in mind that your delay may be holding up a very important decision for a young faculty member who is being evaluated for tenure, or for a graduate student whose ability to stay in school depends on the research grant his professor is seeking from us.

While the basic research proposals are unsolicited, and while the content of research proposals funded in the past are not necessarily a guide to the future, it may be of some help to you to know what kinds of basic research projects in Environmental Engineering have been funded by us. With this in mind, the following tables are presented.

The first is a list of program elements, based on the research proposals we have received and funded in the past three years. The relative amounts of support for each element change from year to year, depending on the relative number and quality of research proposals we receive in each element. The total budget for the Water Resources, Urban and Environmental Engineering Program in any given fiscal year is allocated on the basis of both the number and dollar value of the research proposals received in the previous year. For FY 78, this budget is approximately $3.1 million.

Table II summarizes the awards made in FY 77 for basic research projects that are closest to the interests of the Association of Environmental Engineering Professors. The third table is a similar summary for awards made as of December 1977 in FY 78. The relative amounts may be regarded as a possible indication of future directions.

The relationship of these projects funded by the Engineering Division at NSF to the interests of the Applied Science and Research Applications Directorate at NSF or to the Environmental Protection Agency may be visualized as follows. On completion of these projects which are considered to be basic research the principal investigator could well turn to the ASRA directorate at NSF for support to carry his work one step closer to application and ultimately to the EPA. Conversely, applied research projects supported by ASRA, or R&D projects supported by EPA may well reveal deficiencies in basic knowledge which could then lead to basic research projects supported by the Engineering Division of NSF.

In conclusion, this workshop would be considered a success if it helps to crystallize your thoughts in the form of research proposals in Environmental Engineering.

TABLE I

PROGRAM ELEMENTS

- Transport Diffusion and Removal of Pollutants
- Erosion and Transport of Sediment
- Flow Through Underground Aquifers
- Hydrodynamics of Surface Runoff
- Wind-Wave Interaction
- Interaction of Winds and Urban Structures

TABLE II

FY 77 SUMMARY OF AWARDS

ENVIRONMENTAL ENGINEERING

U. of Mo., Columbia	Novak	Influence of Nutrient Concentration, Light and Temperature on Algae
U. of Calif., Davis	Brewer	Sedimentation Basin Performance
U. of Calif., Berkeley	Jenkins	Activated Sludge Floc Structure and Its Relation to Process Performance
M.I.T.	Harleman	Dynamic Analysis of Nutrient Removal by Waste Stabilization Ponds
U. of Ill.	Holley	Transport of Effluents in Rivers
Old Dominion U.	Kuo	Similitude of Mass Transport Processes in an Estuarine Model
Nat. Academy of Sciences	Schad	Partial Support of Committees for International Environment Program
U. of Ill.	Maxwell	Air Bubble Induced Dilution
M.I.T.	Chisolm	Nutrient Uptake and Growth Under Unsteady Conditions
AEEP		Conference on Fundamental Research Needs in Water and Wastewater Treatment Systems

TOTAL FOR FY 77 $482,100
TOTAL PROGRAM BUDGET, FY 77 $3,374,000
ENVIRONMENTAL PERCENT OF TOTAL 14%

TABLE III

SOME FY 78 AWARDS
ENVIRONMENTAL ENGINEERING

SUNY, Buffalo	Middleton	Phosphorus Solubilization During Anaerobic Decomposition of Algae
Purdue U.	Grady	Factors Affecting Organic Microbial By-Products in Activated Sludge Effluents
U. of Houston	Andrews	Dynamic Models and Control Strategies for Wastewater Treatment Plants
Catholic U.	Kao	Effects of Buoyant Pollution Discharge on Water Quality Control in Rivers and Coastal Regions
Stanford U.	Remson	Groundwater Management and Solute Transport Models
Stanford U.	Franzini	Mixing Processes in the Simulation Modeling of Irregularly Shaped Water Bodies
Columbia U.	Gryte	Dewatering of Sewage Sludges by Freeze-Thaw Techniques
Syracuse U.	Barduhn	Eutectic Freezing Process for Wastewater Treatment and Ice Crystal Growth Rates

TOTAL TO DATE FY 78 $541,682

RESEARCH DIRECTED TOWARD APPROPRIATE MANAGEMENT OF WASTEWATER TREATMENT PLANT SLUDGES AND POLLUTANTS OF DIFFUSE ORIGIN

Edward H. Bryan
National Science Foundation
Washington, D.C.

INTRODUCTION

Shortly after Congressional action amending the National Science Foundation statute to authorize its support of applied research, NSF announced and implemented its program of Research Applied to National Needs. The characteristics of the program were described by Dr. Raymond Bisplinghoff in his address to the American Association for the Advancement of Science at its 1972 meeting. He suggested that, in contrast to basic research, problem-focused research is typically externally motivated to satisfy the needs of society. Its role in providing support entitles society to insist that research objectives be influenced, if not totally determined by ultimate utility of the results and that responsibility for selecting research problems was the sponsor's. He further suggested that research directed toward solving complex problems of society was likely, itself, to be complex because the issues involved social and human values. He further indicated the importance of estimating the value of proposed research before its initiation and that emphasis be placed upon important problems. While sharing with any good research the attributes of discovering gaps in knowledge for further intellectual exploration and offering students educational opportunities, success of problem-focused research would be measured by the extent to which it found solutions to the problems addressed. Provision of reasonable balance between support of traditional basic and applied research would continue to recognize investigator-prerogative for decisions as to the nature of their research and not require that all researchers submit to the discipline that is characterisitc of research management by objectives.

RESIDUALS MANAGEMENT

The residuals management subelement within a program titled "Regional Environmental Management" was based upon recognition of the fundamental residuals-resource relationship of life-forms comprising an ecosystem and that effective management of residuals is both a necessary and sufficient condition of achievement of good environmental quality. Representing as they do both a threat to the ecosystem and potential solution to its security, the program strategy has been to couple management of residues with the recovery of their value as a resource as a means to maximize the potential for implementation of solutions to problems of environmental quality.

The resultant program has consisted of research that addresses management of community wastewater, treatment plant sludges, pollutants from diffuse (non-point) sources and selected industrial pollutants. Research projects currently underway or recently completed that address these four issues are summarized in Tables I-IV., inclusive. The diversity of professional qualifications for undertaking research on environmental issues is apparent from this tabular arrangement of information.

COORDINATION ACTIVITIES

The increased need for coordination with mission agencies and other potential users is a distinguishing feature of problem-focused research in comparison with both basic and traditional applied research. Coordination occurs during review of individual proposals, as a result of participation in appropriate interagency coordinating committee meetings and national conferences and by sharing of research costs through interagency and interdivisional agreement. Examples include current cost sharing between the National Science Foundation and the U.S. Environmental Protection Agency in the research on energized electron disinfection of sludge at MIT and the environmental effects of extracting oil from oil shale. The latter project also includes cost sharing with the Department of Energy (Energy Research and Development Administration and the Federal Energy Office) and the Engineering Division, of the NSF Research Directorate. The final award in support of research directed toward characterization of diffuse pollutants entering Chesapeake Bay conducted by the Chesapeake Research Consortium in cooperation with the EPA-Athens, Georgia laboratory was made through the Philadelphia Regional Office of EPA. This was an initial step toward development of the EPA Chesapeake Bay Studies Program.

The award to the University of North Carolina for Dr. Lauria's investigation of the limits of regionalization relating to the collection and treatment of wastewater was made in cooperation with the Engineering Division's Research Initiation Award program. The study by Dr. Okun of regionalization of water management in England and Wales was jointly supported by the National Science and Rockefeller Foundations. The wetland utilization study in Florida under the direction of Dr. Okun has also been jointly supported by NSF and the Rockefeller Foundation.

CONCLUSION

The goal of research directed toward management of residuals has been to reduce to an acceptable level the risks to human and ecosystem health that arise from and are associated with the use of water. The program has been primarily directed toward application of appropriate technologies to achieve correspondence between natural laws that govern the operation of ecosystems and societal mechanisms for management of factors that affect risk to human health or to that of the ecosystem upon which we are vitally dependent.

The control of water pollution is an important aspect of the interface between community water supply and appropriate management of its wastewater. As one of the natural resources generally in public ownership, benefits from community water and threats to them are of public concern. These threats which arise in large part from failures to properly manage societal residues need to be better understood as a prerequisite to being better managed.

Research, whether basic, applied or directed can contribute to better understanding, the application of that understanding to establish its significance and finally its direction toward solutions to problems.

TABLE I
MANAGEMENT OF COMMUNITY WASTEWATER

Investigator	Discipline	Institution	Title of Project
Robert H. Kadlec	Chemical Engineer	University of Michigan	Feasibility of Utilization of Wetland Ecosystems for Nutrient Removal from Secondary Municipal Wastewater Treatment Plant Effluent
Howard T. Odum	Ecologist (Zoology)	University of Florida	Feasibility of Utilizing Cypress Wetlands for Conservation of Water and Nutrients in Effluent from Municipal Wastewater Treatment Plants
Jeffrey C. Sutherland	Geologist	Williams and Works, Inc.	Use of Wetlands for Managment of Pond-Stabilized Domestic Wastewater
Walter Fritz	Civil Engineer	Boyle Engineering Co.	Tertiary Treatment of Municipal Wastewater Using Cypress Wetlands
Thomas D. Waite	Civil Engineer	Northwestern University	Ferrate Ion Disinfection of Municipal Wastewater
Rober L. Irvine	Chemical Engineer	University of Notre Dame	Application of Sequencing Batch Reactors for Treatment of Municipal and Industrial Wastewaters
Wesley O. Pipes	Civil Engineer	Drexel University	Water Quality and Health Significance of Bacterial Indicators of Pollution
Arun K. Deb	Civil Engineer	Roy F. Weston, Inc.	Wastewater Reuse for Regional Management of Water to Meet Urban Needs
Donald T. Lauria	Civil Engineer	University of North Carolina	Regionalization of Wastewater Collection and Treatment—Location, Scale and Construction Sequence of System Components
Daniel A. Okun	Civil Engineer	University of North Carolina	Applicability of Reorganization of Water Management in England to Wastewater Management in the United States

TABLE II
RESEARCH APPLIED TO MANAGEMENT OF MUNICIPAL WASTEWATER TREATMENT PLANT SLUDGES

Investigator	Discipline	Institution	Research Project Title
John G. Trump Edward W. Merrill Anthony J. Sinskey	Electrical Engineer Chemical Engineer Nutrition/Food Science	Massachusetts Institute of Technology	High Energy Electron Irradiation of Municipal Wastewater Liquid Residuals
Theodore G. Metcalf	Microbiologist (Virology)	University of New Hampshire	Disinfection of Enteric Viruses in Municipal Sludge by Use of Energized Electrons
Bernard P. Sagik	Microbiologist (Virology)	University of Texas—San Antonio	Potential Health Risks Associated With Injection of Residual Domestic Wastewater Sludges into Soils
James L. Smith	Agricultural Engineer	Colorado State University	Land Management of Subsurface—Injected Wastewater Liquid Residuals
Mary Beth Kirkham William J. Manning	Agronomist Plant Pathologist	Oklahoma State U U of Massachusetts	Agricultural Value of Irradiated Municipal Wastewater Treatment Plant Sludges
P.C. Cheo	Plant Pathologist	California Arboretum Foundation, Los Angeles Arboretum	Mechanism of Plant Virus Inactivation in Soils Injected with Municipal Wastewater and Treatment Plant Sludges
C. Fred Gurnham	Chemical Engineer	Gurnham and Associates Inc.	Control of Heavy Metal Content of Municipal Wastewater Sludges
Richard I. Dick	Civil (Environmental) Engineer	Cornell University	Process Selection for Optimum Management of Regional Wastewater Treatment Residuals
Roy Hartenstein	Invertebrate Zoologist	State University of New York—Syracuse	Utilization of Soil Invertebrates in Stabilization, Decontamination and Detoxification of Residual Sludges from Treatment of Wastewater
Jack E. Collier	Industrial Engineer	Collier Worm Ranch	Conversion of Municipal Wastewater Treatment Plant Residual Sludges Into Earthworm Castings for Use as Topsoil
Stephen C. Havlicek Robert S. Ingols	Organic Chemist Biologist	Georgia Institute of Technology	Effect of Infrared Radiation on Compaction of Municipal Wastewater Sludges
Charles Finance	Film Production and Direction	Media Four Productions	Synthesis of a Municipal Wastewater Sludge Management System

TABLE III
MANAGEMENT OF DIFFUSE (NON-POINT) SOURCES OF POLLUTANTS

Investigator	Discipline	Institution	Project Title
William H. Queen	Botany	University of Maryland	Management of Physical Alterations to the Edges of Chesapeake Bay and Their Effects on Environmental Quality
W. Lee Schroeder	Civil Engineer	Oregon State University	Assessment of Impact on Estuarine Ecosystems Resulting from Residuals Management by Dredging
David L. Correll	Physicist	Smithsonian Institution	Impact of Pollutants from Diffuse Sources on Quality of Water in Chesapeake Bay
Peter M. Cukor	Chemical Engineer and Business Administration	Teknekron, Inc.	Management of Threat to Regional Environmental Quality from Discarded, Used Lubricating Oil by Recovery for Reuse as a Lubricant
Richard G. Koegel	Mechanical and Agricultural Engineer	University of Wisconsin	Improvement and Evaluation of Techniques for the Mechanical Removal and Utilization of Excess Aquatic Vegetation
William L. Rathje	Anthropologist	University of Arizona	Socioeconomic Correlates of Household Food Residuals—Phase 2: Management Strategies

TABLE IV
INDUSTRIAL RESIDUALS MANAGEMENT

Investigator	Discipline	Institution	Project Title
Carlos R. Guerra	Engineering Science	Public Service Electric & Gas Co.	Utilization of Waste Heat From Power Plants in Aquaculture
Albert F. Eble	Biologist	Trenton State College	Subcontract with PSE & G
A. Farmanfarmaian	Physiologist	Rutgers University	Subcontract with PSE & G
Jack V. Matson	Civil Engineer	University of Houston	Feasibility of Eliminating Discharges of Pollutants from Cooling Towers
Josef J. Schmidt-Collerus	Organic Chemist	Denver Research Institute	Characterization of Contaminants in Oil Shale Residuals and the Potential for Their Management to Meet Environmental Quality Standards
George D. Ward	Civil Engineer	George D. Ward & Associates	Controlled Soil Microbial Detoxification of Herbicide Residues
Larry Boersma	Soil Science	Oregon State University	Utilization of Waste Heat in a System for Management of Animal Residuals to Recover and Recycle Nutrients
R.V. Subramanian	Chemical Engineer	Washington State University	Immobilization of Hazardous Residuals by Encapsulation
William R. Walker	Civil Engineer Law	Virginia Polytechnic Institute	Management of Liquid Residuals by Deep-Well Injection

FUNDAMENTAL RESEARCH NEEDS AS SEEN BY THE USER OF RESEARCH

Conference on Fundamental Research Needs
Water and Wastewater Treatment Systems

FUNDAMENTAL RESEARCH NEEDS AS SEEN BY A MUNICIPAL WATER DEPARTMENT

Carmen F. Guarino
Water Commissioner, Philadephia Water Department
Joseph V. Radziul
Chief, Research and Development, Philadelphia Water Department
Patrick R. Cairo
Assistant Chief, Research and Development, Philadelphia Water Department

INTRODUCTION

The continuing need for fundamental research in the areas of water treatment and wastewater pollution control has been recognized, redefined and reprioritized on a rather frequent basis. The focus of this action has generally been engendered by the enactment of federal laws, an emerging national awareness of the effect of pollution on the environment and, most recently, the concern for conservation of fixed, unrenewable natural resources. Unfortunately, fundamental research in the water field has seldom been cognizant of operation, maintenance and economic constraints faced by water and wastewater treatment facilities. As ones responsible for planning, management and operation of these two types of facilities, we find it essential that research investigators should also consider input at the level of implementation if they hope that their work will result in successful practical applications. Furthermore, any fundamental research must also include inputs from industry, consultants and the general public since these activities will necessarily require financial support and approval from the community at large.

The dwindling financial support for fundamental research is the second area which needs your serious consideration. Research is an investment which the water field can ill afford to neglect. Although billions of dollars are currently being spent on the design and construction of new wastewater facilities, a meager fraction of this sum is used to support the basic research which is necessary to develop new techniques or to help truly understand existing processes so that these may be operated at constant high efficiency. Increased financial support by the federal government is the only possible means of achieving the level of fundamental research required if this country is truly seeking to reach a safer, cleaner environment.

The panel members of this session represent all areas of influence and contribution to the user such as regulatory agencies, industries and consultants. Missing from this discussion would be members from the general public as well as those with specific interests such as environmentalists. This forum will seek to obtain comments from this sector by the questions and discussions which will follow the presentations.

GENERAL DISCUSSION

Management of a large municipal Water and Wastewater Treatment Department presents many difficult problems in planning, control and innovation. Since all these activities require financial support and approval of the community, time becomes a critical factor in all of our decision making.

For example, the upgrading of our wastewater facilities will encompass ten years of effort and approximately half a billion dollars of public funds. The last updating of our water treatment facilities spanned more than ten years, and current plans for automation and trace organics removal will take at least five more years.

Investments of these types required the application of the broadest knowledge available in the fundamental principles being utilized. And yet, a large part of our decision making process is based on a primitive understanding of the fundamentals involved. For instance, the development of detailed design criteria for the activated sludge process is still an empirical operation. This process has been utilized over seventy years, and not until the last twenty years has a scientific method been employed for the formulation of a basic understanding of the activated sludge process. The application of the fundamental equations developed to date are still subject to engineering interpretation and experience which may result in many different design variations producing the desired effluent quality. Laboratory and pilot plant evaluations are still required in specific areas to support engineering decision making.

Billions of dollars are being presently spent on the installation of this process throughout the country, while a complete understanding of process basics may be available in the distant future.

In other instances, although there may exist a basic understanding of the theory and empirical relationships within the unit process, very little may be known about the dynamics of each individual unit process or the interrelationship that the various processes will have upon each other.

Fundamental research should be directed towards deriving innovations and answers which could be useful to the design and plant engineers in developing process control strategies. These would enable the optimization of entire treatment plants in feed-forward or feed-back fashion to produce a high quality product with a minimum expenditure in personnel, energy and resources.

In large utilities recognition of the gap between outputs from fundamental research studies and usable process design criteria has led to the creation of in-house staffs which must translate fundamental research into applied technology. Although meeting the needs of a specific utility, this has often led to a duplication of effort among the various utilities due to lack of communication and cooperation which often results from independent organizations. It should be emphasized that this is not the case in certain countries such as Germany and Britain where both fundamental research and applied technology is sponsored and directed by strong, unified organizations.

WATER RESOURCES AND WATER TREATMENT NEEDS

Fundamental research in water resources, wastewater and water treatment is being discussed in detail by the various speakers today. To present these from a user perspective, we will briefly discuss certain problems that are facing water utilities trying to meet the requirements of federal regulations as well as safeguarding the interest and welfare of its consumers.

This country is rapidly making decisions on ocean outfalls and ocean dumping of wastewater sludges and other wastes without a fundamental understanding of the environmental effects. There is a need for basic understanding of the environmental impacts on marine life and aquaculture to determine if there are trade-offs in using our oceans for thermal and solid waste disposal. We need a fundamental research effort in the utilization of wastewater sludge for construction materials and the conversion of large amounts of this available wastewater treatment by-product into useful energy products such as methanol.

In the area of water pollution, probably the greatest offender is the stormwater runoff from street surfaces of a municipality. This has an important impact to the overall water quality picture not only from the basis of control of discharges but also to improve treatment plant efficiency.

In the treatment of water, there is need for a complete study of disinfection alternatives. These studies should include effectiveness and by-product analysis, as well as production and cost of disinfection.

Continuing leakage in water transmission lines may cost a city up to 30 percent of its water supply. Although this problem is centuries old, there is a need to develop a method for identification of these leaks through some new and possibly unique means. Water metering which is generally acknowledged as a proven method of insuring water conservation suffers from the use of imprecise and difficult-to-maintain equipment.

Water treatment plant by-product sludges represent a major solid waste problem. Better methods of dewatering and recycling are needed to minimize new chemical usage as well as simplify disposal problems. Study in this area is needed to avoid chemical additions and increasing energy consumption, and development of possible industrial uses of the sludge.

A large need exists in both water and wastewater treatment in improving the entire area of instrumentation. Measurement of the amount of biodegradable organic material in wastewater is indirectly determined by measuring the oxygen consumed over a five-day period by microorganisms in decomposing organic constituents of the waste. Techniques producing real-time results would permit establishment of process control algorithms and adjustments of wastewater treatment plant operations. This is an especially critical area since federal and state discharge standards include BOD measurement as a performance standard for treatment plants.

On the water treatment side we have federal bacteriological standards with no real-time capabilities for measurement. Whether one uses the multiple-tube fermentation procedure, the membrane filter technique or the standard plate count method, many hours are consumed in preparation, incubation and examination of samples. Furthermore, the results obtained do not provide definitive information concerning the bacterial safety of the water, and evaluation must be presently based on examination of a series of samples collected over a known and protracted period of time. Fundamental research is needed to develop more rapid analysis as well as identification of pathogenic bacteria and virus.

In the area of water resources we need instrumentation for rapid detection and identification of the presence of toxic substances, particularly trace quantities of organic chemicals. This instrumentation should be capable of rapid detection of toxic chemicals in river water as well as identification of trace organics in treated drinking water.

Water distribution systems have a multiplicity of elements. These include piping networks, valves and multiple pumping stations. The variability of load on a water system requires the development of control strategies, possibly computerized, that improve system operation maintaining pressures and flows with reduced energy expenditures in power and pumps. The solution would have to be anticipatory rather than reactionary and include daily, monthly and seasonal patterns of water usage. The solution would also include optimum efficiencies of pumps and pumping pressures in water systems. Where the water systems include more than one water treatment or supply source, the system developed would select the most economical and power saving distribution.

As you can see, we have been pushing technological implementation of our basic knowledges in this field to maximum fruition; however, this drive has exposed many voids in our "Know-How." In some areas we will need major breakthroughs in basic understandings before we will be able to take the next giant steps in the water and wastewater field. Our most important need is the initiation of fundamental research now!

ANTICIPATORY RESEARCH AND DEVELOPMENT

William J. Lacy, P.E.
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U.S. Environmental Protection Agency, Washington, D.C. 20460

INTRODUCTION

The purpose of the EPA's Anticipatory Research and Development program will be to:

1. Identify Potential Problems Before They Arise
2. Conduct Long-Term Investigations
3. Conduct Fundamental Research to Advance Basic Understanding of Environmental Problems Leading to Changes in EPA's Decision-Making Process.

In attempting to achieve these purposes EPA desires to improve its ties to the scientific community, to provide improved mechanisms for surfacing innovative research ideas and to improve the long-term scientific basis for its decision making process.

BACKGROUND

The National Academy of Sciences, the Office of Technology Assessment, and most recently, the Senate Committee on Environment and Public Works have all expressed concern over the lack of fundamental research in EPA. Other critics have charged that the Agency's regulatory responsiblities disrupt the atmosphere for fundamental environmental research and that the Agency has often failed to recognize adverse conditions until after widespread damage has occurred. To insulate these types of programs from regulatory pressure it has been recommended that a specific authorization be enacted which would authorize the Agency to perform anticipatory and fundamental research, that EPA consider the establishment of "Centers of Excellence" to conduct research on specific problems and that a "bottoms up" planning approach be utilized to implement these types of programs.

The National Academy of Science's "Research and Development in the Environmental Protection Agency," Chapter II, specifically identified the following activities as areas in which anticipatory research and development should be conducted:

— Design of Monitoring Systems
— Characterization of Pollutants and Discharges
— Assessment of Trends in Environmental Quality
— Determination of the Fate of Pollutants
— Determination of the Effects of Pollutants and Other Man-Caused Environmental Disturbances
— Investigation of Fundamental Physical, Chemical, and Biological Processes
— Analysis and Modeling of Ecosystems

RECENT ACTIVITIES

In order to respond to these concerns, the Office of Research and Development (ORD) undertook during the ZBB process a review of inhouse activities which could be classified as anticipatory or fundamental. The review identified approximately $4.7 million and 66 positions in our ecological, transport, process, monitoring and health (including all cancer research) programs. These activities are included within the Anticipatory Research Budgetary Decision Unit. Specifically, they include our:

— Environmental Carcinogen Research Program
— Integrated Exposure Monitoring Program
— Marine Microcosm Studies
— Basic Environmental Process Activities

In addition, during our ZBB activities we identified, in conjunction with our program office counterparts, a number of areas which could benefit from anticipatory and fundamental research.

A broad outline of these activities was presented to the Executive Committee of the Science Advisory Board for review and comment. As a result of these reviews, ORD proposed that, in addition to the previously mentioned activities, to further research in the following areas:

— Development of Bio-indicators
— Specimen Bank Feasibility Study
— River Basin Ecosystem Studies
— Acid Rain Studies

SOME CHARACTERISTICS OF ANTICIPATORY RESEARCH AND DEVELOPMENT

- Anticipatory R&D is distinct from research for regulatory purposes. However, along with more applied efforts, it must ultimately change the basic decision-making process in EPA.
- As an intermediate step it should be aimed at producing new techniques, mechanisms, assessments and analysis for use by more applied R&D programs.
- The production of data or more detailed data will not be a specific goal although some data will naturally result.
- It may be long term in nature, but the length of time is not an especially distinguishing characteristic; likewise the program may have global, national and local spatial proportions.
- It is not limited to biological, transport, health and monitoring activities, but may include technological and socio-economical sciences.
- It is not oriented toward a single media or group of pollutants.

Broad Approach

- The program will seek broad representation from the scientific community in the conduct of research activities and wide participation in proposal review and program evaluation activities.
- The program will strive to maintain a "bottoms-up" planning approach while providing broad guidance to the research community on the research themes of special interest to EPA.
- The decision-making process for new proposals will be straightforward, i.e., as administratively simple as possible. It may require a different decision process than existing mechanisms.
- The program will be a visible and distinct Agency effort. It will be widely advertised to the scientific community.
- The program will utilize in-house scientists to link external research projects to more applied in-house, grant and contract research.

SOME PROPOSED ACTIVITIES

Carcinogen Research

The basic purpose of this program is to determine whether or not the observed level of chemical agents in the ambient atmosphere and drinking water supplies contribute to the prevalence and incidence of human cancer. The principal objectives are: to establish a monitoring program to characterize chemical carcinogens in the drinking water environment; to develop retrospective and prospective external exposure models suitable for general population studies; and to assess the impact of exposure to chemical carcinogens via inhalation of ambient air and through drinking water upon cancer occurrence and prevalence in the general human population.

In FY-79, it is proposed to extend and expand the program begun in FY-78. Epidemiological studies will continue to use available mortality and morbidity information as it relates to specific localities and conditions and new studies will initiate long-term (lifetime) investigations to relate the total carcinogen exposure history, total carcinogen body burden and the incidence of cancer in human populations. Further, epidemiological studies will relate population health effects to data gathered during clinical and toxicological studies. Such relationships will, hopefully, permit better estimates of the carcinogenicity of substances from animal type toxicological studies.

Integrated Monitoring

One of the basic difficulties in pollution studies is the estimation of exposure. People receive varying levels of exposure to many potentially carcinogenic materials at home, at work, in the marketplace, in the streets or during recreation. These exposures are uncontrolled and most often undocumented. In the future, epidemiology studies would be greatly enhanced if these estimates could be improved.

Activities to be conducted in this area include:

- Research to develop methods for estimating lifetime exposure histories.
- Studies (models) to develop methods for estimating total body burdens of pollutants and carcinogens on single receptors and populations.
- Studies to improve exposure monitoring by optimizing air water quality networks through use of simulation models.

Development of Bio-indicators

The development and application of biological systems as early warning indicators of pollutant stress will be studied. Activities will be conducted by both the Narragansett and the Las Vegas Laboratories.

Narragansett activities will include:

- Development and refinement of integrating marine organisms, e.g., Mussel watch types of systems. In this project, mussels are collected in nearly 100 locations around the U.S. coast. These samples are analyzed for heavy metal transuranics, petroleum and chlorinated hydrocarbons. The first year's collection effort has been completed and the samples are being analyzed. It is hoped the use of these types of systems will permit early detection of pollutant problems.
- Development of understanding of pollutant-induced changes in microbial and biochemical systems. Emphasis will be on investigations of enzyme systems as environmental exposure monitors and early indications of pollution stress.

-- Las Vegas Laboratory activities include: investigations of terrestrial microbiological indicators for monitoring selected substances in the environment; e.g., the use of anaerobic bacteria which use hydrogen as an energy source to determine persistence of ubiquitous pollutants in soil.

Total River Ecosystem Studies

Multimedia studies of the impacts of selected pollutants on river basin(s) ecosystem will be initiated. Identified by the SAB as a serious gap within ORD's total program, these studies have as their goal the development of understanding of ecosystem structure and function when stressed by toxic organics and other pollutants. Impacts of industrial, municipal, agricultural and airborne sources will be evaluated. A draft study is under development.

Marine Ecosystems

Studies of marine - estuarine ecosystem responses to multiple stresses and complex effluents will be conducted mainly through the continued development, verification and use of marine microcosms. Emphasis will on the determination of effects, persistence, and bioaccumulation of pollutants. Ecosystem recovery rates will also be studied.

Environmental Processes

Studies of environmental processes will be directed at: 1) gaining the understanding of selected basic environmental processes (viz., photolysis, oxidation, microbial degration, volatilization, and sorption) necessary to describe them mathematically; 2) development of an evaluative model for predicting the transport and fate of inorganic chemicals and calibrating it for mercury and cadmium; 3) development of an evaluative model for predicting the transport and fate of organic chemicals including pesticides; and 4) development of simplified microcosms for use as rapid test methods for obtaining representative transport and fate data on potentially toxic compounds of interest.

Specimen Bank Feasibility Study

The feasibility of an environmental specimen bank will be examined on a pilot bank basis. The National Bureau of Standards proposes to adopt a portion of their facilities to achieve biological and environmental specimens. Analysis will be made of the quality of preservation, cost of operating and maintaining such a facility and alternative approaches to establishing such a facility.

Acid Rain

Acid rains have become recognized throughout the world as having the potential for significantly altering both aquatic and terrestrial ecosystems. A number of studies have documented the effects on lake systems, but no systematic assessment of the national scope and potential long term effects has been assembled. This initiative would provide for 1) the development of a broad assessment of the national problem; 2) studies of the chronic effects on soil characteristics and selected terrestrial ecosystems, particularly in the northeast quadrant of the U.S.; and 3) studies of the effects on lake ecosystems.

SUMMARY

Whether the research necessary to accomplish the Agency's goals is undertaken in-house or extramurally is entirely dependent on the availability of the scientific expertise, facilities and manpower in the Agency laboratories. If the research is undertaken outside of the Agency, it is normally through competitive grants and contracts. Proposals are reviewed in-house as well as by outside experts for feasibility, cost, and scientific capability of the researchers.

There is no official differentiation between basic and applied research in EPA. In fact, which research falls into which category is open to debate. Procedures for initiation, review, and termination are the same for all research. One proposed solution to EPA's problem of insufficient basic research is to separate basic research from applied research in early planning stages so that it may be better protected.

The Agency has increased its basic research since its inception in 1970. The need for basic research in EPA is certainly more generally recognized. Perhaps it is simply a question of time.

FUNDAMENTAL RESEARCH NEEDS IN WASTEWATER TREATMENT AS SEEN BY AN INDUSTRIAL USER OF TECHNOLOGY

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INTRODUCTION

Before describing an industrial user's view of fundamental research needs in wastewater treatment, it is appropriate to first define the nature of the intended use and the discusser's view of fundamental research. While Union Carbide may be more well known in environmental circles for products such as its pure oxygen activated sludge process or solid waste conversion system, in this discussion the "industrial user" is a representative of the organic chemical manufacturing business "using" the technology for compliance with wastewater discharge regulations. The "use" of technology ultimately resulting from fundamental research would be oriented to treatment for the compliance with NPDES permits or pretreatment discharge permits at thirteen organic chemical manufacturing plants. While an important example of the chemical industry, we are speaking only for Union Carbide.

Within Union Carbide and in this discussion, *fundamental research* is defined as

— A planned search for better understanding of a scientific phenomenon.

Fundamental research can lead to *process research*.

— The search for new knowledge and the application of existing knowledge to the definition of a potential process for a specific purpose.

We would differentiate between fundamental research and *exploratory research*, which we would define as

— The search for new knowledge in a selected area of a general field with no specific application in mind.

Both authors are familiar with wastewater research—both by managing or personally conducting research studies sponsored by Union Carbide or governmental agencies related to process development or fundamental process understanding for treatment of organic chemical manufacturing wastes. We are concerned with the need for additional fundamental research based on the experience that the findings of fundamental research frequently lead to more fruitful process research and other applied research efforts. The ultimate result is the introduction of new or improved treatment process technology which is more cost effective and less energy and resource intensive than available options.

Although it is understood that the primary purpose of this seminar is the discussion of water and wastewater treatment needs, it should be emphasized that it is virtually impossible to prioritize research needs without understanding the goals of required technology. The stated objectives of current federal water legislation are to maintain the chemical, physical, and biological integrity of the nation's waters through application of appropriate waste treatment technology. In addition, it is a stated policy that the discharge of toxic pollutants in toxic amounts be eliminated. In our view a major area for fundamental research in the water-wastewater field is in the area of both long and short term effects of pollutants on water uses to guide attainment of these objectives and policies. Such specific needs will be covered later.

In identifying fundamental research needs, we have cited major areas and phenomena which we have observed to be potential limiting factors to the effectiveness of treatment processes or to otherwise present gaps in needed knowledge. Specific areas of investigation would then be based on a planned search for better understanding of the described phenomena.

BIOLOGICAL TREATMENT

The phenomena listed below are considered to limit treatment of marginally degradable materials in biological treatment systems or to otherwise prevent these systems from achieving maximum potential.

— Competition (and suppression) in multi-substrate, multi-species situations.

— Populations shifts. . . applies to bacterial, bacterial-protozoan, and bacterial-algal systems.

—Unsteady-state nature of system due to factors such as variable feeds.

— Relative mass transport rates of substances, oxygen, and metabolites.

PHYSICAL-CHEMICAL TREATMENT

Phenomena listed below limit effectiveness of separation or oxidation in complex mixtures.

— Competition, displacement, mass transfer, and kinetics in adsorption of organics on carbon.

— Limited extent and rate of dilute-organic-solute oxidation by physico-chemical approaches. (Irradiation acceleration appears promising.)

— Soluble organic interferences with flocculation.

— Formation of interfacial deposits in membrane separation.

In additon, many physical and chemical treatments result in a separation-concentration of pollutants rather than destruction. The ultimate disposal of these residues should receive consideration if the zero discharge goal of current federal legislation is to be attained.

BIOLOGICAL SLUDGES AND OTHER RESIDUES

Main problems are difficulty in achieving release of water from sludges, putrescibility of the sludge, and at times content of metals, organics resistant to degradation, and pathogens. Phenomena involved include:

— Tenacity of water retention by biomass.

— Metal retention and release.

— Varying biological stabilization rates leading to odor generation despite pre-conditioning.

— Leachate release, degradation and transport in landfills.

The limited options for disposal of residues containing salts, organics, and solids from physico-chemical separation systems is a problem needing exploratory research.

ANALYTICAL

Major advances in analysis of volatile constituents recently have been made. Although many materials which may result in deleterious effects in receiving waters have been identified, a considerable fraction of residue from treatment operations remains to be studied. In addition, much experimental work remains to determine effects of sampling, concentration, storage, and analytical technique on recovery, precision, and accuracy for many materials currently identified. Important phenomena are

— Losses and transformations during sampling, concentration, storage, and analysis of trace contaminants.

— Mass transport of non-volatile organic constituents in analytical procedures; e.g., during concentration and matrix change prior to chromatographic separation.

EFFECTS OF POLLUTANTS ON RECEIVING WATERS

In order to establish priorities for pollutant-control technology research needs and capital expenditures for control, accurate measures of the effects of those pollutants remaining in the post-1977 era need to be determined. Phenomena of interest are:

— Interaction of complex organic mixtures at very low levels.

— Threshold effect level.

— Relationship of toxicity tests between various orders of life.

— Cellular transport/biodegradation/bioaccumulation.

SUMMARY

From the viewpoint of this industrial-user, priorities of research needs would be in order as follows:

1. *Effects of Pollutants on Receiving Waters.* Control needs, technology needs, and compliance strategies will continue to be unclear without further information on pollutant effects. Adequate information on pollutant effects cannot be obtained in an economical and timely manner without new information on the phenomena described above leading to new techniques to determine pollutant effects. This area should have a top priority.

2. *Analytical.* In some respects, the need for fundamental analytical work goes hand-in-hand with the Effects-of-Pollutants area. Many of the commonly recognized problem pollutants are contained in the more readily detectable category of "light" materials. Verification of available sampling and analytical techniques as well as development of new techniques for analysis of "light" materials is required to accurately measure pollutant dose and assess the progress of control activities. A second, lower priority analytical research activity would be in method development for analysis of "heavy" materials.

3. *Biological Treatment.* Biological treatment is the most widely utilized process in the world for applications involving soluble organic materials. As such, a fundamental understanding of the basic mechanisms controlling performance and removal of materials in this process must have a high priority. With the number of systems in existence, fundamental understanding could likely be moved to development and implementation in short order.

4. *Biological Sludges and Other Residues*. The problem of disposal of biological sludges has received considerable publicity. Fundamental work particulary in the area of achieving release of water from sludges is desperately needed to provide basic information for solving this important problem. Major additional studies in disposal of organic and brine concentrates may become important as the emphasis to achieve the zero discharge goal of the water law becomes more clear.

5. *Physical-Chemical Treatment*. Physical-chemical treatment would be judged to be most appropriate if guided by the work on effects of pollutants in the aquatic environment in order to prioritize treatments with appropriate pollutants to control in mind.

FUNDAMENTAL RESEARCH NEEDS AS SEEN BY AN INDUSTRIAL USER OF RESEARCH

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INTRODUCTION

A few introductory words are in order to identify the special perspective that the industrial user of research findings brings to this discussion of long range research needs in the water and wastewater treatment area. You have indicated a deep sense of disappointment at the declining visibility of a commitment toward, and support for, long range research by our national regulatory agency. That disappointment and concern are fully shared in the industrial community. We function today under a regulatory framework whose deficiencies can be traced in part to the resultant loss of a credible scientific basis for such regulatory programs.

It is therefore our desire that such a well-financed, fully visible, comprehensive, broadly planned and well-coordinated long range research program be restored to its position of central importance in the nation's water pollution control effort. Whether this can, or should be done within EPA is a separate, yet related, question whose further discussion now might detract us from the conference's stated goal.

A recognition of the conference sponsorship and personal view that the correct solutions to our water pollution control problems will materialize with the application, if you will, of "social engineering" skills leads me to approach the definition of long range needs in the following manner. A systematic approach to their solution should consider three consecutively related phases involving (a) a comprehensive examination of the state of water quality sufficient to identify the problem areas meriting solution, (b) the development of remedial technologies capable of sufficiently minimizing adverse impacts of effluents on water quality, and (c) the deployment of those technologies in a spatial and chronological manner that accomplishes the desired purpose consistent with meeting other social needs as well. Each of these three phases or areas presents long range research needs which are briefly set out below.

AN IDENTIFICATION OF LONG RANGE RESEARCH NEEDS FROM AN INDUSTRIAL PERSPECTIVE

There is little doubt that the comprehensive examination of the dynamics of receiving water quality continues to suffer from the absence of a long term commitment toward its scientific study. With a major national commitment to water pollution control at least twenty years old, with opportunities to either build deliberately onto a preexisting USGS water quality network, or to carefully construct and gradually expand such a specialized network, we still find ourselves discovering a "mercury problem" using the limited findings of a lone aquatic biologist examing fish specimens In Canada, or a "chlorinated hydrocarbon problem" through a public interest organization's limited analysis of cancer frequency statistics on the lower Mississippi River.

Surely a water pollution control program now annually commanding multi-billion dollar capital and operating expenditures in the public and private sectors deserves more than a dollar-short, piecemeal, chronically faltering water quality study program as its scientific justification and progress monitoring basis. To this end I would propose pursuing the following long range needs:

(1) Organization and maintenance of a long-term network of water quality monitoring stations at which flowing water, bottom sediments and a sufficient variety of aquatic life forms are regularly monitored for specific and class indicator chemical constituents and major water quality parameters.

(2) Continuing research on data integrating and analytical techniques to allow: (a) detection of downstream travel of unusual water quality, (b) determination of significance of deviations from desired water quality, (c) automated monitoring of specific parameters, and (d) input to control

models used to modify on-land wastewater management system performance.

(3) Research on the processes controlling the movement of pollutants through major elements of the receiving water environment such as sediment, flowing water and living systems to strengthen (a) our knowledge of the removal processes for both conservative and non-conservative constituents, (b) our appreciation of the role of bioconcentration processes, and (c) our confidence in the predictive capability of mathematical models of all these processes through an improvement in the physical/chemical/biological information base for their use.

(4) Determination of site-specific, practical tolerance limits for key constituents of major categories of industrial effluents that can be used to establish conditions of compatibility between various water uses and growing industrial activity along the nation's waterways.

With well-identified water quality concerns and considerations as a continuing point of departure, the development of improved and new control technologies can move beyond a search for novelty toward improvements and tools fitted to well-defined tasks. Long range research needs supportive of such progress include the following:

(1) In-depth examination of major control technologies to identify the impact of transient changes in process loading, load composition, and process conditions on operational results, so as to improve the performance-predictive quality and process designs. This need exists for both the biochemical and physicochemical processes and extends to a growing number of design-performance parameters.

(2) Development of means for minimizing performance variability through (a) greater on-line process control, (b) improved automatic detection, segregation and reprocessing of intermittent manufacturing process materials losses resulting from either spills or transient, cyclic process steps.

(3) Examination of new control technology approaches to determine whether they offer potential for (a) better or more reliable removal of conventional constituents, (b) lower cost performance, or (c) removal of additonal constituents which environmental study has shown to merit major attention.

(4) Development of optimal means for dewatering the hydrous residues of wastewater management through application of combinations of thermal, mechanical, electrical and chemical energy sources.

(5) Examination of the opportunities for recycle of treated effluent as well as in-process recycle of process wastes in optimal combination with external treatment.

While the last of these is included in this research needs analysis for purposes of completeness, it is viewed as an investigative area uniquely suited for industrial concentration and university participation by those with close experiential ties to industry and thereby sharing a knowledge of problems encountered in altering industrial process technologies. The possible need and justification for public financial support of large scale trials of such modified technologies is, however, recognized under certain high risk conditions.

The final research area to be considered encompasses the various socio-economic problems related to the pace and extent of control technology application. Each of these present continuing methodological problems whose solution commands participation and investigative leadership by the environmental engineering community. These can be identified as follows:

(1) Determination of economic impact on given industries and the national economy of broad-scale application of specific technological control levels.

(2) Development of cost-benefit analyses covering programs for attaining specific regional water quality control objectives through alternative load allocation procedures, low flow augmentation programs, and funding approaches.

(3) Examination of broad-scale effluent treatment versus selective advanced water treatment technology as alternatives for meeting water supply quality needs.

(4) Development of an effective program for regulating the discharge of a multiplicity of effluent chemical constituents in face of the growing sophistication, cost and sensitivity of analytical procedures for their detection in effluents and receiving waters.

SUMMARY

In summary, a series of long range research needs have been identified that fall into three broad categories involving (1) environmental problem identification, (2) development of control technologies, and (3) examination of the factors related to optimal deployment of those technologies. While these problem areas demand participation of investigative skills other than those of environmental engineers, the successful integration of such long range programs, as well as intelligent application of the research results, remain a primary responsibility of this community and should provide opportunities for constructive leadership at a time of apparent disorganization in the wastewater management field.

FUNDAMENTAL RESEARCH NEEDS AS SEEN BY THE CONSULTING ENGINEER

Roy F. Weston
Roy F. Weston, Inc.
Chester, Pennsylvania

It is my assignment to identify and prioritize fundamental research needs associated with water and wastewater treatment systems that are or that should be of particular concern to the consulting engineer. I am assuming that fundamental research pertains to research to establish basic characteristics and principles, as contrasted to applied research, which is concerned with the practical application of fundamental research, or to demonstration research, which is concerned with the practicability of the results of both applied and fundamental research.

Each consulting engineer is inclined to see his role differently, depending on how he perceives his role in society and how he implements that role.

I will identify the roles of the consulting engineer as those of: 1) the definer of the water or wastewater treatment problem; 2) the evaluator of alternative solutions to that problem; 3) the designer of the most feasible and cost-effective solution to the problem; and 4) the advisor on system and/or facility operations.

There are fundamental research needs relative to each of the consulting engineer's roles.

In his role as the definer of the problem, the consulting engineer may have to contend with serious questions of a technological, socio-political, and institutional nature. He may find that analytical procedures or instruments required for accurate measurement of the concentration of ions or substances suggested or required by the controlling regulatory agency are not available. He must determine whether the best practical or the best available wastewater treatment technology will, in fact, meet the environmental standards for natural waters or for gaseous emissions to the atmosphere or for sludge disposal onto the land.

He may find that the environmental and potential public health impacts associated with emission, discharge, or disposal of some ion or substance are not, at the time of the study, clearly defined. This is particularly so relative to the concentration of toxic and/or carcinogenic ions or materials and to the fate of these ions or materials in the environment. This lack of clear definition is also encountered in connection with the attenuation and transport of toxic ions and carcinogenic materials in the earth.

The conscientious and responsible consultant must be concerned and conservative about these matters. On the other hand, he must recognize that life is not without risk. Each and every individual is exposed to many hazards every day of his life. Most exposures to hazards are controlled or controllable and of low risk to life; others are insidious in that they slowly but surely cause discomfort, illness, lack of bodily function, and death; and some are acute in their effect on life. Since risk taking is a normal part of man's life, the consultant is obliged to identify and evaluate the risk associated with alternative plans of action as a part of defining the problem.

He is well aware that environmental control problems are no longer restricted to a single part of the environment; they pervade the entire environment. Therefore, the consultant must face up to the axiom: "The control of air pollution may create water or land pollution; the control of water pollution may create land or air pollution; and the control of land pollution may create air or water pollution, and so on *ad infinitum*."

Scientists and engineers have not, to this date, seriously addressed the full ramifications of the interrelationships among the efforts to protect the quality of the various elements of our environment (i.e., the air, the water, and the land). This is particularly so when energy, the mythological fourth element of our world, is considered.

The consultant encounters socio-political and institutional problems of both fundamental and practical significance. For instance, there are residues from all water and wastewater treatment processes that must eventually end up on the land. In most cases, the socio-political climate and the institutional realities make it impossible to acquire disposal sites for future use, and, in some cases, make it extremely difficult to acquire sites even for current needs.

Although this is not an example of engineering as such, it is so dramatic that the engineering profession must learn how to educate the public so that sound projects will be consistently funded and constructed on a cost-effective timely basis.

Thus, the consultant may encounter serious obstacles relative to positively defining his client's problem due to a need for research in qualitative and quantitative chemical analysis, potential environmental and public health impact, the technological, socio-political and economic interrelationships among the various environmental impacts, efficient energy use, risk analysis, and socio-political and institutional constraints.

Fundamental research is seriously needed in each of these areas.

As the consultant evaluates alternatives, he must consider technological feasibility and financial cost. He may be confronted with the psychological problem of evaluating the seriousness of the regulatory agencies' requirements. If he assumes that he must, in fact, meet their requirements precisely as stipulated, he is faced with the need for reliability of performance evaluation of each alternative. This may present a problem because adequate records on which to base reliability of performance may not be available.

In some cases, the consultant will find that there is no established and proven technology available for solving his client's problem. To complicate the situation, when the problem requires an innovative solution, the consultant may well find that control agency rules and regulations will not permit other than standardized approaches.

In other cases, consultants may find themselves responsible for inadequately performing equipment or instrumentation because EPA decided that such equipment or instrumentation must be included as equivalent to superior products of proven performance.

Current trends relative to the liability of consultants for facility quality and performance increase the need for more information relative to the fundamentals involved in the control of the performance of various process units and the impact of variable operating conditions on such performance. Such fundamental information is also pertinent to reliability of performance predictions.

Consultants recognize that safe drinking water supply and fishable/swimmable natural water are important elements, but not the only important elements, of a high living standard and a high quality of life. As long as there are ill-fed, ill-clothed, and ill-housed people in the United States, there will be competition for a limited supply of dollars. Therefore, safe drinking water supply and fishable/swimmable natural waters must be provided at the lowest possible cost.

I believe that this country cannot continue to reduce and control water pollution on an arbitrary basis without consideration of cost. Fundamental research relative to establishing national costs for protecting the quality of our waters, using different control philosophies and concepts, should be undertaken.

I believe that it is imperative to let the public know the cost of attaining and maintaining incremental differences in water quality. The importance of fundamental and accurate models for such predictions is emphasized by the need for more definitive information on the impact of non-point source pollution and the interrelationships between point source and non-point source pollution on overall environmental and water quality.

Current federal law requires preprogrammed, robot-like, unprofessional performance from the environmental quality control profession. Technological rather than legal solutions to environmental problems are badly needed.

To assure adequate evalution of alternatives, the consultant needs fundamental research so that he can be more accurate in predicting the reliability of performance of the different unit processes and combinations thereof under various operating conditions; he needs economic research to establish the relative costs to the whole community of different pollution control strategies; and he needs systems research to develop methodologies and models for determining the relationships between point and non-point pollution control that will achieve the most cost-effective environmental results.

In addition, research to establish the technical and economic feasibility of alternative concepts for solving problems should be continued. Examples are dual water supply for reducing the costs of providing an acceptable drinking water supply, and special actions or facilities to protect endangered species or unusual spawning areas.

Also, research is needed to confirm the validity of existing state facilities standards. The consultant must endeavor to design systems and facilities that will achieve the desired level and reliability of performance at the least "life cycle" cost. The escalating costs of energy and operations and maintenance, wages and salaries will significantly influence municipal treatment plant designs.

While fundamental research may not be involved, data on energy utilization and on operation and maintenance should be collected under the auspices of EPA for various process units and control systems. Also, EPA should undertake studies to determine the economics of different types of materials of construction for sewage treatment facilities.

Studies should be made to evaluate the influence, if any, of the federal grant philosophy and process on the "total cost" of municipal water supply and wastewater treatment. "Total cost" should be evaluated because it is minimum "total cost" that will minimize taxes.

As the consultant participates in solving operations problems, the need for fundamental knowledge of process and equipment behavioral characteristics is apparent. While we must recognize that a few operators can achieve operating results from "gut feel," or intuitive reaction, to situations that others cannot achieve with the most sophisticated monitoring and control facilities, we must also recognize that knowledge of fundamentals helps both the intuitive and the pragmatic. Thus, fundamental research relative to unit-operations behavioral characteristics is essential to optimum utilization of existing equipment and processes.

PRIORITIES

It is difficult to establish priorities when there are so many and such diversified research needs to optimize the solutions to our problems. I would establish research priorities on the following basis:

1. Identify and characterize those drinking and natural water quality problems that may, directly or indirectly, adversely affect the public health.
2. Identify and characterize those pollution problems that adversely affect the balance of nature and essential natural processes.
3. Identify those pollutants for which adequate control methods are not available, and then develop the needed control methodology.
4. Further develop fundamental information essential to optimizing the performance of existing investments in water and wastewater treatment and disposal systems.
5. Establish the technologies, the tradeoffs, and the means by which the technological, socio-political, institutional problems associated with water and wastewater treatment residue disposal may be resolved.
6. Develop the parameters, criteria, and methodologies needed:
   a. to evaluate the costs and benefits from achieving alternative national water quality goals; and,
   b. to establish the most technologically and economically feasible strategy for achieving the selected national water quality goal.

Although the development of parameters, criteria, and methodologies appears in the foregoing priority list as No. 6, there are many reasons why it should be No. 1.

DISCUSSION

Marty Wanielista (Florida Technological University):

A proposal was advanced to establish an extensive water quality monitoring system in our national waters. This should be questioned in view of (1) the need for extensive data to separate point from non-point sources which now requires much field data if no models are available, and (2) the more cost effective applications of models based on in-depth basic research studies of a minimum of surface ground waters.

Water J. Maier (University of Minnesota):

Water utilities have provided the community with a high quality product at a very low cost. Unfortunately, it appears that the water industry has neglected support and sponsorship of research that can help solve the problems posed by the presence of the host of new chemicals in the water environment. There has been too much reliance on EPA or other government agencies for research funding that properly belongs in the domain of the water utilities. In many cases, the problems are local and relate to the geographical-geological environment and should be studied at the local level without involving Washington, D.C., as a source of financial support and, hence, control of research.

Roy Hartenstein (SUNY-Syracuse):

The decomposition processes that occur in sludges following their discharge upon land are extensions of the biological processes which were begun in the wastewater treatment plant. Aerobically digested sludges are generally compatible with most biological processes that occur in soil. Such sludges are usually consumed rapidly by earthworms and other soil invertebrates. The egested materials from these animals have (1) a surface area which is enormously greater than the sludge had prior to ingestion, and (2) a greater density of micro-organisms. In consequence of both of these changes the sludge decomposes and stabilizes at an enhanced rate, and the material can be dried more rapidly and transported more economically for land application purposes.

Anaerobically digested sludges, in contrast, are acutely toxic to earthworms when they are initially obtained from a digester. Toxicity diminishes with aging and exposure to air as indexed by an Eh in excess of +250 mv. Anaerobic digests are nevertheless inferior to aerobic digests with regard to earthworm nutrition.

Since large metropolitan areas produce mainly anaerobic sludges which are increasingly being disposed of by land application, it is critical to understand the potential of using earthworms in sludge management. In soils earthworms predominate over other biological agents with respect to mixing and aerating the soil components. In conjunction with other members of the soil fauna (including nematodes, enchytraeids, and protozoa) and microflora (bacteria and fungi) they may be used as important tools in environmental management. To maximize this potential, more research is needed on the biological attributes of these organisms and on the technology necessary for using earthworms in large scale operations. These operations include the conversion of various organic wastes, such as pulp sludge, biodegradable solid wastes, and sewage sludge into marketable products which help offset the costs of waste treatment.

David W. Hendricks (Colorado State University) for Mr. William Lacy:

EPA has grown and expanded to the point that there are a multitude of disciplines employed. With this there is no professional identity. The sanitary engineering profession has lost its leadership within the agency in developing the vision of what ought to be done in research and in preserving a good ethical climate for proposal handling. Instead, EPA research programs follow fads and novelties and lack continuity. Current practices are in sharp contrast with the old USPHS traditions of high ethical standards in handling proposals and which supported sound well-conceived research. A vast amount of monies have been poured into EPA research activities through in-house projects and competitive bid contracts, with very little output of worthwhile results. There really needs to be a budget reallocation toward support of fundamental research outside of EPA, using the unsolicited proposal mechanism provided by NSF.

Lacy's Reply:

The Environmental Protection Agency's Office of Research and Development has grown a total of 10% in seven years, about 1.3% per year, not much in anyone's book. It is my opinion our scientific staff feels a high professional identity and would resent implications to the contrary. I doubt that sanitary engineers ever had the leadership within this or any agency or should have had that role. In a National Science Foundation survey on occupation distribution of full time, government workers EPA was found to have 26% scientists, 21% engineers and architects and 2% lawyers; all other agencies average 15% scientists, 10.4% engineers and architects and 3.4% lawyers.

The degree levels for these R&D scientists and engineers were found to be in EPA, Ph.D. 27.6%, Masters 38.3% and

B.S. 32.1% and for all other federal agencies Ph.D. 19.3%, Masters 26.4% and B.S. 48.2%.

The current handling of all proposals has placed this activity on the highest professional ethical plane and by competition for the limited funds virtually eliminated the "old boys club" atmosphere.

The NSF still uses the unsolicited proposals mechanism and should for their type program.

a. In planning and executing research, the ORD strategy does have a balance between research that responds to the minimum regulatory requirements viewpoint, and quality research that is responsive to the full scope of environmental control problems.
b. The longer range anticipatory and fundamental research programs are designed (i) to guide rather than respond to environmental control strategies and (ii) to develop concepts which may provide the basis for future pollution control technology and regulations.
c. Planning includes input from a broad base of scientists and engineers both from inside and outside the Agency
d. EPA has in-house researchers who are experienced in fundamental research and they help plan a balanced program that includes such anticipatory research.

Peter O. Nelson (Oregon State University) for Mr. William Lacy:

1. Are unsolicited proposals considered seriously? 2. How are unsolicited proposals handled which don't directly fit into EPA-identified programs? 3. If a proposal gets poor internal reviews by EPA program directors, is it sent out for outside review too? Comment: It was unclear to me whether EPA will or desires to fund fundamental research from unsolcitied proposals that may or may not fit into their identified programs. Are the so-called "centers of research" to be established by EPA going to handle the only fundamental research effort in the future?

Lacy's Reply:

1. All unsolicited proposals that are received by EPA's Office of Research and Development are considered seriously. Each is analyzed and evaluated with regard to program interest including need and technical merits i.e. adequacy of design, competency of proposed staff, suitability of purpose, available resources, appropriateness of the project period, probability the intended results will be accomplished and uniqueness or originality and last but not least availability of funds.
2. Unsolicited proposals which do not fit into EPA identified programs or outside the mission or responsibility of EPA are usually returned to the sender with specific suggestions on where support might be obtained.
3. If a proposal gets poor internal review it is returned to sender with these comments and their specific suggestions. We do not send this type proposal outside for additional reviews.

It has been said that the Agency's regulatory responsibilities disrupt the atmosphere for fundamental environmental research and that the Agency has often failed to recognize imminent adverse conditions until after widespread damage has occurred. To insulate these programs from regulatory pressure, it has been recommended that the Agency be authorized to perform anticipatory and fundamental research, that EPA consider the establishment of "Centers of Excellence" to conduct research on specific problems, and that a "bottoms-up" planning approach be utilized to implement these programs.

As a result of these and other recommendations, specific language is embodied within the recently enacted (Dec. 77) Public Law 95-155, the Environmental Research, Development, and Demonstration Authorization Act of 1978, to require the long-range plan to address some of these concerns. This year's (FY 78) R&D program plan presents information specifically in response to Sections 4, 6, and 10 of the Act.

These presentations include:

- Several resource projections to assist Congress in evaluating the progress and level of effort of the research program (Sections 4)
- A presentation of a continuing and long-term environmental research and development program (Section 6).
- The status of the implementation of the recommendations prepared for the House Committee on Science and Technology in "The Environmental Protection Agency's Research Programs with Primary Emphasis on the Community Health and Environmental Surveillance System (CHESS): An Investigative Report" (Section 10).

Also in compliance with Public Law 95-155, this year's R&D plans been reviewed by EPA's Science Advisory Board.

Stanley Klemetson (Colorado State University) for Mr. William Lacy:

I would like Mr. Lacy to discuss Mr. Gellman's comment on the recycling of treated effluents. We put considerable emphasis on treatment of wastewaters but not to the level of potable use. What is the future of research on water treatment and more particularly water reuse for potable use?

Lacy's Reply:

In EPA's Industrial Pollution-Control Research and Development Program we emphasized closed loop water systems but not for potable use. I think Mr. Gellman was concerned with the industrial recycling of water and not potable reuse.

In a growing economy, the attainment of improved ambient environmental quality by the application of a fixed set of emission and effluent controls is not an ultimate solution. As the economy expands, the amount of pollution discharged will grow due to increased utilization of existing production capacity and the addition of new sources. If best available technology advances toward "zero pollutant discharge," a containment of all regulated pollutants could be achieved. However, with zero pollutant discharge, most of the pollutants are removed from the airborne or waterborne phase as solid or liquid residuals and must be either recycled or dealt with through some form of comprehensive land disposal program. If these wastes are not prudently managed, hazardous and toxic pollutants will cause secondary air and water pollution problems.

EPA's regulatory mandate has resulted in the promulgations of a number of standards to reduce the adverse health and environmental impacts of industrial pollution. In cooperation with other government agencies, research institutes and universities, the Agency is currently conducting studies in the following areas:

a. Identification, characterizations and measurement of pollutants released into the environment;

b. Development of control technologies to eliminate or reduce pollutant releases;

c. Study of the transport and fate of pollutants after release to the environment;

d. Effect of pollutants on human health and the environment;

e. Socioeconomic effect of new environmental regulations and policies.

Joseph V. DePinto (Clarkson College) for Mr. William Lacy

It was apparent from this morning's talk that more long-term, farsighted research was needed in the water and wastewater area. The EPA, however, typically funds research on a three-year project period basis. To me this time period only serves to ask more questions than it answers. Will the anticipatory R&D program of EPA consider funding truly fundamental research projects, which have designated project periods longer than three years? For example, a given environmental engineering group may have a particular expertise or may be located in an ideal section of the country for making a significant contribution to the fundamental knowledge of a particular topic. In that case a six or eight year cohesive study may prove to be more efficient and rewarding for both the agency and the researchers.

Lacy's Reply:

EPA has a new $13 plus million anticipatory research program. It is proposed to:

a. Identify and characterize emerging environmental problems before serious crisis;

b. Provide stable support to investigate long-term problems;

c. Serve as a mechanism to assure that basic studies needed to applied research are conducted.

The key features will include:

a. Proposal reviewed by panels of EPA/outside scientists

b. Areas of interest advertised each year.

c. Panels will provide guidance and recommendations, not final decisions.

d. Laboratory scientists would monitor most projects, occasionally reviews of special projects may be conducted by panels

e. Individual ORD scientists/(labs) would be encouraged to submit proposals and compete for resources.

f. Internal positions would be provided from a rotating pool or term appointment.

The center support program would be located primarily in universities but national laboratories and other existing institutions could be used. The funding provided to a research team would be from 3-5 years or longer if justified. It is hoped that cadre of expertise for special analysis would be developed along with more efficient training of scientist and engineers in special research areas.

Dennis Clifford (University of Houston) for Mr. Roy F. Weston:

Mr. Weston made reference to the fact that often consultants are forced into accepting legal solutions to technological problems and are put upon to assess the commitment of the regulatory agency to the enforcement of unrealistic and sometimes impossible-to-meet effluent standards. My experience causes me to agree with these observations. But because we are interested in specific research needs would Mr. Weston please give an important example or two citing these unrealistic standards and solutions.

Weston's Reply:

Mr. Clifford requests specific examples of unrealistic standards and solutions demanded by regulatory agencies. While there are numerous illustrations to refer to, specifics may cloud the basic issue; that is, the need for research to establish the methodology and criteria by which we can practically evaluate whether or not benefits derived from any specific regulation are commensurate with the total costs incurred.

David G. Stephen (US EPA) for Messrs. Hovious, Gellman and Weston:

EPA has reduced its support for fundamental research in the waste treatment and water transport areas over the last 6-7 years. Have other sources of sponsorship for such work increased their support to 'fill the gap' and will they do so in the future?

Hovious' and Conway's Reply:

No apparârent new sources have developed for support of fundamental research. Hopefully, NSF can aid in filling this gap.

Weston's Reply:

Those responsible for pollution have expended significant funds for research to reduce pollution and to accomplsih such reductions economically. Such organizations will, without a doubt, continue to expend funds for such purposes. Unfortunately, the law and past and present experiences with regulatory agencies do not provide optimal motivation for such research. The concepts of Best Practicable and Best Available Technology and the required use of such technology whether it is needed or not motivates against spending research dollars for improving technology. This is so since newly developed technology may be used against the researcher to increase his cost. I believe research will be a sub-optimal level until the incentives for research are changed.

Mr. Roy O. Ball (University of Tennessee) for Mr. Joseph Hovious:

Research has been completed and/or is underway on many of the areas you describe. Is your major concern (1) insufficient research effort? (2) more fundamental effort? (3) coordination and documentation of research? (4) all or none of the above. As a former industrial pollution control engineer, I believe that (3) may be the greatest problem, a point of view supported by Dr. Rohlich's historical remarks.

Hovious' and Conway's Reply:

Dr. Rohlich's remarks are well taken that we should not proceed without a full knowledge of past findings. However, we feel that much can be gained by a more fundamental examination of the areas we suggested. A multitude of applied studies have been addressed to problems associated with these phenomena, but much less work to the basic phenomena themselves.

CONGRESSIONAL PERSPECTIVE ON RESEARCH NEEDS

Conference on Fundamental Research Needs Water and Wastewater Treatment Systems

A CONGRESSIONAL PERSPECTIVE ON RESEARCH NEEDS FOR WATER AND WASTEWATER TREATMENT SYSTEMS

G.E. Brown
Chairman, Subcommittee on the Environment and the Atmosphere of the Committee on Science and Technology
U.S. Congressman (California, 36th District)

I am pleased to be here today to contribute my views on "Fundamental Research Needs for Water and Wastewater Treatment Systems." As your luncheon speaker, I am expected to distract you from your working sessions without distracting your digestion. I hope I can do this, while still contributing to our mutual consideration of the issues before us.

As Chairman of the Subcommittee on the Environment and the Atmosphere of the Committee on Science and Technology, I have had an opportunity to review the general issue of basic, or fundamental, long-range research versus short-term applied research in a generic sense. I've also had a chance to examine this issue particularly with regards to the EPA's Municipal Wastewater Research and Development Program. At the end of September, 1976 my subcommittee held hearings on this subject, at which the general state of EPA's Municipal Wastewater Research Program was thoroughly examined. The information gathered then was much the same as today—*the fundamental high risk research on new technological processes was nearly non-existent, and the multitude of issues identified by organizations such as this group, as well as EPA researchers themselves, went unfunded and largely unexamined.* In fact, even some of the same specific and present research needs and concepts were identified at the same time.

My own recent review was pre-dated by President Roosevelt who, in a March 10, 1938, speech to Congress, noted the need for the conservation and development of our water resources. Almost forty years ago he stressed the importance and interrelationship of water quality, water supply, and land management. Implicit in his address was the call for innovation in this field and the cry has yet to be adequately answered.

A quick look at the R&D budget of EPA and its predecessor shows that while there has been almost a quantum leap in the construction grants program—up to over $4 billion today, from about $200 million in 1968 and 1969, the supporting water R&D program has actually decreased from about $25 million in 1968 and 1969 to less than $12 million. What is the cause for this state of affairs? Why haven't some of the billions of dollars going into the construction grants program been channeled into the research programs you are all familiar with? Why has the Administration and the Congress allowed this near phaseout of fundamental research to occur? It is not reassuring to tell you that I, as one of the Members most concerned with the EPA budget in the Congress, do not know the answers to these questions. I do know that many research issues have been identified and that both the EPA Office of Research and Development and my Subcommittee on Environment and the Atmosphere share the goal of addressing these issues with adequate resources. I also know that somewhere between the EPA's Office of Research and the Congress, the "official" budget contains none of these research initiatives, and only the base, applied program continues. I also know that notwithstanding the recommendations of both the House Committee on Science and Technology and the House Committee on Public Works, the House Appropriations Committee does not appropriate the same level of funds which we authorize. Research funds, especially for what is seen primarily as an operational public works, jobs program in the minds of most Members of Congress, are considered as either totally unnecessary or as too low a priority to include in the final Presidential or Congressional budgets. It is my hope that this past history will not continue next year!

As you probably all know, the House and Senate Conferees have agreed to the Conference Report on H.R. 3199, the 1977 amendments to the Federal Water Pollution Control Act. In these amendments there is a basic agreement on the number of significant policy questions which reflect the Congressional judgment that adjustments to the Water Pollution Control Act are necessary. Some of these amendments are intended to make it easier for new or innovative technologies to be implemented. Many of these revisions either imply, or require, new research and development programs. All of these amendments give the Administration the basis for re-examining its research and development budget, and supplementing it where clear-cut gaps exist in basic knowledge.

I would recommend to those attending here today that you take this time of mid-term readjustment in the Federal Water Pollution Control Program to focus on high priority research needs, and present those needs to as wide an audience as you can. Those of us who have attempted, and failed, to improve the EPA's research budget in the past need assistance in gathering support and increasing the awareness of the Administration and the entire Congress to the research gaps and potential from a broader research base.

There is one more reason why this is a good opportunity to renew efforts to improve support for fundamental water research. The country has again discovered a "water shortage." Together with the "energy shortage," and the general financial constraints hitting all governments, this is creating a climate where more energy efficient, cost-effective and water conserving means of controlling wastes are being sought. In addition, the President's Water Resource Policy Study is now well underway, and promises to make even further changes to our laws which apply to water pollution.

The debate that this study will generate will give Congress another opportunity to infuse the proper amount of funding and innovation in our R&D efforts. The climate for such a change appears right and we should be optimistic. Optimism must be translated into action, however, and researchers like yourselves and groups such as this can help Congress meet its responsiblity. You have been articulating the problems for some time; you are capable of providing technological answers and you are equally capable of providing sound organizational responses to our mutual dilemma. You must carry these messages beyond these walls to federal, state and local officials, to private interest groups, whether friendly or adversary. In short, you must carry them to the public.

The implementation of innovative basic research faces roadblocks other than funding shortages and ignorance on the part of officials. Research scientists, particularly those engaged in basic research, will have to be sensitive and responsive to economic and social considerations. While it is not practical for scientists to heavily engage in training in sociology or economics, the path from basic research to wide-scale adoption of technologies—and even concepts—will be far shorter and less hazardous than it is presently proving to be when we are constantly aware that basic research is only one of the vital parts to a larger effort to solve a social, economic and technologic problem.

I believe that a good case can be made for greatly increasing our funding of fundamental water research and development. The related issues of land use, environmental quality, human health, energy conservation, and economics all add support to the need for a comprehensive and well-planned water research problem. I certainly want to see what specific research needs this conference identifies for water and wastewater treatment systems. However, this effort will only be a success if we can transform our analysis into action, and convince others less attached to the research program that we would all benefit from its results. I hope we can work together to this end.

FUNDAMENTAL RESEARCH NEEDS IN WATER TREATMENT

Conference on Fundamental Research Needs Water and Wastewater Treatment Systems

AN INTRODUCTION TO FUNDAMENTAL RESEARCH NEEDS IN WATER TREATMENT

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INTRODUCTION

What are the fundamental research needs in water treatment? Wherever we find man, we necessarily find the wastes produced by man's activities. Such wastes inevitably find their way into the air, the soils or the waters near their points of origin. Since matter can neither be created nor destroyed by man, man's activities serve merely to change the form of matter and the place and concentration in which it is to be found in our environment. Every time it rains, the wastes produced by man are washed from the air and, depending on the rate of precipitation and the solubility of the wastes, are carried to and into the soil and underground water supplies or are carried by surface runoff into our surface waters. It is, therefore, safe to conclude that all products and wastes produced by man are present at some time in the water sources from which we derive our drinking water—our public water supplies.

In other words, every solid, liquid or gas that is produced is likely to come into contact with water which eventually serves as our source of drinking water. Such contact will mean that the water will contain constituents which affect the quality of water. In order to assess their effects on the use of the water as a source of drinking water, all such constituents may be classed in one of four general classifications depending on whether their presence in water would be:

- Impermissible
- Undesirable or objectionable
- Permissible but not necessarily desirable
- Desirable.

The placing of a particular constituent of water into one of these classifications will depend on the concentration of the constituent and its probable effects (health, esthetic, synergistic, economic, etc.) on the water's use:

- Poisons or carcinogenic materials at levels harmful to the health of man are clearly impermissible constituents of drinking water.
- High oxygen and low water pH levels are undesirable or objectionable because they contribute significantly to corrosion of water distribution systems.
- Calcium and magnesium, which contribute to water hardness, are cations which are permissible in water but not necessarily desirable since they contribute to increased costs of using the water.
- Fluoride at a concentration of 1 mg/l is desirable in water since it contributes to reduction in the DMF (Decayed, Missing and Filled) incidence of tooth decay, but at 10 mg/l it must be reclassified into the impermissible classification since it then contributes to mottling of teeth.

How many other constituents in water may be desirable at one concentration, permissible but not necessarily desirable at a different concentration, and objectionable or even impermissible at still different concentrations?

FACTS TO CONSIDER

Before we look at the fundamental research needs in water treatment, consider the fact that we are of a NATION OF CRISES. We react slowly to growing challenges, but when we become suffiently concerned, we do react.

- We establish new goals.
- We establish new institutions and the authority we need to reach those goals.
- We establish teams of people—ecologist, biologists, chemists, attorneys, economists, engineers—needed to define the problem and to effect the solution.

In the early 1950's we determined that we needed a 100 billion dollar system of Interstate highways to bind our country together. All of you have benefited by the fruits of that goal. In 1965, we determined that by 1983 we would eliminate pollution of our water environment. We are on the way, even though we have extended the date when we expect to reach the goal of "no pollution" of our water resources.

On December 16, 1974, the Safe Drinking Water Act was enacted giving the Administrator of the Environmental Protection Agency the power to control the quality of drinking water in public water systems through regulation and other means. The Act provided for the establishment of:

- Primary drinking water standards that are health-related, and
- Secondary drinking water standards that are related to esthetic qualities of water.

Primary and secondary drinking water standards have been established. In carrying out the design, construction, operation and maintenance of the water treatment facilities required to meet these standards, however, several facts need to be evaluated:

- At present, we do not have an adequate supply of trained scientists and engineers with background in design, operation and maintenance of the new treatment processes which will be required to meet the new standards. The new standards, for example, will undoubtedly require that cities such as New York, Bridgeport, Connecticut, Seattle, Washington and Los Angeles, will for the first time, be required to build treatment plants if they are to fully meet the new quality standards.
- During the past several years, new water constituents—asbestos fibers and the trihalomethanes (5)—have been discovered which resulted in the establishment of new standards requiring advanced treatment technology not now standard in the water supply industry. It is not only possible but probable that additional water constituents will be discovered that will require development of advanced treatment technology to effect their control. Continuous research must be conducted to evaluate such potential.
- At present, we have not developed effective, economical treatment technology which will be required to meet our water quality needs based on today's water quality standards.
- Research today is specifically pointed at the solution of today's problems rather than the development of fundamental knowledge in a given area related to water treatment needs.

APPROACH TO CONSTITUENT CLASSIFICATION AND WATER QUALITY MODIFICATION

To be able to classify the constituents in water as to their probable water quality effects, it is essential first that we be able:

- to develop analytical procedures necessary for the qualitative and quantitative identification of all constituents present in public water supplies, and
- to evaluate the potential and the probable health and economic effects resulting from the presence of the measured constituents in the water.

Probably the most important and most uncertain scientific and political question that needs clarification today is associated with risk and its relationship to hazard assessment. Since no one has yet found the "Fountain of Youth," all of us here today must eventually die. Therefore, our risk of dying some day is 1 on 1. In fact, one in four of us will die of cancer. If you smoke, your risk increases to 1 in 3. Most of us arrived here today by automobile, yet our risk of being killed in an automobile this year are 1 in 4,000 or 1 in 50 in your lifetime. Today we are legislating *mandatory* air bags in our cars at a cost of $300-$400 each to reduce that risk to 1 in 5,000 in 1983, yet *at the same time* we subsidize the growth of tobacco, the use of which *increases* the risk of dying of cancer from 1 in 4 to 1 in 3. When such risks are accepted voluntarily, what levels of risk should our water consumers be protected from involuntary exposure to? The classification of water constituents must be based on logical and justifiable criteria and levels of risk. At the present time, there is reasonable scientific disagreement as to what methods should be used in assessing health effects of water quality constituents and in establishing reasonable levels of risk (and/or safety factors) required for establishment of drinking water standards (1,2,3,4).

Once the constituents present in water are properly classified, then steps can be taken to meet the requirements established by the water quality standards established. This may require:

- location and development of a new source of water supply, or
- elimination of the undesirable water quality constituent at its source—either by prohibition of its manufacture or by treatment prior to its discharge to our water sources, or
- removal of the constituent by treatment of the water in a water treatment plant.

In 1965, the U.S. embarked on a 100 billion dollar program directed at the elimination of the discharge of all pollutants from the nation's water resources. In support of that program, a graduate training program was supported by the U.S. Environmental Protection Agency (and its predecessor organizations) to increase the number of scientists and engineers trained in principles of water pollution control. At its peak of support in 1972-73, over 2000 new masters-level candidates entered the field each year. This year, with phasing out of federal support, less than 400 new masters-level students entered academic training in this field. At the very time that new personnel needs were increased by passage of the Safe Drinking Water Act of 1974 (Public Law 93-523), federal support for training of professionals in the field was curtailed and terminated.

Although many of the principles involved in treatment of wastewaters are the same as those used in the modification of the quality of drinking water, the application of the principles differ in water and wastewater treatment systems. The recognition that wastewater treatment systems cannot be 100 percent effective should signify the importance of water treatment systems in the water quality modification scheme.

In general, the major problems in water treatment involve the following:

- Evaluation of the public health and/or esthetic and economic significance of water quality constituents.
- Evaluation of techniques and procedures to be used for the quantitative measurement and identification of water quality constituents that may be of health, esthetic, or economic significance.
- Development of effective, economic methods for removing or reducing the concentration or modifying the form of water quality constituents of significance.

The major water quality problems that deserve long-term fundamental research attention include:

— the identification, characterization and development of methods for removal of *all* suspended particulates from water.
— the identification, characterization and development of methods for the removal of *all* soluble organics from water.
— the identification and characterization of factors affecting the deterioration of water quality in water distribution systems and the development of systems for control of the stability of water.
— the identification, characterization, and development of methods for control of both pathogenic and non-pathogenic (but significant?) biological growths and corrosion in water distribution systems.
— the identification of circumstances under which water softening is justified for esthetic and/or economic reasons in spite of its potential negative public health effects.
— the identification, characterization and development of methods for control of owner-induced pollutants to water supplies, a source of pollution specifically exempt from control in the Safe Drinking Water Act.

Let me comment briefly, on several of these areas. On June 27, 1977, the U.S. Environmental Protection Agency issued new drinking water standards which established "maximum contaminant levels for several parameters associated with drinking water supplies." The "maximum contaminant level" means the "maximum permissible level of a contaminant in water which is delivered to the free flowing outlet of the ultimate user of a public water supply system, except in the case of turbidity where the maximum permissible level is measured at the point of entry to the distribution system. Contaminants added to the water *under circumstances controlled by the user,* except those resulting from corrosion of piping and plumbing caused by water quality, are *excluded* from the definition(5).

During the period the final standards were developed, consideration was given to establishing a maximum permissible total bacterial count (500-1000/ml @ 20°C growing on plate count agar) to limit the total number of bacteria in finished water. Although a total count limitation was not established, interest and concern for high total plate counts has not gone away. High plate counts can result from:

— introduction and growth of chlorine-resistant organisms in the water system(6).
— removal of chlorine in the distribution system with subsequent growth of microorganisms normally controlled by chlorine residuals.

According to Geldreich, et al. (7):

"With proper treatment and adequate chlorine residual, finished water leaving a treatment plant should have a very low bacterial density. However, as a result of inadequate treatment procedures or *contamination within the distribution network,* the bacterial flora of a finished water may include, among others, Pseudomonas, Flavobacterium, Chromobacter, Proteus, Klebsiella, Bacillus, Serratia, Corynebacterium, Mycobacteriau, Spirillum, Clostridium, Arthrobacter, Gallionella, and Leptolthrix."

The introduction of excessive numbers of bacteria in water distribution systems results in several potential problems:

— High plate counts can contribute to taste, odor, and food spoilage problems in products produced by food, beverage, cosmetic, and drug industries.
— Substantial populations of bacteria may include some genera which could constitute high health risk to patients in hospitals, clinics, nurseries, and rest homes (8,9). As a possible risk in a hospital environment, Flavobacterium was reported as a primary pathogen for some surgical patients (10).
— Non-coliform populations in finished water have been implicated in suppressing coliform growth in test media. The critical level for such suppression occurs when the general bacterial population exceeds 1000 per ml.

So, how does this concern owner-controlled introduction of pollutants? Undoubtedly, the two most numerous pieces of equipment used by home owners in both public and privately owned systems are water softeners and water filters, many of which include carbon for taste and odor control. As far back as in 1922, Baker demonstrated that zeolite water softeners filter bacteria from water and that water with an initially low bacteria count may increase in bacterial content upon passage through such a softener(11). As a result, several studies have been conducted to determine methods which could be used for disinfection and/or sterilization of such water treatment media (12,13). An important observation by Klumb in 1949 was the fact that although experimental evidence indicates that the siliceous and resinous exchangers are *incapable,* in themselves, of sustaining bacterial growth, many bacteria *may grow* in the softener material *because of the presence of filtered organic matter,* especially during optimum temperature conditions.

In 1969, Stamm, et. al., conducted studies of the effluents backflushed from 143 separate exhausted urban and rural tank resins and cleaned resins containing the sulfonated copolymer of styrene and divinylbenzene (5DB) and 44 different bacterial and fungal genera were identified, including a pathogenic staphylococcal strain (14). Since cationic exchangers of the sulfonic type have been shown to take up all cations, both organic and inorganic, growth of bacteria on the organic matter is to be expected. Stamm concluded that increased bacterial retention, survival, and multiplication occurred concomitantly with accumulation of organics and inorganic materials and the Ca^{++} and Mg^{++}

cations from tap water.

In a similar manner, Wallis, et al., in 1974 (15) discussed the hazards of incorporating charcoal filters into domestic water systems. He reported, for example, that the number of bacteria discharged from a Tareyton carbon filter installed on a cold water tap to simulate home use showed a growth in bacteria discharged from about 200 per 100 ml at the time the unit was placed in service to over 7,000,000 bacteria per 100 ml in only 6 days. Further studies indicated that such growth was not caused by the filter ingredients themselves but were due to the removal of organic matter by the carbon which served as the food source for growth. He repeated Geldreich's contention that "Bacteria when present in water in small numbers may be innocuous to man, but when ingested in large numbers may be capable of causing disease. In the event that a municipal water system becomes contaminated with pathogenic bacteria, they might pass through the household water system in numbers too small to cause disease. However, if the bacteria were concentrated on a charcoal filter, they could multiply in the water conditioners to a high enough dose to cause illness"(15).

The adsorption of organics on all surfaces such as sand filter media, resins, and activated carbon has been recognized by many. Johnson and Baumann (16), for example, reviewed adsorption literature and proposed a new type of wastewater treatment process based on the observed effects. It is clear that the concentration of organics on surfaces can be several orders of magnitude greater than the concentrations of the organics in water. Thus, any home treatment device that introduces large surface areas, such as filters, softeners, or carbon units, will enhance the growth of microorganisms on the media surfaces. The problem is merely more acute using carbon, for the simple reason that the carbon also reduces the chlorine residual which might serve to keep bacterial growth under control.

What treatment methods need to be developed to control the introduction of pollutants by owner-controlled treatment systems? Or, do we ultimately relegate home softeners and filters to oblivion because of our inability, through lack of fundamental research, to control bacteria growths in such media?

APPROACH TO DEVELOPMENT OF FUNDAMENTAL RESEARCH PROGRAMS

The functions of a university are to conduct programs of teaching, research, and service. The teaching program should be effective in training students at both the undergraduate and graduate levels in the principles and practices that are essential to the solution of problems in a given area of endeavor. The research program should be designed to:

— develop a body of knowledge that will provide a fundamental basis for evaluating the scientific and engineering principles that will expand our understanding of the "truth" in specific areas of importance to mankind.

— provide opportunity for student participation in research, and financial support for that participation, so that the student can develop ability to apply the results of both fundamental and applied research to the solution of future problems.

The service program should be designed to convey the results of both fundamental and applied research and methods for their scientific and engineering application to industry and the people practicing therein.

In order to enhance the development of sound, effective, fundamental research programs of research required to solve the major drinking water supply problems outlined earlier, it would be desirable to create or to support already existing "Centers of Excellence" in each problem area. Such Centers of Excellence should involve:

— the creation of an interdisciplinary research team of engineers and scientists with expertise of significant depth and variety in the problem area.

— the development of laboratory space and modern equipment required to isolate, identify and characterize water quality constituents and to permit development and evaluation of new technology for their control. Table I, for example, lists the equipment now being used in our sanitary engineering studies of particulate identification and characterization. Note, for example, that some equipment, such as our x-ray diffractometer, are obsolete for the purpose we now need to use them—quantitative identification of crystalline particulates.

— the development of a "primary core" of scientific and engineering courses required to enable the scientist or engineer to practice in general areas in the water works industry and a "secondary core" of courses required to provide the additional background required to obtain competence in the special area of the "Center of Excellence."

— the development of a continuing 5-year program of research with specific objectives in the problem area even though the objectives are not related to any current or expected problem.

One specific and major failure of the current Environmental Protection Agency research support program is that it is too problem oriented without sufficient continuity to permit development of true Centers of Excellence in fundamental research areas. The availability of $5,000,000 or even $100,000,000 per year in a research area this year followed by nothing next year contributes to the destruction of rather than the creation of fundamental research programs. Little fundamental research knowledge is created when an academic staff must jump from one research problem this year to a completely new problem in an area new to them the next. A program of long-term funding of Centers of Excellence is essential if academic institutions are going to be able to assemble a qualified staff and the facilities required to contribute fundamental knowledge and the personnel to apply it in water quality problem areas. Applied research is enhanced when it is based on the availability of a fundamental understanding of basic principles. Both types of research are needed, but EPA supports only applied research and the fundamental research needs have not and do not now receive adequate, long-term support.

This panel is going to focus on several problem areas:

— Drs. R.S. Engelbrecht and V.L. Snoeyink of the University of Illinois will discuss fundamental research needs with regard to microbial, viral, and organic contaminants.

— Dr. Charles R. O'Melia, a member of the Research Committee of the American Water Works Association, will discuss fundamental research needs with regard to solids-fluids separation systems.

— Robert Peters, President of the American Water Works Association, will discuss attempts by that organization to create a Research Foundation that can provide long-term funding of research of both a fundamental and an applied nature that are important to the water supply industry.

THE CRISIS IN FUNDAMENTAL RESEARCH IN WATER SUPPLY AREAS

Many of you may already have concluded that our university educational and research programs are currently operated on a sound basis. For the last 15 years, we have had EPA support for training of professionals for work in the water supply and pollution control field. For years, however, that support has been restricted to the support of masters-level graduates, preferably those on a terminal degree program so that the entrance of new professionals the field could be facilitated. Study programs were, therefore, designed to cover general principles and their application in the water supply and pollution control field. Advanced study at the doctoral level—the level at which most of our increase in fundamental knowledge is generated—was handicapped and not facilitated by the EPA training program. Where did this leave us in development of educational and research programs? Let us look at only one area—our ability to train students and conduct research in the isolation, identification, and evaluation of the significance of suspended particles in drinking water and the development of technology for their separation from water (Solids/Fluids Separation Technology).

Turbidity, used as a measure of suspended solids, has been included as a parameter that has had a maximum permissible concentration established under current primary drinking water standards. Prior to 1962, the turbidity of drinking water had to be less than 10 turbidity units. In 1962, the standards reduced the maximum permissible turbidity level to 5 turbidity units, primarily because that was an achieveable goal and not because of any fundamental research that related that turbidity level to desirable health results. The now-current turbidity level of 1 turbidity unit was established as a primary drinking water standard because a high water turbidity is an indication that a water may produce an adverse health effect (17). A low turbidity water, however, does not guarantee that a water is potable. The American Water Works Association, for example, has a drinking water quality goal of 0.1 turbidity unit, an order of magnitude better than the current drinking water standard. Turbidity does not measure the type, number, mass, size, or other characteristics of the particulates present. A water with a turbidity of 1 unit may still contain thousands of particles per ml over 2.5 μm in diameter. Even water whose turbidity is less than 0.1 turbidity unit can still contain hundreds and even thousands of particles per ml, particularly if all size ranges of particles could be counted. Which of these particulates, if any, are harmful to health? Such particles need to be identified, counted, and methods developed for their removal from water. Adin, Baumann, and Cleasby recently reported (18) results that indicate a water treatment plant was unable to reduce water turbidity below a level of 0.3 turbidity unit. Particle size analysis of the raw and filtered water indicated that, whereas particles large enough to be removed by straining mechanisms were removed in greater percentages, only 74-75 percent of the smaller particles in the 2.5-4.0, 4-6, 6-8, and 8-10 μm size ranges were removed. They concluded "that not all particles found in water will interact with the same chemical treatment under the water ionic content and pH conditions. It is probable that some waters will contain colloids that will need a separate treatment from that accorded to the bulk of the particles present to effect their removal"(18). They also suggested that the flocculant-particle interaction must take place uniformly and will require a careful design of the mixing scheme, but, they concluded, not enough information is currently available to effectively design such systems and more research should be undertaken in this area. Polymer-particle interactions are the key to selecting polymers for different water utility interactions. However, to select the proper polymer for a given application, we must know more about the characteristics of both the available polymers (19) and of the particles and the filter media that they are supposed to interact with (7). How can we do this effectively without adequate facilities, without scientists and engineers trained adequately in the principles and techniques used in solids/fluids separations, and without long-term support? Do we now have adequate facilities, personnel, and support?

In the November/December, 1973 issue of Filtration and Separation (20), a statement on "The Crisis in Solid-Fluid Separation Technology" was published. This statement was evolved from the number of meetings of members of INCOFILT (International Consortium of Filtration Research Groups) whose members found that they could identify experts in deep bed filtration, in precoat filtration, in particle technology, in cake filtration, in thickening, in centrifugation, working with potable water, with wastewater, and with air. However, there were no people and no universitites with even a reasonable balance of effort in solid-fluid separation technology. Even in universities with significant expertise in one area, filtration, for example, they found that the program lacked emphasis (and course work) in areas directly related to that process. For example, all solid-fluid separation processes require the separation of particulate matter from suspension, hence research progress in any process must depend on a basic knowledge of the particle technology (size, shape, surface characterization, structure, composition) involved in the particulate matter the process is designed to remove.

The "Crisis" statement was signed by a "number of well-known people in the filtration world," including the following Americans with activities in water supply and pollution control: E.R. Baumann, recognized for work in granular media and precoat filtration; Richard I. Dick, recognized for work in activated sludge thickening; Robert B. Grieves, recognized for work in flotation processes; Charles R. O'Melia, recognized for work in granular media filtration; Frank M. Tiller, recognized for work in cake filtration; Don A Dahlstrom, recognized for practical application of solid-separation techniques in both industrial process and pollution control; Lloyd A. Spielman, recognized for work in granular media filtration. The statement includes the following:

> "The authors of this paper view with concern the present state-of-the. art in solid-fluid separation technology, particularly in those areas commonly referred to as cake filtration, granular bed and cartridge filtration, centrifugation, dust collection cycloning, scrubbing, electrostatic precipitation, thickening, flocculation, and deliquoring of cakes. Solid-fluid separation operations are basic to treatment of waste streams, filtration in the chemical and allied industries, and mineral and solid fuel processing. With tightening environmental restrictions, increased utilization of poorer grades of raw materials, and mounting fuel costs, the existing dearth of teachers, researchers, and specialists in solid-fluid separation technology presents a real danger to finding adequate solutions to long neglected, but key areas involving particulate separations.
>
> It is against this background that the authors of this statement emphasize the importance of particle science, technology, and separation in critical problem areas faced throughout the world. There is a dangerous lack of knowledge and knowledgeable people. *Programs to solve existing deficits must be put in action immediately if foreseeable consequences are to be avoided a decade hence.* Nothing can be done quickly. It will take time to develop research, plan and enlarge curricula, introduce training programs for engineers in industry, and in general increase activity in the field."

This statement emphasizes that educational institutions should provide:

1. Basic education in particle science and surface chemistry
2. Optional course sequences in solid-fluid separation at the undergraduate and graduate levels
3. Increased research activitiy
4. Encouragement to faculty members to develop expertise in particle separation.

As a result of publication of the "Crisis" statement, Dr. Joseph A. Fitzpatrick of Northwestern University conducted a survey of U.S. and Canadian universities to provide current data on education and research activities in areas of solid-fluid separation. His survey led him to conclude that "there is a serious lack of research as well as educational opportunities at most universities. Less than a dozen schools offer any course, graduate or undergraduate, with major thrust in solid-fluid separation. *If there is already a critical shortage of trained people in solid-fluid separation technology, the situation will not improve in the near future.*"

A questionnaire was sent to the university departments of chemical, civil, environmental, mining and mineral processing, materials and metallurgy, and petroleum engineering (182 schools) with a 70 percent return. The results indicated "that *no* university has an organized program in solid-fluid separation technology in general or even filtration technology in particular." In general, *universities (and individuals) are recognized for their research accomplishments rather than their programs.* Fitzpatrick suggests that this is due to emphasis by the universities on support of individuals rather than support of "a whole interdisciplinary group working toward a long-range objective."

> "Universities train or educate three categories of technical people, the scientist, the engineering scientist, and the engineer. The hierarchy of their function is: the scientist discovers new knowledge, the engineering scientist applies this to provide design criteria, and the engineer uses these design criteria to accomplish an economical design. In solid-fluid separation technology, our ignorance is manifest in particularly the first two categories, basic science and engineering science. Thus, university education for filtration and separation should provide training for each category,

but a redoubling of effort in the categories of basic and engineering sciences, for it is in these areas that we look for new separation processes and major improvements of old ones."

This is the university situation in areas of solid/fluids separation technology. This is the university situation in all of the other areas of research needed in water supply which are going to be discussed today. The seriousness of these problems demand that we find a better way.

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TABLE I

MAJOR EQUIPMENT USED IN PARTICULATE IDENTIFICATION AND CHARACTERIZATION AT IOWA STATE UNIVERSITY

Equipment	Date Purchased	Cost, $	Description	Functions	Proposed Utilization
Transmission Electron Microscope (TEM)	1961	51,700	SIEMENS ELMISKOP I. Magnification 200X to 200,000X with 10A point to point resolution. This instrument is equipped with an image intensifier and an X-ray microanalyzer.	Investigations of micro and submicro particle size, distribution and shapes; identification of crystalline constituents and their crystal structure.	Characterization of micro and submicro particles.
Scanning Electron Microscope (SEM)	1971	67,000	JEOL JSM-U3. Magnifications 20X to 100,000X with 100 A resolution. This instrument is equipped with an EDAX International energy dispersive X-ray analyzer.	Investigations of particle size, shape, and interactions; micro and macro structures; grain boundary reactions; qualitative and quantitative element analyses; element distributions.	Characterization of particulate materials and their surface interactions.
Thermal Analyzer	1972	26,000	RIGAKU. Differential thermal analysis, thermogravimetric analysis and derivative thermogravimetric analysis in the temperature range of -80 to 1000° with a 1 microvolt and 1 microgram sensitivity under controlled atmosphere including high vacuum.	Identification of constituents of materials. Investigation of interactions between solids and gases or vapors. Investigation of phase equilibria.	Identification of constituents of particulate materials and their interactions with environment including adsorption of organic ions.
X-ray Diffractometer	1956	16,000	GE XRD-5. X-ray diffraction and fluorescent analysis. This instrument is equipped with a crystal monochromator, a high temperature furnace, a controlled pressure compression sample holder, a single crystal orienter, Debye-Scherr and Land cameras for forward and back reflections.	Identification of crystalline constituents of materials, their crystal structures, crystallite sizes and elemental constituents. Investigation of the effects of temperature, pressure, and gases and vapors on crystallography and structure of materials.	Identification of cyrstalline constituents of particulate materials. Investigation of the effects of various environments on behavior or particulate materials through structural and crystallographic changes including ion exchange and adsorption of organic species.

Equipment	Date Purchased	Cost,$	Description	Functions	Proposed Utilization
Coulter Counter	1972	16,000	COULTER COUNTER MODEL TA-II Counting capacity of 0.5 μ m to over 800 μ m. This instrument is equipped with triple volume manometer, different aperature tubes, and aperature observation microscope. Results are automatically displayed and printed out by a 16-channel analyzer.	Investigations of fine particle size distribution.	Characterization of fine particle distribution and the effect of various environments on particle growth and size distribution.
HIAC Particle Counter	1977	14,200	HIAC Counter Model 320 Counting capacity of 1.0 m-60 m using light blockage techniques. Results displayed in 12 separate channels.	Particle size distribution of suspended solids in water.	Characterization of materials.
Zeta Reader	1975	10,000	400 Lazer Zee Meter.	Investigation of particle surface charges.	Characterization of particle surface charges and the effect of various environments including adsorption of organic ions on surface charge.
Colloids Disperser	1977	1,050	SD45N SUPER DISPAX with G454 generator (Tekmar Co.). 10,000 rpm mixer with a suction head exerting high hydrodynamic shear forces. Applicable for different suspensions and organic solvents.	Investigations involving colloidal suspensions of clays and other particles.	Provides fine particle distributions in a reproductible manner.
Infrared Spectrophotometer	1966	15,000	Beckman IR-4 equipped with a double beam Internal Reflection under controlled temperature and atmosphere including vacuum. Transmission and Internal Reflection Spectroscopy of bulk and interfacial materials.	Identification and quantitative analysis of structural units. Investigation of thin films and adsorbed species. Investigation of the spectra of powders in microgram quantities.	Determination of the constitutional and structural properties of suspended particulate materials in microgram quantities. Investigation of adsorption of organic ions by particulate systems and other interfacial reactions.

Equipment	Date Purchased	Cost,$	Description	Functions	Proposed Utilization
Mercury Porosimeter	1974	10,300	MICROMERITICS 915. This instrument has a pressure range of 0 to 15,000 psi corresponding to pore size range of 20 to 80,000 A.	Analysis of pore size, pore distribution, and pore structure.	Investigation of internal and external pore structure of particulate materials.
Adsorption Isotherm Apparatuses	1965-1968	approx. 25,000	Consists of two gravimetric and one volumetric apparatuses. The two gravimetric apparatuses were built at the ISU Engineering Research Institute Soils and Materials Laboratory using Calin Electrobalances and MKS electronic manometers. Sensitivity: 1 microgram and 0.001 millimeters of mercury. The volumetric apparatus consists of a MICROMERITICS ORR Analyzer.	Determination of adsorptive properties, specific surface and pore size distribution of porous and fine particulate materials.	Investigation of surface chemical properties of particulate systems.
Constant Speed Constant Torque Control	1973	1,000	Cole-Palmer, 0-300, 0-3000 rpm speed control, 0-180 mv. D.C. torque control. Electrocraft Corp. Motomatic Motor generator.	Control energy input to coagulation-flocculation studies.	Coagulation-flocculation studies.

FUNDAMENTAL RESEARCH NEEDS IN WATER TREATMENT MICROBIAL, VIRAL AND ORGANIC CONTAMINANTS

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INTRODUCTION

There are many aspects of water treatment which require additional research. One of the more important problems is the health effect associated with the continuous intake by the public of low levels of contaminants over long periods of time. In many instances the analytical techniques to qualitatively and quantitatively characterize chemical contaminants of this nature, as well as biological agents, which might be present are either deficient or lacking. Concomitant with the health effect and characterization studies, research on water treatment processes, both established and new, is needed to determine how they can be operated to give maximum removal of specific contaminants at the lowest costs. Process monitoring techniques which can be used to insure that only water of acceptable quality is distributed to the consumer should also be developed. This paper focuses on research needs primarily related to biological and dissolved organic chemical contaminants. No attempt has been made to provide a comprehensive coverage of this topic. Rather, selected needs have been presented in an attempt to show that there are several areas of additional research required in water supply.

In order to consider the quality of public water supplies, it is first necessary to identify the problem. To do this, it is important to determine if specific impurities are present and, in many instances, their concentrations; the chemical, physical and biological characteristics of these impurities; and the effect of these impurities on human health. With this information available, safe levels of impurities in water for protection of public health can be determined. Also, this information can be used to select or develop processes required for their removal, or inactivation in the case of microorganisms.

HEALTH EFFECTS RESEARCH NEEDS

Incidence of Disease

Although the incidence of enteric disease in the United States has been reduced over the years, there were 28 reported waterborne disease outbreaks in 1974 with 8,413 cases. In 1975, there were 24 outbreaks and 10,879 cases. These outbreaks and the resulting cases were caused primarily by microorganisms, e.g., 81 percent of those reported in 1974. Overall, the category designated "acute gastrointestinal illness" accounts for the largest number of cases. Unfortunately, no specific etiological agents have yet been identified for these gastrointestinal outbreaks and studies to determine them should be intensified as should studies on the control of waterborne epidemics.

Virus Problems

The presence of viruses in water is an aspect of microbiological quality that has received considerable, but not nearly enough, attention in recent years. At the present time it is difficult, if not impossible, to establish rigid standards regarding the allowable level of viruses in public water supplies. For example, according to the World Health Organization (WHO), "If not even one plaque-forming unit (PFU) of virus can be found in 1 litre of water it can reasonably be assumed that the water is safe to drink. It would, however, be necessary to examine a sample on the order of 10 litres to obtain a proper estimation of the PFU's at this level." The "European Standards for Drinking-Water," published by WHO, indicates that there should be no viruses in drinking water when 10 liters of water are examined. On the other hand, it would appear that the "Canadian Drinking Water Standards" does not permit any viruses in drinking water, regardless of the quantity of water analyzed. The significance of these standards depends on the method employed in concentrating and assaying the viruses present, and as yet no concensus has been developed regarding this measurement of water quality.

It has been observed that there are viruses with no known associated disease. On the other hand, such virus may possibly interfere with genetic transfer and thus have long term health implications. That such viruses might be transmitted through the water route is not known. It is significant to note that many believe that an individual need only ingest one detectable virus unit to develop an infection. However, even this concept needs to be further investigated.

Although many viral diseases have been accused of being transmissible by water, only infectious hepatitis is known to be significant in this respect. However, it has been suggested that waterborne transmission of enteric viruses at low levels could occur without the presence of a type of disease outbreak recognizable by current epidemiological methods. This then is an area of considerable importance and in need of further research.

Organic Chemical Problems

The presence of certain organic chemicals in public water supplies has been implicated as having mutagenic or carcinogenic activity. However, the full impact of specific organic compounds in this respect is unknown. Cause and effect studies of this nature are required. The health effect of mixtures of organic compounds must be similarly investigated. There also is need for improved procedures for determining the health effect of chemicals. For example, the applicability of the Ames test for mutagenicity should be studied more intensively.

Inorganic Chemical Problems

The toxicological significance of many inorganic elements and compounds found in water is unknown. For example, the precise cause and effect relationship of sodium and cadmium, as well as water hardness, to cardiac problems still remains unanswered. The significance of these and other elements to cardiovascular diseases and hypertension needs to be determined. The ingestion of water as it might relate to mineral accumulation in the body, leading possibly to the formation of gall and kidney stones, deserves further study.

Suspended Solids Problems

There are also questions regarding the presence and significance of suspended materials in water. This particulate matter may be either inorganic or organic in nature and consists of particles ranging in size from colloidal to more than 100 micrometers. Although there may be other substances and microorganisms attached to them, these particles, such as with asbestos fibers, may have health implications in themselves. On the other hand, their significance may be indirect in that they affect the quality of the water by acting as a means whereby other contaminants are concentrated and, perhaps, transported to another site only to be released unaffected; e.g., microorganisms.

A high concentration of particulate matter, giving a measurable high turbidity, is an indication that a water may produce an adverse health effect. However, there is little reason to believe that a low turbidity measurement guarantees a satisfactory quality water. Measurement and related health effects of particulate matter in water supplies deserves further attention. Additional studies should be undertaken regarding the interaction between viruses and suspended solids or particulate matter in water, including the protection from disinfectants offered by the particulate matter.

Approach to Research on the Health Effects of Chemicals

The two general ways to assess the health hazard associated with ingesting chemical pollutants that might be in a public water supply are epidemiological investigations and laboratory studies of toxicity. Both approaches are aimed at providing information on the health hazard as it relates to man. Most of the current knowledge of toxicity of various constituents is based on observation of their effect upon man and animals using doses and/or dose rates that are much larger than those that correspond to the usual concentration of harmful materials in drinking water. As a result, there is great uncertainty in estimating the magnitude of the risk to health that ingestion of specific contaminants in water may produce. An additional problem is presented by the potential combined effect of two or more contaminants. Obviously, this is an area in need of considerably more research. Water quality standards and criteria can be established reliably only if the threshold level of the health effects of various contaminants that may be in water supplies is known. For example, considering only the inorganic constituents, additional toxicological information is needed with respect to barium, beryllium, cadmium, and other elements. The health effect of trace amounts of various organic compounds must also receive further attention. Thus, the health effects of various inorganic constitutents as well as organic compounds need to be identified as they affect the establishment of water quality standards and the design of treatment facilities. In considering health effects, it is important to note that there are beneficial effects of many inorganic constituents that might be found in water, such as fluorine, iodine, cobalt, chromium, arsenic, manganese, vanadium and zinc.

SOURCE QUALITY

There are many areas associated with the quality of raw water sources which require study but only a few are selected for discussion here. Other areas such as surface and groundwater basin management to control quality are also important.

Plankton Bloom Control

Plankton blooms in reservoirs can lead to troublesome taste and odor problems which require expensive treatment methods for control. In recent years, aeration for reservoir destratification and mixing has been used to control these blooms. Although this procedure has been successful in certain instances, additional information is needed so as to ascertain the reasons for successes and failures at various geographical locations throughout the country. The reasons for reduction in the plankton population during reservoir mixing and a shift in organism predominance toward the less troublesome green algae needs to be explained. The older and perhaps the most common method of controlling plankton blooms in reservoirs is through the application of copper sulfate. There is need for more information regarding the effective copper concentration required for algacidal action against specific microorganisms without a destructive impact on the surrounding biota. Additional metabolic studies of the copper reactions with algae are also indicated. This and related information would assist in determining the proper timing and placement of copper sulfate in reservoirs.

Groundwater Quality

The potential for pollution of groundwater, because of increased recharging operations, irrigation with reclaimed water and the increased use of agricultural chemicals, should be evaluated further. Specifically, the effectiveness of soil percolation as it affects the removal of viruses, metals and organic compounds in water should be studies. These investigations should give consideration to various soil types and their combination as well as to climatological conditions. rate of application, etc. Long-term studies to determine the possible buildup of pollutants in soils are also indicated. An inconsistent relationship between total coliforms, fecal coliforms, and standard plate count bacteria has frequently been reported in groundwaters. This would indicate that perhaps a different or, at least, a more significant means of determining the bacterial quality of groundwater is required.

Water Reuse

Indirect reuse is being practiced in all 50 states and in other countries. Further, direct water reuse may be necessary in the near future, e.g., the year 2000. Broadly speaking, there is need to determine the health effects of and the quality criteria for reclaimed water. To obtain the information necessary, it will be necessary to perform more detailed and, perhaps, more sophisticated toxicological studies than have been reported in the past. If a reclaimed water has been found to be toxicologically safe for public consumption, the level of the physical, chemical and bacteriological constituents of the acceptable water should be determined so as to establish the required quality criteria for the direct reuse of water. In this respect, there is need to identify a reliable protocol for evaluating the acceptability of a reclaimed water for direct reuse.

ANALYSIS OF BACTERIAL AND VIRAL CONTAMINANTS

Deficiencies of Existing Tests

The precise importance of viruses in water is now known, primarily because there is no simple, routine procedure for the recovery, detection, and enumeration of viruses in water. Clearly, there is a need for developing a simplified procedure for determining the presence and density of viruses in drinking water. Such a procedure is needed for selected monitoring of water supplies.

The current microbiological tests to determine the presence of fecal contamination of a drinking water require a minimum of 18 hours. Thus, a positive test indicates that water which was treated 18 or more hours previously contained bacterial contamination; with this time lag, the water probably would have already entered the distribution system. Thus, there remains a need to develop a rapid, sensitive indicator test to determine the presence of pathogenic or infective agents in drinking water. Such a test may be microbiological in nature or it may turn out to be a chemical, such as coprostanol which is a chemical contaminant which is produced only in the intestinal tract of warm-blooded animals. With respect to the present coliform test, there is need for additional studies on the influence of turbidity as an interference in their enumeration.

Based upon available epidemiological data, it would appear that the present coliform standard is adequate to protect public health when water is obtained from a protected source and is appropriately treated and distributed in a contamination-free system. However, this may not be the case for water reclaimed directly from wastewater. Additional microbiological standards are needed in this situation. This may involve the determination of viruses or some other more sensitive indicator bacteria. Use of the standard plate count for determining the acceptability of a reclaimed water should also be considered. This will require that there be a correlation between the standard plate count and the health hazard associated with drinking water.

Virus Identification Needs

There is epidemiological evidence to indicate that infectious hepatitis is transmissible through drinking water. Unfortunately, the etiological agent, presumably a virus, for infectious hepatitis (hepatitis A) has not been cultured in the laboratory. Therefore, the current viral detection methods do not determine the virus of infectious hepatitis. There is need to develop a system for detecting, recovering and isolating this particular virus in order to evaluate its persistence, particularly in the case of epidemiological studies, and its removal by water treatment processes. The same type of information is needed for the ill-defined gastrointestinal disturbances which are believed caused by a viral agent(s).

There is need for standardization of virus detection methods, with special attention being given to the critical need to quantitate the recovery of small numbers of viruses from large volumes of water and the detection of viruses under adverse environmental conditions. Further information is required on the virus removal capability of various unit operations and processes employed in the treatment of public water supplies. Emphasis should be place on naturally occurring viruses, and such studies should extend to both pilot plant and field investigations.

Considerable progress has been made in recent years in the detection and enumeration of enteric viruses in water. Most acceptable methods today involve clarification to remove interfering substances (e.g., particulate matter), adsorption, elution, and finally reconcentration. Each water system can present different problems in the monitoring for viruses; it would appear that no single method has yet been found that is applicable in all cases. Procedures utilizing the above indicated steps have been found to be generally acceptable in determining the presence of the more usual enteric viruses, such as polio, echo and coxsackie. Factors that complicate virus concentration, and especially reconcentration, can be identified as breakthrough, floc formation, or co-concentration of cytotoxic agents. Each virus recovery method should be assessed in light of each of these factors, as well as the spectrum of viruses recovered. Research and developmental studies should be continued with the object of developing a method with universal or, at least, broad utility for detection of enteric viruses in water supplies. Detection of viruses is complicated beyond that associated with the concentration, adsorption and reconcentration steps in that no single cell system has been found suitable for the detection of all of the known enteric viruses.

Virus enumeration has progressed to the point that an extremely small number of viruses in large volumes of water can be determined under ideal conditions, i.e., 1 virus or infective partical per 100 gallons. Through more extensive studies, using virus monitoring and concurrent epidemiological data, it should be possible to determine the practical significance of this level of detection and its adequacy for the protection of public health. It may be that such studies will show that more sensitive methods of detecting viruses are required if waterborne outbreaks of viral diseases are to be eliminated completely.

Relation to Particulates

Studies are needed to support current evidence that particulates may be significant in the concentration and transport of pollutants, including microorganisms. Further studies are needed to correlate raw and treated water supplies with respect to turbidity and microbial association. Interference of particulates with the inactivation of viruses, pathogenic bacteria and indicator bacteria with respect to disinfectants such as chlorine, chlorine dioxide and ozone, is not fully known.

CHARACTERIZATION AND IDENTIFICATION OF ORGANIC COMPOUNDS

There is a definite need to improve the database for the kinds of organic compounds which exist in water supplies. This information is needed as a basis for research on the removal of potentially harmful compounds. Additional information is needed to both identify and quantify the organic compounds which are present. Current analytical procedures are much better for the low molecular weight volatile compounds than for the non-volatile, generally high molecular weight compounds. Special attention should be given to the latter and to compounds which produce a specific effect such as odor. The development of simple, reliable tests for gross contamination, or groups of contaminants such as organic chlorine, is especially important, particularly if these parameters can be correlated to health effects. The test for total organic chlorine is potentially very useful in this regard. There is still need, however, to develop improved but simple analytical procedures for specific types of organic compounds, e.g., nitrosoamine, nitriles, organophosphates, etc.

There is a great need to establish parameters which can be used by operating personnel to efficiently operate their plants for removal of organic compounds. The group parameters such as total organic chlorine, total organic sulphur, etc., appear potentially useful in this respect. Detailed measurements of many individual compounds which might be present do not appear possible for the purpose of plant control. A recent survey has shown almost a complete absence of monitoring procedures for organic compounds in use by the water treatment profession.

Taste and odor problems occur predictably and unpredictably, seasonally and sporadically, in a wide variety of forms that constantly challenge the ingenuity of the water treatment plant operator. Nearly every water supply system is plagued with such problems. The present method for determining taste and odor in water is subjective in nature and, therefore, variable in its reliability. Additional information is needed on the causes and treatment of taste and odor substances. Specifically, there is need for the identification and quantification, and the treatment of individual taste and odor causing bodies. Availability of such information will permit operating personnel of water treatment plants to attack the problem in a logical, scientific manner rather than by trial and error.

There is much that must be learned concerning the effects of the organic compounds which may be present in water. Also, little is known about the ability of organic compounds found in water to chelate heavy metals, and the ability of the natural organic compounds such as humic substances to associate with pesticides and other potentially harmful compounds. There is also much that must be learned relative to the chemistry of such compounds, especially their reactions with water treatment chemicals such as chlorine, ozone and chlorine dioxide. Also the haloform formation reaction and other reactions between chlorine and aqueous organic compounds must be more closely examined.

CONTROL OF CONTAMINANTS

Removal and/or Inactivation of Virus and Bacteria

The success in reducing the incidence of waterborne epidemics of a bacterial nature can be attributed directly to the treatment of water, particularly the use of chlorine as a disinfecting agent. On the other hand, existing data indicate that there are enteric viruses which are more resistant to chlorine than either bacterial pathogens or the commonly used bacterial indicators. Further, different enteric viruses vary in their response to chlorine. These observations should be investigated. Moreover, it is not known whether viruses can, through one means or another, be altered with respect to their resistence to chlorine or other disinfectants. There is also the question whether a laboratory cultured virus responds in the same way as a fresh viral isolate. With respect to the disinfection question, possible substitutes for chlorine should be considered, e.g., ozone, ultraviolet light, chlorine dioxide, bromine and iodine. Further, the precise effect of turbidity on the inactivation of viruses by all possible disinfectants is not known.

The most important waterborne parasitic diseases in the U.S. are caused by amoeba and giardia. Both of these parasites appear to be more resistant to chlorine than bacteria. Fortunately the cysts of both can be removed by proper coagulation-flocculation and filtration. With respect to these two parasites, a method for determining the viability of cysts of giardia is needed. The dose-response relationship for both amoeba and giardia is needed with respect not only to chlorine but to other potential disinfectants such as indicated above. However, in the case of giardia, a more reliable method of cultivation and enumeration is required first.

Removal of Organic Compounds

There is much to be learned concerning process control for the removal of different kinds of organic contaminants. Except for processes to remove organic compounds which cause color and odor, water treatment plants in the U.S. have not been designed or operated to remove organic contaminants. In this regard, ozonation, adsorption on carbon and synthetic resins, and other procedures specifically designed for the removal of organic compounds should be examined in detail with respect to many variables. Each process needs to be characterized as to what it can do so as to know what kinds of compounds must be eliminated from the raw water supply. Fail-safe monitoring systems need to be developed which will enable operating personnel to divert water containing organic compounds which are extremely harmful, and which cannot be removed by the available treatment processes.

The interrelationships of treatment processes need to be closely examined; for example, predisinfection with chlorine and other disinfecting agents is commonly practiced to prevent biological growths within the treatment plant and to improve terminal disinfection. However, the disinfecting agents can also react with activated carbon or compounds adsorbed on activated cabon when carbon is employed for removing organic matter. Are harmful compounds produced and is there a way to prevent undesirable effects if harmful effects are noted? This is a subject area which requires detailed investigation. Much more attention must also be given to minimizing the cost of treatment. For example, the organic compounds which cause haloform formation in some cases can be removed by a properly operated coagulation-flocculation process, and it may not be necessary to install new adsorption or other processes to remove such compounds. Operating the coagulation process in a way such that the removal of organic compounds is optimized may well result in a lower overall cost of treatment, considering reduced frequency of carbon regeneration or replacement if activated carbon is applied after clarification. Another example is the use of carbon following ozonation. This raises a number of questions concerning the microbiological development on the activated carbon as well as the optimum design of each process. Ozonation increases the biodegradability of organic compounds and, thus, extensive biological growths may result in the carbon bed. This growth reduces regeneration frequency required for the carbon but may result in a high bacterial population in the effluent. A well-designed process possibly should encourage biological growth but be controlled so as to prevent any adverse effects of the growth. Small scale test procedures also need to be developed such that design data for organic compound removal processes can be reliably obtained.

The use of chlorine to disinfect water supplies causes taste and odor at times. It has also been reported that chlorine reacts with certain organic compounds to produce carcinogens. Thus, there is a need to identify and determine the effectiveness of possible alternative methods to chlorination for disinfecting drinking water. The replacement of chlorine by some other effective disinfectant should be considered. Further, it may be possible to use some other chemical, such as ozone, chlorine dioxide, hydrogen peroxide, or potassium permanganate, in conjunction with chlorine so as to reduce the amount of chlorine necessary and thus eliminate any taste and odor problem or the formation of any carcinogens.

DISTRIBUTION OF TREATED WATER

There is good evidence that water quality can deteriorate to a significant degree in water distribution systems. Corrosion problems generally create nuisance conditions and require more frequent replacement of the distribution mains which is costly. Metal ions of various types may be leached from the piping. Biological growths may also develop in distribution systems thus creating odorous compounds, and increasing the rate of corrosion in some instances. The corrosion problem, accumulation of particulate matter and biological growth development in particular need to be better understood so that improved control measures can be identified and used.

SUMMARY

With respect to the questions associated with undesirable constitutents in public water supplies, the following summarize the need for additional research:

1. What substances may be found in public water supplies which potentially could adversely affect human health?
2. Based upon the effect of these substances, what are the maximum threshold levels necessary to protect human health?
3. Are present analytical procedures sufficiently reliable and sensitive to provide the required level of measurement of these substances?
4. To routinely determine the level of these substances in a water, what analytical procedures should be used for monitoring?
5. What treatment technology is required to reduce the concentration of these undesirable substances to an acceptable level?
6. Considering that some undesirable substances may be formed during treatment, or even during distribution of a public water supply, what steps should be taken to eliminate or minimize this potential problem?

Although it would seem that the most orderly approach to the above discussed research would be to determine the health effects of contaminants and then to investigate the procedures by which the contaminants can be eliminated or reduced in concentration, health effects data are difficult and time consuming to obtain. In view of this, a more rational approach would seem to be one in which studies on ways to remove suspected contaminants are performed simultaneously with studies to determine their actual health effects.

THE CRITICAL NEED FOR FUNDAMENTAL WATER RESEARCH

Robert R. Peters, P.E.
President, American Water Works Association

There is today a critical need for long-term, fundamental research and coordinated planning for the management of not only our nation's but the entire world's water resources. Our nation, particularly, faces interrelated problems of water quality, environmental considerations, water supply, land use, and energy conservation which require integrated planning and management. There is a need for increased governmental and non-governmental cooperation in the planning and implementing of water management programs. In many communities, water conservation—a sound way to avert critical water shortages—is now being made mandatory due to this year's severe drought conditions. Conservation of our water resources must necessarily become a joint venture of all governments—local, state, and federal—that plan and regulate water use. The public, too, must be educated on the long-term social, economic, and environmental ramifications of continued water abuse.

Some of the questions asked are: What are our nation's, and the world's, most critical water problems? Do we presently have the tools and technology to properly assess our water problems? Seven departments, with eighteen agencies, and seven independent agencies of the United States Government have water programs, funded through seventy appropriation accounts. In 1972, federal investments for water programs (not research) totalled 5.2 billion dollars, and in 1976, 10.2 billion dollars were spent for water quality improvements alone. Was this money spent wisely? Dollars not properly directed are not the answer. We need to tap the abilities of our intellectuals to provide our water resource managers the sound data upon which to base wise, farsighted decisions.

The world is not running out of water; rather, the problem is the wise management of same. Water managers cannot make really wise decisions without assistance from scientists involved in fundamental research on water problems. Water is a renewable resource; thus the solutions to many of our problems should be relatively simple to find—when given adequate attention and funding.

Water resource management is a common denominator in which all nations are involved. Nothing, including mankind, can survive, or be produced, without water. Water knows no boundaries. When it reaches a state line, or international boundary line, it continues to flow; therefore, water problems are international in scope.

The world's people migrated to and established great cities largely because of the nearby availability of a water supply. Many nations today have tremendous monetary resources but are lacking in water resources. Water is a product second only to air when man's fundamental needs for survival are considered. These requirements for survival are: air, water and food, in that order. I believe that water is a common problem affecting man's survival throughout the world, and it is therefore imperative that a safe and adequate water supply be provided to all people. No one people, or nation, can exist and thrive without a safe and adequate water supply. Today, approximately 75 percent of the world's population does not have safe drinking water, and 25,000 people per day are dying from diseases related to their water supply, or lack of same. Our world has existed for millions of years, but throughout its history great wars have been fought for the cause of religion or water.

Because water resource managers must have scientific input to help resolve their diverse problems, I believe the world's water problems must receive careful evaluation by the "think tank" of the nations—the intellectuals—and therefore have proposed the establishment of a World Water Foundation, similar in concept to the Rockefeller or Ford Foundations, whose endeavors in research would encompass the complete hydrologic cycle. My idea is to establish the foundation similarly in structure to that of the National Geographic Society, utilizing the guidance of some fifteen to twenty of the most renowned experts in the world who have established themselves in the field of water and have the qualifications that entitle them to recognition as world leaders. Monies that the foundation received would be placed in a trust, and the monies generated therefrom would be used to fund water research projects throughout the world, on a partial, complete, or matching fund basis, and encompass the complete hydrological cycle from source, to consumer, to waste, and back to source. To my knowledge, no foundation exists which specializes in the field of water.

The foundation's source of funds would be from industry, various governments of the world, philanthropists, and private individuals. The research projects funded by the foundation would, in addition to basic and applied research, include research into new product lines that would assist in the improvement of water supply, such as new pumping methods, or treatment processes, or means of locating water leaks. The foundation would be international in scope, allowing all nations of the world to participate in seeking solutions to the various problems inherent in providing better water for all people. In that water is of vital importance to all nations, it is possible that a foundation dedicated to unified action to solve water problems can contribute to world peace.

Our world might be compared to a spaceship, a home equipped with essentials for survival for a given period of time. Replenishment of essentials would be of prime consideration in flights of long duration. The comfort, even survival, of spaceship Earth's inhabitants might very well depend upon their facing the reality of their need to conserve and manage their precious resource, water.

As a goal for the inital funding of the foundation, I would envision $1.00 per person from sources in the United States (about 250 million dollars) and three to five billion dollars from sources throughout the world. It is obvious that a trust fund of this size would generate tremendous funds that could be used for vital water research throughout the world. It would not require funding on a yearly basis but would be self-perpetuating.

The idea was presented by me to the Board of Directors of the American Water Works Association in January, 1977, and it was greeted with enthusiasm. The senior science editor for ABC News thought that the project had tremendous potential and said that he would be honored to be a member of the foundation's board of directors. At the State Department, I talked with Mrs. Patsy Mink, Assistant Secretary of State, and also Mr. Roy Morey, who also felt that the project had great potential. They suggested several ways of furthering the project and offered to assist in whatever way they could. They agreed to collect data on world water projects and also to provide a list of those nations of the world that might contribute.

I presented the idea to Dr. Andy Breidenbach, formerly of EPA, who indicated that the foundation would be a great way to resolve world water problems and promised to investigate the possibility of getting EPA to provide monies for its establishment. I also discussed the plan briefly with the board of directors of the Water and Wastewater Equipment Manufacuters Association, who told me they were interested and would assist when provided further data. The president, president-elect, and a member of the executive committee of the Water Pollution Control Federation are interested and have allowed me time to present the idea to their executive board. I am awaiting their decision. After presenting this proposal in Canada recently, I have been requested to present the concept to the Council of Ministers at their meeting in Ottowa next January. Renowned leaders in the field of water, such as Dr. Abel Wolman, Dr. Mark Hollis, and Mr. Frank Butrico, as well as officers of The World Bank, have given me encouragement and support. I have talked with several congressmen and senators, who recognized that the foundation has tremendous potential and indicated their willingness to assist wherever possible. Just prior to my attending the AWWA Conference in Anaheim, I received a letter from Congressman G. William Whitehurst reiterating his support and willingness to assist in meeting with people or establishing meetings for furthering the project.

The American Water Works Association, National Water Well Association, Water and Wastewater Equipment Manufacturers Association and Water Pollution Control Federation are some of the major associations involved in the field of water, and therefore they are being asked to provide the seed monies for establishing the foundation, which is certain to have a great impact on the water industry and also a great reward for the associations. I requested $50,000 from the Board of Directors of AWWA toward the foundations' establishment and am happy to report that the Board provided this money, contingent upon the provision of matching funds in the amount of $100,000 from other sources. I will approach the Water and Wastewater Equipment Manufacturers Association's Board and the Water Pollution Control Federation's Board for their assistance. The legal work in establishing the foundation will not only require considerable time but also the contacting of many people. An Advisory Council will be chosen to select potential candidates for the foundation's board of directors and recommend guidelines for the actual establishment of the foundation. Upon incorporation, the foundation will proceed to solicit large donations.

In my travels I have had the opportunity to discuss the foundation with the heads of university engineering departments throughout our country. They have been most receptive and feel that the establishment of such a foundation would allow the water industry to do something for itself rather than its being dependent on our government for almost all research funding. As we see more and more government involvement, such an independent foundation becomes more and more important. In addition, the importance of water and the problems that we know exist require that water research be expedited. We don't have to look into the future to find major problems. We know population centers will grow, that agriculture and industry will demand more water, and we, as the experts in the field, must find the answers.

In reading a recent article by one outstanding member of the research field whose subject was food, I was amazed to find that in his listing of the things necessary to provide food for the world, water was not even mentioned! Water is taken for granted and is little understood; yet it is our most common and necessary resource. However, we are finding that it is being headlined more and more often as problems arise. If the United States can embark on a project to place a man on the moon and return him safely to earth, and place a vehicle on Mars for taking soil samples, analyzing them, and transmitting the data to earth, we certainly can solve the problems of water for our people and survival. We presently have the technology and the expertise to accomplish this goal. But I believe we must also have an independent foundation established to fully utilize the vast talent and technology that presently exist.

FUNDAMENTAL RESEARCH NEEDS IN WATER TREATMENT: PARTICLES AND POLLUTION

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INTRODUCTION

Research in environmental engineering has been beset and perhaps seriously impaired by the opinion shared for at least a decade by many scientists and engineers that we have sufficient knowledge and technology available to solve our environmental problems. In my judgment, (1) we do not even recognize many present and future problems, (2) we would not know how to solve many of these problems if we were able to recognize them, and (3) we have hardly begun to try.

Solutions require research, both fundamental and applied. This essay is directed towards one aspect of this broad and general need for research in environmental engineering, i.e., particles in water supplies. This selection is made to be illustrative and indicative; it is not comprehensive.

Conventional water treatment plants use coagulation, settling, and filtration processes; they are designed to remove particles from water. Since these have been in use for water treatment for up to a few thousand years, it is tempting to assume that we use them efficiently and effectively now. Such is not the case. We do not measure important physical, chemical, and biological properties of particles in water. We do not know the identity of the chemical species most commonly added to water to remove particles. We do not know well the reactions these chemicals undergo, and we certainly do not know the kinetics of the reactions of coagulants with substances in water. Our theories of flocculation and settling are inadequate. Filtration theories focus on clean filters and are invalid as soon as a run begins. We cannot measure important characteristics of the product water. In fact, we may not even know what all of these important parameters may be. Hence, we have problems in even describing the effectiveness of a treatment plant.

PARTICLES IN WATER SUPPLIES

The majority of the substances in water that may be hazardous to human health are either solid particles (e.g., asbestos fibers, viruses) or are associated with such particles (e.g., pesticides and toxic metals adsorbed on clays). These solid particles vary in size from about 0.005 to 100 μm, a range of over four orders of magnitude. Their chemical and biological properties also vary extensively. They may be organic or inorganic; they can occur naturally or be produced by man; they can be present in a raw water supply or added during treatment; they can be living or detrital.

Particles in water are measured at present by the mass of the larger sizes (suspended solids) or by their optical properties (turbidity). Unfortunately, their removal in water treatment plants and their possible effects on human health are only marginally related to these properties. Probably the most important characteristic of solid particles that affects their removal is size distribution. Particle size determines the transport of particles in solid-liquid separation processes such as flotation, gravitational sedimentation, packed bed and cake filtration, and centrifugation. Surface area (and hence particle size) influences chemical reactions of particles including interactions with coagulants and adsorption of contaminants. For particles larger than 1 or 2 μm, there has been very recent work determining their size in water using Coulter, HIAC, and Zeiss Videomat counters. Smaller particles still escape measurement. The chemical composition of solids and the distribution of chemical composition with particle size are not known. Similar inadequacies exist when the biological composition of solids is considered.

Ultimately, these problems in measurement must be solved. It is difficult to develop useful theories for solid-liquid separation processes in water treatment without knowing the size and composition of the solids to be removed. Similar problems also affect the design of water treatment plants in practice.

PARTICLES IN FLOCCULATION

Particle size distribution has pronounced effects on the kinetics of flocculation. Collisions between suspended particles are essential for coagulation. They occur in water by three distinct processes, viz., Brownian diffusion, fluid shear, and differential settling. The equations of flocculation for heterogeneous particles are cumbersome; for didactic purposes the collisions between particles of two different sizes are considered here. The analysis is patterned after Friedlander(1).

The rate at which particles of sizes d_1 and d_2 come into contact by the j^{th} transport mechanism is given by

$$N_j(d_1, d_2) = k_j(d_1, d_2) \cdot n(d_1) \cdot (d_2) \qquad [1]$$

Here $N_j(d_1, d_2)$ is the collision rate in collisions per unit volume per unit time, $k_j(d_1, d_2)$ is the "bimolecular" rate constant for the j^{th} mechanism with dimensions of volume per time, and $n(d_1)$ and $n(d_2)$ are the number concentrations of particles of sizes d_1 and d_2, respectively, with dimensions of volume $^{-1}$. The rate constants are given as follows:

Brownian Diffusion $$k_B = \frac{2 \cdot kT (d_1 + d_2)^2}{3 \mu d_1 d_2} \qquad [2a]$$

Laminar Shear $$k_{SH} = \frac{(d_1+d_2)^3}{6} G \qquad [2b]$$

Differential Settling $$k_S = \frac{\pi g(S-1)}{72 \upsilon}(d_1+d_2)^3(d_1-d_2) \qquad [2c]$$

Here k is Boltzmann's constant, T is the temperature (°K), υ is the kinematic viscosity, μ is the absolute viscosity, G is the velocity gradient (time^{-1}), S is the specific gravity of the solids, assumed to be the same for all particles, and g is the gravity acceleration.

These rate constants are compared for two cases of interest in water treatment in Figures 1 and 2. Conditions representative of a settling tank in winter and also for a lake hypolimnion in summer are presented in Figure 1. Calculations have been made for the collisions of particles having size d_2 ranging from 0.01 to 100μm with particles having size d_1 equal to 7μm. Values of T = 5°C, S = 1.02, and G = 0.1 s^{-1} have been assumed. A minimum collision rate occurs between particles having identical sizes, in this case for $d_1 = d_2 = 1$ μm. Stated another way, heterogeneous suspensions can flocculate faster than homogeneous ones. The extent of this effect depends on the size distribution of the particles, which is not known. Brownian diffusion is the fastest transport mechanism here for particles smaller than 1μm. It can be shown that the rate at which small particles can collide with each other (e.g., collisions between viruses of size about 0.02 μm) is significantly lower than the rate at which they can collide with larger particles by diffusion. Collisions by fluid shear do not become predominant for any size in this case. Differential settling becomes predominant for large particles. For example, particles of size equal to 1 μm will collide with 10 μm particles predominantly by this mechanism. It is plausible, then, that significant coagulation can occur in unstirred systems such as settling tanks if the suspension is heterogeneous in size and if the particles are destabilized sufficiently to permit aggregation when contacts occur.

Collision rate coefficients for flocculation tanks in summer are presented in Figure 2. Calculations are made for the collisions of particles of size d_2 from 0.01 to 100 μm with particles having a size d_1 equal to 10μm. Values of T = 20°C, S = 1.02, and G = 10 s^{-1} have been assumed. A minimum in contact efficiency again exists. In this case it is quite broad. Brownian diffusion is significant only for particles smaller than 0.01 μm. Collisions by fluid shear predominate over a wide range of particle sizes, i.e., 0.01 μm$<d_2<$100 μm. This indicates that flocculation tanks can be effective in aggregating very small particles by fluid shear if the suspension is heterogeneous and contains larger particles. This enhancement of flocculation rates by heterogeneity has not been recognized sufficiently. Previous work has emphasized that homogeneous suspensions of submicron particles cannot be flocculated by fluid shear. In water treatment, suspension of small particles can be made more heterogeneous by adding clays or alum flocs.

This simplified analysis of a two particle system should be expanded to consider complete particle size distribution as found in water supplies. The changes in particle size accomplished by flocculation and the removal of pollutants associated with these particles can then be considered both conceptually and experimentally.

COAGULANTS AND COAGULATION

Aluminum and iron(III) salts are chemicals commonly added to water supplies in order to destabilize solid particles and thus promote coagulation and filtration. These chemicals can destabilize suspended particles by forming polymeric hydroxometal complexes that are adsorbed on solid surfaces, and also by forming metal hydroxide precipitates that can act as targets for collisions in the flocculation of dilute suspension. These chemicals have been used for many years and in many places. However, we really know very little about what is actually added to a water, what reactions occur, and what the results are.

Consider stock solutions of alum, prepared prior to its introduction into a water supply. Alum can be purchased in dry or liquid form, and may be prepared in a variety of strengths for use in treatment plants. It has been observed by some that the strength of a stock alum solution can affect the results obtained when the chemical is mixed with the water to be coagulated. This is because the actual chemical species

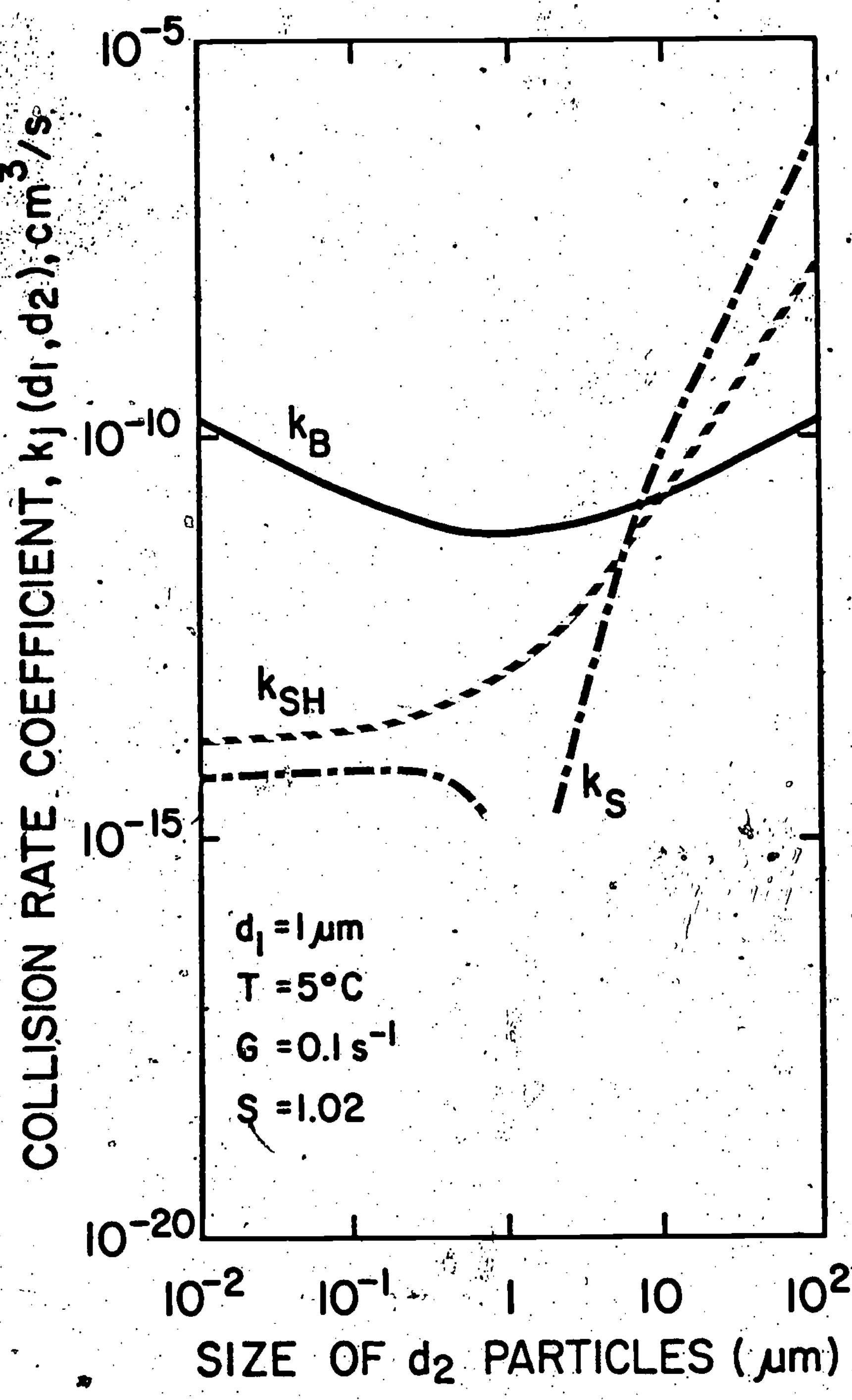

Figure 1. Comparison of Flocculation Rate Coefficients in a Settling Tank.

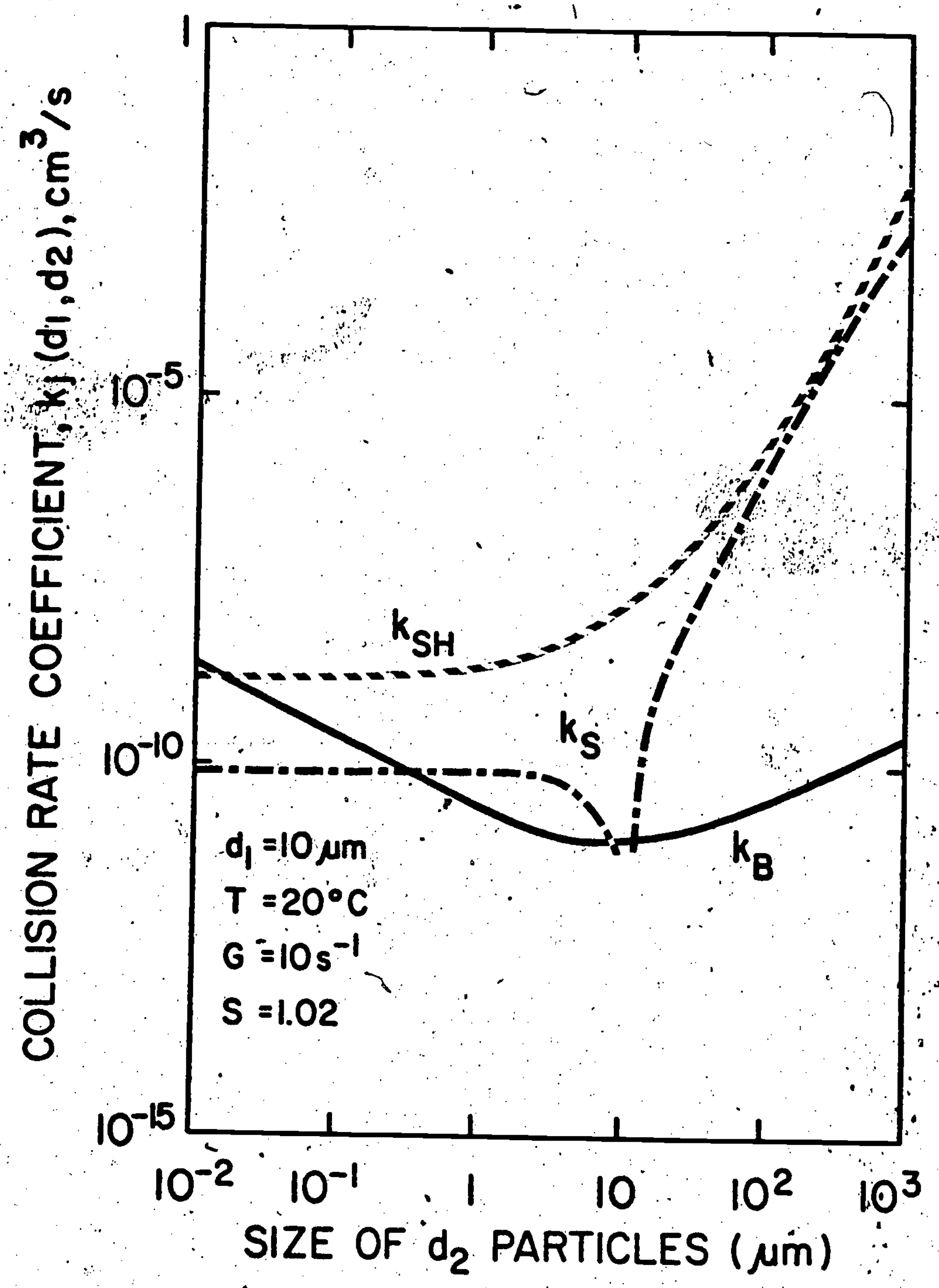

Figure 2. Comparison of Flocculation Rate Coefficients in a Flocculation Tank.

present in a stock solution depend on its strength. This is illustrated in Figure 3. These results are calculations based on the addition of pure alum to pure water. Species composition and solution pH are plotted as functions of pAl_T ($-\log[Al_T]$), where $[Al_T]$ is the total aluminum concentration in moles/liter. When $[Al_T]$ is 1 mole/liter, pAl_T is 0 and the corresponding alum concentration would be 333 g/l as $Al_2(SO_4)_3 \cdot 18H_2O$.

For very dilute alum solutions (e.g., pAl_T of 4 or larger), the principal soluble species are hydroxo complexes such as $AlOH^{2+}$. These solutions, corresponding to 33 mg/l or less of alum, are too dilute for use as stock solutions. In concentrated systems (e.g., pAl_T of 0.5 or smaller), the principal soluble aluminum species are sulfato complexes such as $AlSO_4^{+}$. In the intermediate range ($0.5 < pAl_T < 4$), the simple aquo complex, Al^{3+} or $Al(H_2O)_6^{3+}$, predominates. At $pAl_T \approx 2.5$, the fraction of the aluminum species that exists as Al^{3+} is a maximum. This also corresponds to a stock solution of 1 mg alum/cm^3, a concentration frequently used for stock solutions in jar tests.

Some liquid alum solutions may contain excess sulfuric acid, have a lower pH, and contain more sulfato complexes than the solutions described in Figure 3. The addition of dry alum to raw water containing alkalinity can produce a higher pH and more hydroxo complexes. It is plausible that "polyaluminum chloride," a liquid aluminum coagulant solution apparently developed in Japan and used in Europe, may contain stable hydroxoaluminum polymers produced in concentrated stock solutions because chloride is a weaker ligand than sulfate. It is not yet possible to determine the effects of such speciation on coagulation. It is only possible to say that the reactions of alum with substances in water and with water itself may depend on the actual aluminum species added, and hence on the type and strength of the stock solution. The "best" stock solution is still a matter for additional conceptual and experimental research.

Aluminum and iron(III) salts can react with many substances in water, including water itself, colloidal surfaces, humic substances, other soluble organics, and phosphate. Possible products would include soluble hydroxo metal species, metal hydroxide precipitates, destabilized colloids, metal-humic precipitates, soluble metal-organic complexes, and metal phosphate precipitates. The actual products formed will depend on the kinetics of these competitive parallel reactions, and hence on reactant concentration, pH, temperature, and reactor type. It is possible, for example, that the best reactor configuration to remove humics with alum may be different from the reactor type that promotes destabilization by alum in coagulation. The kinetics of the reactions of metal coagulants with substances in water, and the effects of reactor type on the performance resulting from these competitive reactions in real continuous flow systems are significant areas for scientific inquiries that are conceptually demanding and have practical utility.

PARTICLES AND FILTRATION

Existing theory and experiment indicate that suspended particle size has profound effects on the performance of clean packed bed water filters. Three observations are noted here. First, the removal efficiency of a clean filter depends on the size of the particles being filtered. A critical particle size exists in the region of one or a few microns; particles of this size have the lowest opportunity for contact with the filter media and subsequent removal from suspension. Smaller particles are effectively transported by Brownian diffusion and larger ones by interception and settling. Second, small particles have large surface areas per unit mass and require large dosages of destabilizing chemicals to provide attachment within the filter bed. Third, the removal efficiency of a clean filter is independent of particle concentration.

Clean filter theories become inadequate as soon as a filter run begins. Theories for ripening filters are being developed (2,3). Theory and experiment indicate that suspended particle size and concentration have additional important effects. First, for a given mass of particles removed, head loss varies in an inverse manner with suspended particle size. Submicron particles can produce enormous head losses when treated by conventional packed bed filters.[4] An example of this effect is given by the data presented in Figure 4. Note that minimum removal efficiency occurs for particles of 1 μm size, while head loss development is rapid for 0.1 μm particles. Second, removal efficiency improves after the start of a run. Dirty filters can be more efficient than clean ones. This is because particles retained within a packed bed filter during the initial stages of a run serve as the filter media during the later stages of filtration. In other words, the particles applied to a filter act as the filter media in a ripened filter. The size and chemical properties of these particles, and hence the actual filter media, are determined by the water source and the pretreatment used. Third, removal efficiency at a given time increases with increasing particle concentration in a ripening filter. This is because new media are collected more quickly, so that ripening is accelerated.

Research on theories of particle removal of real filters treating real heterogeneous suspensions is needed. The role of pretreatment in filtration should be explored, with particular emphasis on the physical aspects of flocculation and settling. This research should be both conceptual and experimental.

MEASURING PLANT PERFORMANCE

Turbidity is a measure of the optical properties of a suspension. Measurements of turbidity provide rapid, inexpensive, "in situ" indicators of plant performance and are valuable for monitoring and operating water plants. However, the test is beset by a wide variety of definitions, instruments, standards, and units of measure. Comparisons between water plants cannot be quantitative. In addition,

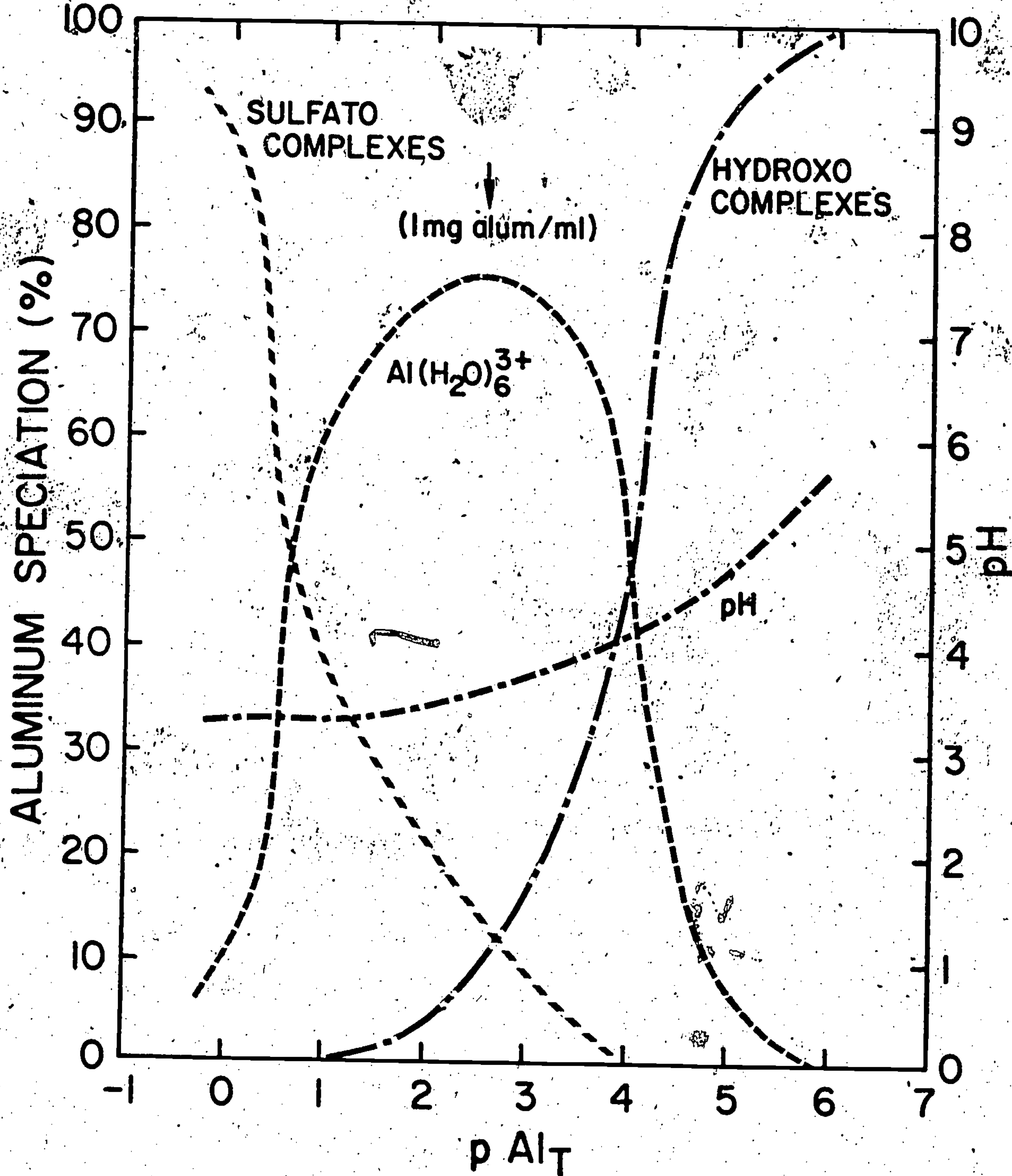

Figure 3. Calculated Speciation in Alum Stock Solutions.

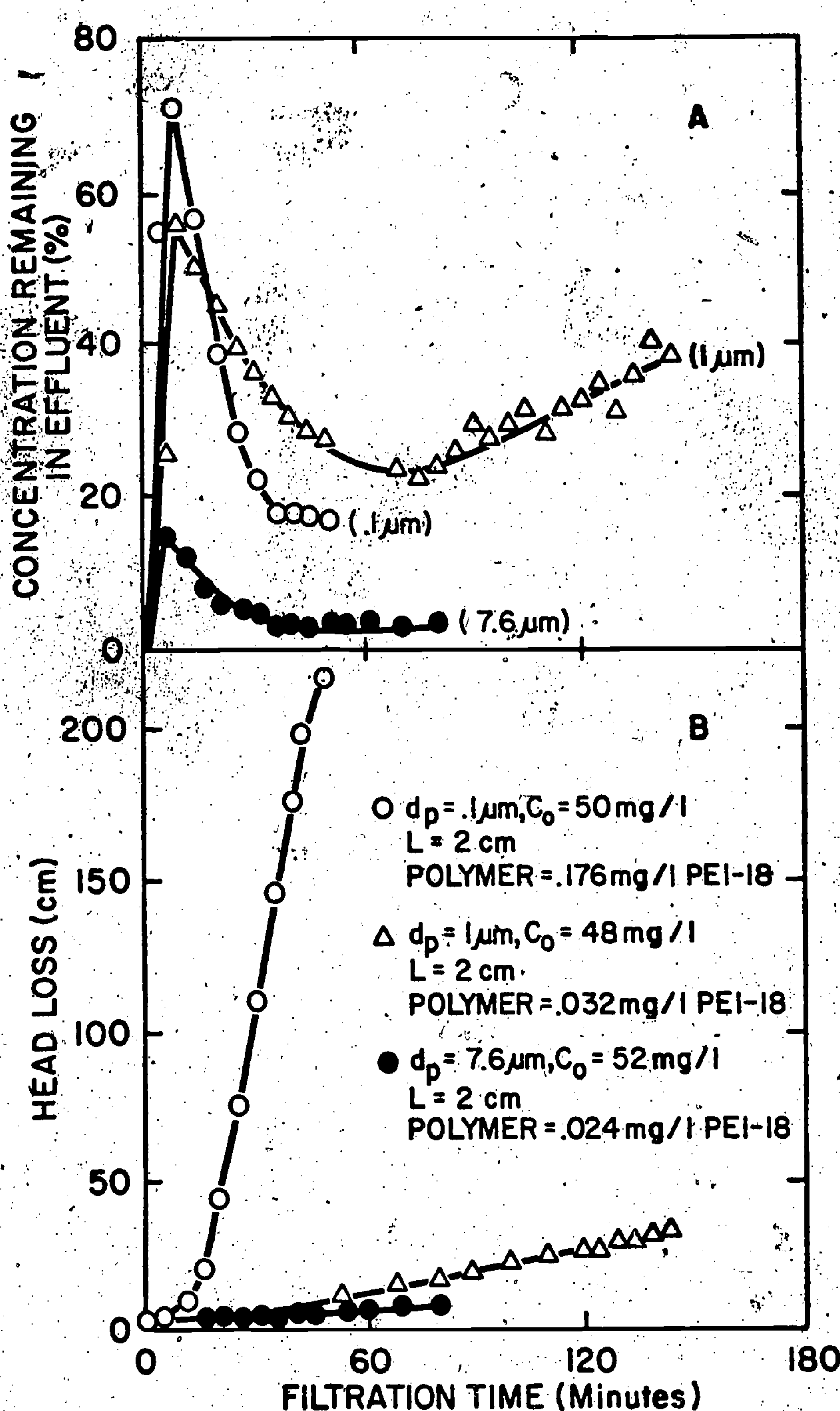

Figure 4. Effects of Suspended Particle Size on Filter Performance.

turbidity measurements do not give sufficient information about the size, number, mass, type, or chemical composition of the particles in a water.

The turbidity test is in need of standardization. Of greater significance, specific tests for the physical, chemical, and biological properties of particulates in raw and finished water are needed if plant performance is to be evaluated in a meaningful way. Such data will be valuable for evaluating the public health significance of substances in a water supply.

CONCLUDING REMARKS

There are many other aspects of water supply and treatment which require research. Epidemiological studies of infections from human enteric viruses in waters that meet current bacterial standards are fundamental, difficult, expensive, and necessary. Studies of the optimal design of new facilities and the optimal management of existing facilities are needed. Important and challenging areas for research abound. This essay has concentrated on one important research area, particles in water, as an illustration of these many needs for fundamental research in water treatment.

The particle problem may be summarized as follows. Most pollutants are solid particles or associated with solid particles. For examples, the emerging problem of asbestos fibers in water involves solid-liquid separation. Even the chloroform question can be considered as involving the removal of humic substances which are precursors for chloroform production. These humic substances are colloidal particles or may be adsorbed on particles.

Despite the scope and significance of the problem, we do not know the important physical, chemical, and biological properties of the particles in raw and treated water. We do not know the chemical species present in stock solutions of common coagulants, or the species formed when they are added to water. Design involves control of reaction rates, yet we do not know the chemical kinetics involved in the competitive reactions that occur when coagulents are used. The physical kinetics in the flocculation of polydisperse systems are important and unknown. Solid-liquid separation must depend on physical particle properties, but these are not considered in design. This serious deficiency arises from inadequate data and inadequate theory. Stated another way, fundamental research on particles in water treatment is needed.

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DISCUSSION

Donald B. Aulenbach (Rensselaer Polytechnic Institute):

It was stated that some materials in water may be desirable, yet it is proposed to develop techniques to remove everything from water. It seems to me that we are going overboard in water research if we propose to remove everything from water - even the desirable constituents. I feel that spending research money on methods for removal of desirable constituents is a waste of money. In addition, it must be stated that there are many sources for pollutant entry into water. One of the major sources is the atmosphere. It is very difficult to separate water from contact with the atmosphere. Therefore, one of the best ways to prevent pollution of water from the atmosphere is to prevent the entry of these pollutants into the atmosphere. It must be recalled, however, that many of the materials in the atmosphere exist naturally and, therefore, naturally gain access into the water. Since we plan to have methods for removal of everything from water we must be prepared to remove such natural constituents from the water. We must remember, too, that even after we remove all the constituents from water we still have to breathe the air with all its constitutents including the natural ones. Furthermore, today there are many other environmental antagonisms which are a greater hazard to the health and safety of mankind and for which we see fit to do nothing to control. For example, we know that smoking of cigarettes is harmful and causes cancer, yet people will continue to smoke. We know that the automobile kills 40,000 persons per-year, yet we are not willing to give up our automobile in terms of saving lives. There are many more deaths than are caused by constituents in our drinking water. If man puts priorities on his needs, when he feels that water is the most important need he will put more effort into doing research in water pollution and in providing sufficient water treatment that there may be no question or doubt of the safety and purity of these waters for all consumers.

Philip H. Jones (University of Toronto):

In looking over the papers and hearing the presentations of the past day, I have been particularly impressed with the diversity of the group interest and, at the same time, with the specificity of the individual interests. This leads me to believe that as individuals we are peering through long narrow tubes at our basic research needs and that, if the sum total of the perspectives which we individually have makes up a composite picture of the environment, it is much more by luck than by judgment.

I came to the conclusion a long time ago that environmental engineers cannot be expert at the fundamental level of all the relevant sciences such as Physics, Chemistry, Virology, Biochemistry, Economics, etc. This must lead us to the conclusion that a team approach will be essential and that environmental engineers, if they are prepared to, can serve the very important role of orchestrators of basic research. If we conclude that basic research really lies in the sciences and some of the applied sciences, there obviously has to be a need for a manager to ensure that the basic research is geared to producing a clear and intelligible mosaic which can be recognized as the environment.

The institutional problems, not to mention the intellectual problems of transdisciplinary research, are indeed formidable. We collectively have the opportunity of bridging the disciplines and identifiying and coordinating the research effort serving as orchestrators. For this to be effective, we must reduce our efforts to copy pure scientists at work and rather profit from their pure scientific discovery by adding relevance to their research. It will take great self-confidence to back away from the pristine pure approach ourselves, relinquish it to others, and rather to try to influence those others to provide the answers we need.

E.H. Ted Curtis (US Department of the Interior) for Mr. Robert R. Peters:

What interactions have you made with your proposal on financing and guiding water related research with the National Water Policy Study, specifically in relation to the Task Force report on Water Resources Research? This report has recently been prepared and it is now appropriate to direct comments toward Mr. Guy Martin, Assistant Secretary for Land and Water Resources, Department of the Interior.

Peter O. Nelson (Oregon State University) for Dr. E. Robert Baumann:

I certainly believe that expertise should be developed by one person or a small group, and that many problems may require an interdisciplinary approach to solution. If a university develops a "center of excellence" based on expertise in an area, this should be a vehicle for seeking funding. However, my comment to Mr. Lacy of the EPA was whether funding would automatically go to "centers of excellence" and thus not be subject to the competitive peer review process. Peer review is a necessary check and balance on quality research and should not be circumvented in the funding of research.

A. Amirtharajah (Montana State University) for Dr. Charles O'Melia:

I wish to reinforce some of the remarks made by Dr. O'Melia. There is a general impression in the environmental engineering field that we have a reasonably complete understanding of most water treatment processes as well as adequate parameters for design. I believe that our knowledge is glaringly incomplete in almost all areas and we have very little fundamental understanding of the various processes. Most studies have been macroscopic and empirical in nature with very few that deal with the processes from a microscopic theoretical framework.

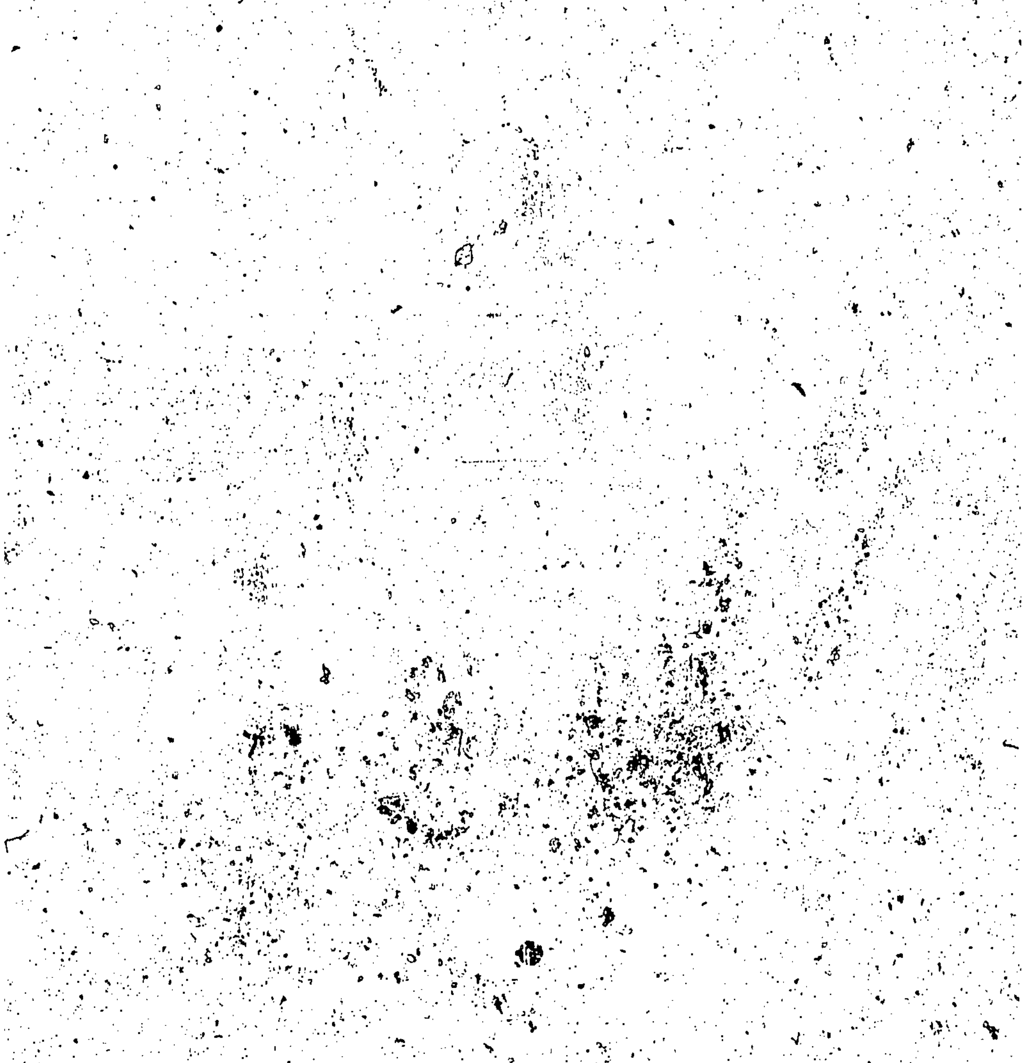
Let me illustrate with specific examples in each process area. In the rapid mix of a water treatment plant, an interaction takes place with chemical reactions and a turbulent fluid field which varies spatially. We have a partial understanding of the chemical kinetics, but almost no understanding of the interaction of the two systems. For a fundamental viewpoint, we need to marry the hydrodynamics of the fluid field with the chemical kinetics.

Water treatment plants commonly use sludge blanket type clarifiers for coagulation-flocculation. The design of the flocculation zone is painfully simplistic based on detention time and the parameter, Gt, partially derived from von Smoluchowski's theory of orthokinetic flocculation based on a laminar fluid field. The sludge blanket zone in a clarifier is a fluidized bed, and is very, very different from von Smoluchowski's theoretical system. Even in horizontal flow-through flocculation systems, it is only in recent times that attempts have been made to model reality from a microscopic viewpoint, with a turbulent fluid field and simultaneous floc formation and erosion.

Filtration is one area which has been probed from a fundamental as well as empirical approach for several hundred years. Even here we are unable to quantitatively and realistically model the performance of a filter, namely to predict its effluent quality and head loss characteristics. In addition, we commonly design declining rate filters on the basis of uniform loading and head loss characteristics like a constant rate filter. In backwashing, we know very little about air scour except that it is effective. Recent designs indicate that the filter design may in fact be controlled by its backwashing characteristics rather than its filtration effects, leading to coarse single media ($\approx$2mm) deep beds ($\approx$ 8 ft.).

In conclusion, I believe that we have only a limited knowledge of water treatment processes from a fundamental viewpoint. What we need are studies similar to Camp's lifetime work in this area: quantitative theoretical studies from a microscopic viewpoint which would lead to macroscopic design parameters.

FUNDAMENTAL RESEARCH NEEDS IN WASTEWATER TREATMENT



Conference on Fundamental Research Needs
Water and Wastewater Treatment Systems

FUNDAMENTAL RESEARCH NEEDS IN WASTEWATER TREATMENT FOR BIOLOGICAL PROCESSES

Perry L. McCarty
Stanford University

INTRODUCTION

Wastewater treatment processes are normally divided between biological, physical, and chemical processes. Several processes may be used together to form a wastewater treatment system that is capable of removing undesirable contaminants in the wastewater and to process the resulting sludge for final disposal. This paper concentrates primarily on the fundamental research needs for biological processes, since it is expected that needs for chemical and physical processes, for sludge disposal, and for integration of the overall system will be covered in papers by other participants at this conference. Several other recent reports which address research needs in these and related areas should also be noted (1,2,3,4).

Wastewater treatment system include not only the traditional wastewater treatment plants, but also soil systems employing overland flow, percolation, or subsurface injection of wastewaters. The latter systems offer potential beyond that available in the traditional treatment plant, and present some of the most challenging problems for fundamental research.

NEED FOR FUNDAMENTAL RESEARCH

The estimated budget authority for pollution control and abatement for the federal government in FY 1978, exclusive of construction grants, is $2.4 billion (5). Additional billions of dollars have been and will contine to be spent yearly for pollution control equipment by private and governmental organizations. Yet, the understanding of environmental problems is so limited that the appropriateness of much of these expenditures frequently has been challenged. There is little question that long-term fundamental research applied to the solution of practical environmental problems would be of major benefit to society by helping to reduce the cost for pollution abatement and to direct available funds for correction or prevention of the most serious environmental problems.

The U.S. Environmental Protection Agency's requested operating budget of $802 million is part of the above estimated federal budget and includes $266 million for research and development (5). Because of this significant budget it has been common to assume that EPA is a major supporter of fundamental environmental research. However, as pointed out by a recent National Academy of Sciences report (5), the EPA research budget must and for the most part is used to support the agency's primary mission which is regulation. The NAS report indicated that EPA should not be considered as a lead agency in fundamental research for environmental science and technology. Thus, even with the considerable federal expenditures in the environmental area, there is no federal agency offering significant support for fundamental and long-term research on environmental problems. The National Science Foundation would seem to be the logical federal agency to supply this need to the country.

A question which might be asked is: Can fundamental research solve practical problems? It certainly can when it is well conceived and properly focused. Two examples will be used for illustration. An important success of fundamental research was the elimination in the early 1960's of excessive foaming at wastewater treatment plants and in rivers and groundwaters where sanitary wastes were discharged. This problem resulted after World War II when synthetic detergents were introduced to the household market. While much empirical trial and error research was conducted on this problem, it was the fundamental research carried on within environmental engineering programs at M.I.T., the University of Wisconsin, the University of California, and elsewhere that provided the solution. This research was the first to reveal that industrially synthesized organic compounds may not yield to normal biodegradative forces, and thus was the first to challenge the long-held concept of microbial infallibility. The fundamental research showed that the presence of a quarternary carbon atom, an uncommon but not absent feature of natural organic molecules, together with frequent molecular branching made the ABS detergent molecule extremely resistant to microbial attack. This indicated that if the quaternary carbon was eliminated and the molecule was straightened, than an effective, but biodegradable detergent termed LAS should result. This clear elucidation of the fundamental molecular structure causing resistance led to a rapid changeover to LAS and elimination of this significant problem by the early 1960's, about fifteen years after it had first appeared.

The other example is of a major research effort which used an indirect and empirical approach, and largely for this reason, the problem is still unresolved after fifteen years. In the early 1960's it was noted at the San Antonio, Texas, treatment plant that phosphorus was almost completely removed through activated-sludge treatment, a phenomenon which offered potential for elimination of this pollutant without expensive chemical addition. Empirical observation indicated that an unusually high aeration rate was correlated with the phosphate removal and a national research effort was instigated to demonstrate at several treatment plants that this was a cure for the phosphorus problem. This attempt met with only partial success at some plants and none at others. A controversy began over whether the phenomenon was luxury uptake by microorganisms or chemical precipitation due to the high resulting pH, but insufficient funds have been provided to pursue the mechanism in the fundamental way it deserves. Some headway has been made, but the problem has not yet been resolved. As a result, few treatment plants are designed to take advantage of this potential cost-saving phenomenon. Engineers responsible for the design of treatment plants costing millions of dollars cannot afford to gamble on designs which are based upon other than established scientific principle as there is too high a probability that they will fail.

The above and similar examples illustrate that a good fundamental understanding of a process is necessary before confidence in its use can be gained by the industrial and engineering community. In the following, various areas for fundamental research which should contribute to our understanding of biological processes and lead to broader applications to meet future needs is presented.

RESEARCH AREAS IN BIOLOGICAL WASTEWATER TREATMENT

Because of their continuing or growing importance, the following four aspects of biological wastewater treatment should be subject to long-term fundamental research: (1) microbial ecology of treatment systems, (2) kinetics of biological processes, (3) persistent organics, and (4) innovative processes. There is overlap between these areas as indicated in the following.

Microbial Ecology of Treatment Systems

Billions of dollars are being spent for municipal biological treatment plants each year. This capital investment comes largely through federal support, but the operating costs, which will be of equal magnitude, will be borne by local municipalities or by industry. Experience has indicated that we should anticipate reliable operation of these plants will be sporadic and that failures to meet effluent requirements will be frequent. An important fundamental research need is to develop a better understanding of causes for treatment plant failures, and to develop methods for their systematic evaluation and control. Failures may result from inadequate design, inappropriate operation, or from abiotic characteristics of the wastewater being treated. It can be exceedingly difficult to judge the cause of an improperly operating system, mainly because of a lack of a fundamental understanding of the way in which mixed microbial communities respond to different environments. The need then is for a better fundamental understanding of the microbial ecology of wastewater treatment systems, particularly as this affects operation and reliability.

Biological treatment systems represent mixed cultures of high complexity. Microbial ecology is concerned with interrelationships between microorganisms and their environment. Under a given set of conditions, shifts in microbial population can have significant effects upon effluent quality. Such changeovers may be caused by purely random forces, or they may result from changes in influent wastewater characteristics such as temperature, the concentration of a required nutrient, or the presence of an inhibitory material, or they may result from the particular treatment plant design or operation. While many factors can result in an undesirable shift in microbial population, the introduction of inhibitory materials is commonly blamed. A better understanding of the microbial ecology of treatment systems would help differentiate between potential causes of biological treatment plant failures.

One example of need in this area is the effect of oxygen tension on the characteristics of the microbial community in activated-sludge systems. Claims for pure oxygen systems which result in higher dissolved oxygen concentrations are that the biological flocs are more compact, settle better, and result in less excess biological sludge production than air systems. Experience with the reliability of such systems has been variable. Lack of adequate fundamental research in this area has left many of the important questions unanswered, and has left engineers in a quandary over the real merits of air versus pure oxygen systems.

The above is perhaps a subset of a broader and continuing problem which requires more funadmental understanding of microbial ecology. This is "bulking" in activated-sludge systems. Bulking results from a change in bacterial predominance leading to a light and poorly settling sludge, an inability to retain the microorganisms in the treatment system, and their subsequent discharge to the effluent, producing high effluent suspended solids and BOD. Bulking has no doubt been the major operational problem with activated-sludge systems since the process was developed over fifty years ago, and it appears it will continue to be one of the major problems in the future. Good fundamental research has been conducted in this area and the causative organisms are known. However, there is insufficient understanding of the relationship between these organisms and others in the treatment system as affected by environmental factors.

Factors which lead to a change in predominance from a desirable microflora to a bulking one are poorly understood and based largely upon empirical evidence. Control is largely through trial and error using such procedures as the addition of toxic substances, like chlorine or hydrogen peroxide to reduce the population of bulking organisms in the hope that they will not return to dominance, the addition of more oxygen, or sometimes less, with the observation that at times these measures lead to desirable population shifts. Evidence at times indicates bulking is caused by toxic substances of undefined character, the introduction of unusual organics, lack of inorganic nutrients, such as iron or nitrogen, or an excessive carbohydrate fraction in the organic makeup of the waste. Change from one modification of the activated-sludge process to another, or operation at different solids retention times have also been judged at times to relieve a bulking problem. Observations of bulking in some laboratory systems but not others when operation is identical suggests that random population shifts are also likely to be a cause. Continued, but more intense fundamental study on the ecology of mixed cultures directed towards an understanding of factors which cause shifts toward predominance by bulking organisms and methods for their control should lead to a reduction in this significant problem. This has been and will continue to be a long-term research problem.

Another important problem in biological treatment systems which can be addressed through a better understanding of microbial ecology is the relationship between growth rate and effluent quality. The major parameter which the operator used to control organism growth rate is the solids retention time (SRT) or sludge age. By "tuning" the SRT, he can avoid the dispersed growth occurring at low SRT or the "biological residue", which results at long SRT, both of which lead to high effluent suspended solids and BOD. Because of the importance of this control parameter, and the variability of results which it provides, a more fundamental understanding of the relationships between microbiological populations and growth rate is desirable. How does growth rate under steady-state operation affect organism predominance, the ability of organisms to agglomerate and settle, and the biological end products from metabolism or decay? How does variability of flow rate and waste loading affect these properties? These are all questions which can be addressed through investigations of microbial ecology.

The National Science Foundation is now supporting fundamental studies on the microbial ecology of methane fermentation. This is already leading to conclusions, which are of importance not only to wastewater treatment, but also to the production of energy in the form of methane gas from a variety of organic residues from industry, municipalities, and agriculture, and from energy farms and coal. These studies are helping to understand natural decomposition of organics in the environment and are even providing new clues to the origin of life. Methane fermentation offers one of the best procedures for recovery of a valuable by-product from waste decomposition, and rather than requiring energy, it produces energy in the process.

In the wastewater treatment field, methane fermentation has frequently been criticized because of lack of reliability. However, it is evident from experience in England, that a national effort to gain an understanding to this process and to establish a protocol for evaluating the causes of failure of anaerobic treatment systems can meet with enormous success. Continuing emphasis toward understanding the microbial ecology of methane fermentation, with emphasis on the nature and effect of inhibitory materials, and methods for monitoring their presence and control, is highly desirable.

A more fundamental understanding of the effect of inhibitory materials on microorganisms in wastewater treatment systems in general is of importance. Gross inhibition can usually be recognized and eliminated. Of more importance for long-term fundamental research is the more subtle effects from sublethal concentrations of toxicants, which may cause changes in organism predominance and resulting decreases in effluent quality. They may cause changes in the ability to remove certain trace organics of toxicological significance, even while effecting adequate BOD removal. A more fundamental understanding of the effects of inhibitory materials can thus lead to better evaluation and control of biological wastewater treatment plants.

In summary, the sustenance of a well-operating biological wastewater treatment plant is dependent upon the ability to maintain the proper microbiological community. A more fundamental understanding of microbial ecology of wastewater treatment systems is essential for understanding the needs of desirable populations and how undesirable communities develop, and to develop adequate monitoring and control procedures so that the microorganisms desired can be maintained. It is essential, however, that research in microbial ecology be clearly focused toward solution of these significant problems.

Kinetics of Biological Processes

A better fundamental understanding of biological wastewater treatment systems is slowly evolving. Our present understanding has been quite adequate in general to evaluate the major treatment parameters for the design and control of biological treatment systems. This is especially true of suspended growth systems. The kinetics of microbial fixed-film systems is more complex to describe, and is becoming better understood. Nevertheless, it is essential to continue fundamental research here because of the introduction of new and promising fixed-film processes including rotating biological media, submerged reactors, and fluidized biological beds. In addition, it has become apparent that the fixed-film biological processes are of great importance in the removal of contaminants by activated-carbon adsorption.

Also of growing importance are soil systems, involving either spreading of wastewaters onto or injection into the ground. Observations of such systems to date are largely empirical and indicate that soil systems can be quite effective in removal of materials as measured by traditional parameters such as BOD. Empirical observations also indicate that aerobic conditions, as enhanced by "resting" the systems between periods of spreading is beneficial, at least in preventing undue clogging. Whether or not this otherwise increases the ability to biologically remove organics is unknown. The kinetics of such processes, from a fundamental standpoint, remains largely an unknown. Can the emerging understanding of fixed-film processes be used to describe biological decomposition of contaminants by soil systems, or must other factors be considered? How is the ecology of such systems related to that of other biological processes? Do fungi provide a significant role in these processes, and if so, what additional capabilities or difficulties do they present? An important but related topic concerned with decomposition of persistant chemicals at low concentration is discussed in the next section. The ever increasing emphasis on use of soil systems, their potential economic and technical advantages, and potential for adverse impacts from groundwater contamination, suggest the need for much more concentrated and fundamental research program both on the kinetics and the microbial ecology.

Toxic and Persistent Organics

Persistent organics refer to materials which are difficult to degrade biologically. Physical and chemical processes are generally proposed as an alternate means of removing such materials and so the need for fundamental studies of biological processes in this area might be questioned. However, there are aspects of certain persistent organics which are important for investigation from a biological point of view. For one, it is now generally acknowledged that most persistent organics which are present in wastewaters and in most natural aquatic systems are produced naturally through biological processes and are not anthropogenic. While there is generally little fear that these materials are of toxicological significance, they do present problems. It has long been recognized that they impart tastes and odors to waters. In addition, some react with chlorine to produce halogenated organics which are of concern because of presence in drinking waters. In addition, their relative abundance in natural waters and wastewaters tends to mask the presence of industrial organics for which there is a major health concern. For these reasons, a better understanding of the nature of biologically produced persistent organics is desirable. Although controversial, there are indications that as much as 40 percent of the dissolved organics present in the effluent from biological treatment of municipal wastes is produced biologically during treatment. The characteristics making this naturally produced material so refractory is largely unknown, as is its general composition and molecular structure. This material is generally thought not to be of toxicological significance, but since this has not been studied, this also is not known with certainty. Thus, fundamental studies to evaluate the source, composition, molecular structure, and toxicological properties are most desirable. Knowledge of the relationship between molecular structure and persistence would also be most beneficial.

Reasons for persistence of anthropogenic organics should also be studied in a fundamental way. This would permit evaluating whether proposed chemicals could be treated by normal biological processes and by soil systems, or decomposed when added to the environment. Organics which cannot be decomposed biologically and which do not adsorb well in soil systems can result in contamination of groundwater supplies for generations if present in wastewaters applied to the land. This may even be a problem with chemicals which are normally biodegradable, but because of their low concentration in wastewaters, they may not support biological activitiy.

Thus, another area of fundamental research need is on the kinetics of degradation of organics at low concentrations. Current theory of bacterial kinetics suggests that when an organic exists in concentrations below the fractional mg/l level, the concentration is not sufficient to permit growth of microorganisms, i.e., organisms' decay rate exceeds the rate of growth. This suggests that some group of "persistent" organics found in natural environments and treatment plant discharges simply may be a collection of normally biodegradable organics each of which is too low in concentration to support bacterial growth. In support of this, low concentrations of biodegradable substances are indeed found in natural systems and treatment plant effluents.

However, other observations indicate that decomposition of organics can occur down to levels as low as the $\mu g/l$ level. This may be explained as one form of co-metabolism in which an organism is supported by one organic that is present in relatively high concentration, making it possible to degrade another organic which is present in low concentration. A better fundamental understanding of the kinetics of microbial decomposition of organics when present in low concentrations is most important. This has application to normal treatment plant operation, to natural waters, and especially to treatment of wastewaters in soil systems. Research in this area seems especially important now with the greatly increased need for control of specific organics, instead of just gross organics as measured by BOD, COD, or TOC. The increased emphasis on wastewater reuse especially by land-spreading and groundwater injection calls for an immediate start on long-term fundamental reseach program in this area.

Innovative Processes

Support for fundamental studies of innovative treatment processes should always be available. The need is not for the immediate support of a special group of proposed concepts, but to have funds available for well-conceived ideas which evolve and offer potential breakthroughs in treatment technology. Often sufficient support is required to test a concept which has some demonstrated promise, but which needs additional evaluation for technical and economic feasibility. Frequently, one new concept explored in some depth, may not prove to be feasible, but will lead to other concepts which do. Most biological systems presently used evolved in this way, and there is a real need for some level of support to generate newer concepts for the future. Each experimenter no doubt has his own ideas as to which lines of investigation offer the most promise. A modest level of support in this area will stimulate the generation of proposals for study of new concepts, and the most promising ones can be selected by peer review. Financial assistance to study a broad range of innovative concepts is desirable in order to sort out the one or two concepts which will revolutionize the wastewater treatment field from the many ideas which have the potential.

SUMMARY

Four areas of fundamental research needs for biological processes of wastewater treament have been presented. In each of these areas the need is for long-term, methodical research aimed at providing fundamental information with wide application to a variety of problems. Such research is needed to generate new ideas and concepts. It is also needed to give an understanding of processes so that they can be designed with more confidence and operated with more potential for success. This is a major advantage of the fundamental research approach to the solution of applied problems.

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FUNDAMENTAL RESEARCH NEEDS FOR WASTEWATER TREATMENT: PHYSICOCHEMICAL PROCESSES

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INTRODUCTION

The title for this panel session—indeed, that for the Conference—might leave question as to whether we address fundamental research, or fundamental needs. Any notion of inherent ambiguity in the title(s) should be put aside. For wastewater treatment, as indeed for all applied technologies, what is fundamental by way of need defines what must be fundamental by way of research.

Two fundamental needs of man are safe water to support life and conservation of those resources which maintain the infrastructure of his activities; these needs intrinsically define the direction and scope of our research.

The realities and consequences of diminishing resources in the face of expanding demand have made it increasingly necessary for man to recycle a variety of resources, including water. Other non-recycleable resources, such as conventional energy, must be efficiently cascaded and conserved in the course of use. Several aspects of these considerations take on colinear perspective in wastewater treatment, while others are diametric. Our research vista must focus on development and refinement of treatment systems which colinearize as many facets as possible of our needs and our practices.

Environmental issues of the past two decades have markedly changed the emphasis of wastewater treatment. No longer is it sufficient to remove BOD to protect the oxygen resource of a receiving water. Levels of environmental sensitivity and conern have elevated (1,2); so too has the complexity of residue emanating as waste from an increasingly sophisticated industrial technology responding to broadened consumer demand.

Moreover, the primary function of many so-called receiving waters has evolved in concept and reality from waste transport, to recreational resource, to water supply. The eventual link between water and wastewater has been abridged—spatially and temporally—by a combination of circumstances. These include the increasingly contiguous location of communities and industries along water courses, the escalation of social water requirements, and the persistence and ubiquity of an increasing number of contaminants of relatively recent origin and/or detectability.

It may be considered gauche to quote oneself, but, since I have been asked to give my opinions on research needs, I would reflect upon a position I took a number of years ago and staunchly maintain(3):

> "—the increasing frequency with which the normal hydrologic cycle of water is short-circuited makes superficial boundaries between natural waters, water supplies, and wastewaters more artificial than they have been". . . "Engineers and scientists must address themselves to the reality of a continuum of water quality and to the development and implementation of measures for transforming water of any quality along the continuum to any other quality required for a particular use."

Water recycle and reuse, *defacto* and *dejure*, is a reality. In developing wastewater treatment technologies—and in the research related thereto—we must be aware that we are the ultimate consumers of our product. We must be aware too that, in the final analysis, even those of us in wastewater treatment are eventually and ultimately concerned *cum aquatio*. Our fundamental research must therefore be designed to provide answers for how best to secure the water.

RESEARCH NEEDS

In the context of the foregoing, I advance several research areas of particular significance; all relate to a central concept of reclamation systems to permit and facilitate expanded reuse of water. The order of presentation does not imply order of significance; each must, I feel, be addressed with maximum vigor and acuity. In simple statement, these research areas are:

1. treatment systems for specific control of pollution by toxic, carcinogenic, teratogenic, and mutagenic substances;
2. disinfection and chemical oxidation treatments, with particular reference to potentially adverse implications;

3. integrated bio-physicochemical processes to accomplish higher levels of treatment than conventional biological systems at levels of energy and cost utilization below those associated with conventional physicochemical systems;

4. multiple-purpose integrated systems which combine several objectives of water, air, and solid-waste pollution control; and,

5. treatment systems which can, in addition to their primary function of water purification and recovery, provide for secondary resource recovery.

I will not attempt here to elaborate each of these research needs to the degree and depth they warrant; neither time nor space permit, nor, indeed, does the awareness of the imaginative researcher require it. Nonetheless, it is appropriate that some conspectus and exemplification be given.

Specific Pollutant Control

In consideration of the reality of water reuse, it is essential that our research evolve technologies that will ensure specific removal of potentially toxic, carcinogenic, teratogenic, and mutagenic substances in water recycle and/or discharge systems. National concern for the rational control of such substances in the environment is manifest by the Toxic Substances Control Act of 1976 (P.L. 94-469)(4). The development of elaborate industrial societies, particularly in the U.S. and in Western Europe, has led to proliferation of a vast number and variety of complex chemicals for industrial, agricultural and domestic use. The character of many of these chemicals is such that they can have insidious and profound effects—either direct or synergic, either short term or long term—on man and his environment in uncontrolled exposure situations (5-9). Such compounds eventually find their way into municipal and industrial wastewaters, and, unless specifically removed by waste treatment processes, ultimately appear in receiving waters and water supplies (10-14).

We know, generally, the qualitative character of treatments that will address this issue. For the most part, they are physicochemical separation and/or transformation processes, such as adsorption on activated carbon, membrane separation, or oxidative transformations. Our research must focus on characterization and quantification of the specific reaction mechanisms and micro-transport dynamics associated with such processes in these applications, and on derivative evolution of rational design bases.

One might ask, from an applications standpoint, why is it necessary to elucidate fundamental reaction mechanisms and microtransport processes. This is an issue I will subsequently address in more detail and in broader context. For the present, suffice it to say that wastewater treatment applications of most separation and transformation processes are sufficiently different—in both objectives and dynamics—to dictate against rote adoption of research data and information developed from other applications on similar processes. Case in point; adsorption by activated carbon when addressed to isolation of a specific organic compound present in high concentration in a chemical process stream can, and likely will, involve entirely different mechanisms and dynamics than when addressed to the removal of extremely small concentrations of that compound from an aqueous solution containing a background of other organic substances of similar and dissimilar character. Adsorptive selectivity, competitive interactions, and potential chromatographic elution or displacement become matters of substantial import with respect to process feasibility in the latter case. In like manner, processes such as oxidative conversion frequently defy conventional thermodynamics and kinetics in the dilute and complex mix represented by most wastewaters. Such factors as interferences and formation of kinetic intermediates frequently control process feasibility, and dictate specific pretreatments and/or process modifications, the nature of which can be elucidated only by fundamental research.

Disinfection and Oxidative Treatments

Although some arguments to the contrary can be advanced, it is likely that the benefits associated with the disinfection of wastewaters for destruction of pathogens and inactivation of enteric viruses will insure continuation of this practice. There is no question of this in reclamation and recycle systems, and in situations where *defacto* reuse of water is a possibility. Nonetheless, we must be cognizant that disinfection of wastewater—at least as commonly practiced—is not without potentially adverse side effects. If this be questioned for one minute, consider the findings of the National Organic Reconnaissance Survey(14) of the EPA and other similar reports of the effects of the chlorination of drinking waters (10,12,15-17). Reference is made to the formation of halo-organic compounds having much the same potential with respect to toxicity, carcinogenesis, teratogenesis, and mutagenesis as those materials referred to in the preceding category of research need. If this is indeed the case for raw water supplies, there can be little doubt of the formation such compounds in wastewaters, which—at least in early stages of treatment—contain higher concentrations of a greater variety of organic compounds which can function as halo-organic precursors.

One might suggest that our research must seek out some disinfectant other than chlorine which will not yield potentially harmful end products. Yet we know that many other materials which might logically serve as disinfectants, such as ozone, also react with organic substances to form partial-oxidation products and kinetic intermediates of reasonable longevity (18). The depth of our knowledge with respect to such reactions is scant for disinfectants other than chlorine; this alone suggests definite need for fundamental research on the potential adverse impacts of likely alternatives to chlorine.

It is conceivable that there are chemicals or methods for disinfection which can function without adverse side effects and which, at the same time, will be technically and economically feasible; conceivable, but unlikely. This becomes evident when we realize that the very properties which render a chemical and/or chemical-generating method (as are most irradiation and electro-chemical techniques) effective for disinfection also render it effective for partial or substitutive oxidation and transformation of organic compounds. Indeed, this reality makes it necessary for us to include, as I have in this category of research need, concern regarding the potential adverse implications of chemical oxidation treatments for wastewaters.

It is far more likely that there are methodologies by which we can apply conventional disinfectants and oxidants to ensure their primary functions, but at the same time eliminate or minimize adverse side effects. This should be the primary thrust of our research in this area.

Such methodologies in some applications may take the form of variations in the sequencing of processes. For example, it may be unwise to prechlorinate a wastewater—as is frequent practice—but rather to disinfect only after other treatments have substantially eliminated or reduced the most likely precursors of halo-organic side products. In this regard, fundamental research must focus on defining which of the common constituents of wastewaters are most susceptable to substitutive or partial oxidations by conventional disinfectants, and what the end products are. Information of this type can form the basis for choice of disinfectants and sequencing of disinfection operations for specific types of waste treatment applicaions.

Conversely, it may be the logical conclusion of such research that—at least for certain applications—additional or modified treatments are required prior to disinfection for precursor removal, or following disinfection for removal or destruction of potentially harmful end products.

Again, what is said with respect to disinfection must be extended also to chemical and irradiative oxidation processes. The latter treatments can be attractive for many industrial and municipal situations for which other treatments are ineffective or more costly. However, only detailed knowledge of the reaction dynamics associated with such processes can identify potential adverse implications, and form the basis for implementation of measures to avoid adverse effects. Such detailed knowledge can evolve only from fundamental research.

Integrated Bio-Physicochemical Systems

Truly integrated—not simply additive—bio-physicochemical treatment systems intrigue me, and I believe, constitute one of the most challenging and potentially rewarding areas of research endeavor in the wastewater treatment/water reclamation field.

Biological treatment processes alone cannot meet water quality requirements for most reuse applications; indeed, they are frequently incapable of meeting higher levels of treatment required for discharge. Quite simply, biological processes, even when designed and operated under optimum conditions, can remove only biologically degradable and otherwise bacterially-incorporable pollutants. Such materials are certainly important, but just as certainly do not constitute the only, or necessarily major, concern for wastewater treatment today. Physicochemical processes, such as adsorption and membrane separation, can be designed to meet reuse and/or stringent discharge requirements, but are energy and operating cost intensive. These considerations, while important for municipal applications, are of particular significance for industrial wastewater reclamation and reuse.

Energy use and operating costs are generally proportional to waste load, both in intensive or concentration aspects and in extensive or mass-loading aspects. Current wisdom dictates the use of biological and physicochemical processes in series. Biological treatment, with lower energy requirements and operating costs, can minimize the waste load applied to subsequent physicochemical treatment(s) by removing that fraction of the load which is susceptible to bio-oxidation. However, the use of such treatment systems in series entails capital cost, material requirements and land area nearly double those of conventional biological treatment systems.

Adsorption on activated carbon—to select one widely applicable example of physicochemical treatment—can by itself be a cost effective process for many reclamation applications. Carbon is particularly effective for removing trace amounts of halogenated organic compounds and other materials that have been previously indicated as of substantial environmental concern. The major operating costs and energy utilization associated with this process relate to regeneration of the carbon. Essentially, the lighter the adsorptive loading of the carbon the less frequent is the need for regeneration, and the lower are the costs and energy requirements for a particular application.

Recent groundwork has been laid for development and optimization of an integrated biological-adsorption process which utilizes an expanded, or partially fluidized, bed of granular activated cabon with a fixed-film biological growth on the surface of the carbon (19). The process is capable of providing simultaneous oxidation of biodegradable contaminants and adsorption of non-biodegradable contaminants in a single reactor, obviating the need for dual treatment systems. Expected results are lower capital cost for a single reactor system, less frequent regeneration of the carbon, and reduced energy requirements and operating expenditures.

The biologically-extended adsorption concept is presented here as but one example of an integrated treatment scheme. The use of powdered carbon in aeration basins of otherwise conventional activated sludge systems to upgrade the performance of such systems is another simple concept of an integrated treatment. Despite the relative simplicity of these concepts, substantial research effort must be directed toward elucidation of process mechanisms, significant process variables, process optimization and subsequent development of rational design and applications criteria.

Multiple-Purpose Integrated Systems

The notion of integration can be expanded to broader horizons in the pollution control field. Again only by way of example, a recent research publication describes the potential of combined clarification of raw wastewater and incinerator stack-gas wet scrubber effluent, and the use of the clarified effluent for the scrubbing of stack emission (20).

Wet scrubbers require huge amounts of water, and their effluents are acidic and turbid, requiring—at a minimim—neutralization and clarification prior to recirculation and/or blowdown or disposal. Raw wastewaters, on the other hand, contain large amounts of colloidal organic solids which can be removed effectively by chemical coagulation. These wastes are also usually well buffered. The concept of the system cited as an example here is that the combined treatment of raw waste and scrubber effluent will provide effective coagulation of the former by the fly ash contained in the latter, which, concomitantly, will be also removed effectively in the coagulation-sedimentation process. Further, the fly ash can provide some phosphate removal by precipitation and/or adsorption. At the same time, the acidic scrubber effluent will be at least partially neutralized by the buffer capacity of the wastewater. Lastly, the clarified combined effluent can serve as a recyclable supply of water for the wet scrubber system. The organics in this stream will provide enhanced wetting of the fly ash in the stack emission, thereby improving the performance of the latter.

Initial indications of this work are that the integrated scheme will substantially benefit the performance of both the water and air pollution control systems involved, yielding: higher rates of clarification; higher gas phase and water phase removal efficiencies; savings in chemicals for coagulation, neutralization, and fly-ash wetting, and the associated costs and energy requirements; savings in on-site sludge treatment and disposal facilities; and, perhaps most importantly, water conservation.

Even with respect to this one example of a multiple-purpose integrated system there remain many questions that can be answered only by fundamental research on process mechanisms and variables. For example, some question exists as to whether environmentally hazardous materials such as heavy metals in the stack emissions may be solubilized in the wastewater effluent by organic or inorganic chelation. If so, research is required to define what process modifications may be effected to eliminate or minimize this possibility. Surely the potential environmental rewards of developing and refining such a system warrant the fundamental research required to define optimum conditions for design and application.

This is but one example of the type of multi-purpose integrated environmental control system I address in this research category. I am confident that many advanced technologies of this type can be evolved by our imaginative thinkers, but only if our fundamental research properly addresses the development of insight to process mechanism and dynamics.

Secondary Resource Recovery

In this last research needs area I refer to resource recoveries which are secondary only in the sense that recovery of the water resource itself must be our primary concern in wastewater treatment. Of the five areas of research need I have identified, this last one will be discussed in least expanse. If I had to assign priorities amongst the five areas, secondary resource recovery would rank last, but only because of its more limited scope of application, and its site-specific character. This lack of universality should not, however, detract from its value in those applications for which it is appropriate.

I include energy in the class of secondary resources for purposes of this discussion, although for some industrial waste treatments it may constitute a primary resource. In fact, the greatest potential for all secondary resource recovery probably relates primarily to industrial waste treatment. In such applications stream isolation and point source treatment by specific processes which yield recovery of secondary resources are more feasible than in municipal wastewater treatment applications. Further, industrial wastes are much more likely to contain secondary resources which have direct recovery value and are present in sufficient quantity to warrant recovery.

There are numerous examples of such recoverable resources; heavy and precious metals, catalysts, oils, protein sources, enzymes, and dyes, to cite a few. Even energy, in the form of heat content of water, can often be recovered. By way of illustration, treatment of dye baths by selected physicochemical processes at the elevated temperature of the dying-process can often recover not only the water for reuse, but also the latent energy contained therein. Our research must be so structured as to identify such potential applications, and then develop fundamental means for exploitation of such opportunities to the benefit of man and his environment.

SUMMARY

I have defined, largely by way of departure for further consideration, several broad areas which I believe our fundamental research must address. They are not all inclusive, nor intended to be, but rather areas which my own activities indicate have merit for further and continuing fundamental research. Others of equal or perhaps greater merit will be advanced by my colleagues in this seminar.

I have underscored the reality of water reuse and resource conservation, and the consequent need for emphasis on reclamation processes and advanced technologies in wastewater treatment. This is not a debatable issue, but rather a matter of fact. In view of the costs and energy requirements commonly associated with advanced technologies in their present state, I have pointed to the needs for, and value of, fundamental research designed to enhance and refine our level of sophistication regarding the mechanisms and dynamics of these processes. Such research should be designed also to gain fundamental insights which will foster imaginative thinking in the development of innovated treatment processes and systems.

I noted earlier in this discussion that many may question, from an applications standpoint, the necessity of fundamental research on the character of complex reaction mechanisms and micro-transport processes. Indeed, the need to even consider such factors is a question sometimes put to me—in one form or another—by students enduring the rigors of process thermodynamics, kinetics, transport processes, and reactor dynamics; this is excusable of novitiate students. More disturbing is the fact that the question is sometimes addressed to me by practicing engineers and others responsible for the design and operation of water and wastewater treatment systems.

The answer to the question lies in the fact that the complexity of problems we now face in wastewater treatment, and the increased demands for higher levels of treatment and reclamation, demands a sophisticated engineering approach to design of wastewater treatment processes. There is a time-honored "joke" in the field that sewage treatment plants have most commonly been designed by "dusting off the plans from the last plant." Unfortunately, this has too often been the case. However, forward-looking engineers recognize that more specific and quantitative design approaches are required. The concepts of process dynamics are finally emerging as the basis for rational design of treatment systems. This approach is evidenced most markedly in industrial waste treatment and reclamation, but is being decanted gradually to the municipal waste treatment area.

Only fundamental research can establish a firm basis for evolution of methods and procedures—and indeed thought processes—through which we can improve levels of wastewater treatment and reclamation. One might argue that we now have advanced technologies that can provide high levels of treatment. The question I then pose is whether we can afford brute force application of such technologies. The obvious answer is that we cannot. Consumable resources such as chemicals and energy are increasingly scarce and costly. Our approaches to the optimum use and conservation of such resources in accomplishing the objectives of wastewater treatment must thus be increasingly innovative and sophisticated. Once again, fundamental research must provide solutions to fundamental needs.

The cost—more appropriately investment—to support the fundamental research required to pursue such issues as I have outlined is large. The upside reward of such investment is as enormous as the environment itself. The downside risk associated with neglect of that investment is obvious.

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DYNAMICS AND CONTROL OF WASTEWATER TREATMENT PLANTS

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INTRODUCTION

The need for consideration of dynamic behavior in both the design and operation of processes used for wastewater treatment is frequently greater than that for industrial processes because of the large temporal variations which occur in wastewater composition, concentration, and flow rate. However, our understanding of this dynamic behavior and how it may be modified through the application of modern control systems is in its infancy. Gross process failures are all too frequent, and even when these are avoided, it is not unusual to find significant variations in process efficiency, not only from one plant to another but also from day-to-day and hour-to-hour in the same plant.

Dynamic mathematical models are necessary for the description of time-variant phenomena, as commonly encountered in wastewater treatment processes, and increasing efforts are being devoted to their development. The models usually consist of sets of non-linear differential equations for which analytical solutions are not available, and this is one of the major reasons why the dynamic behavior of wastewater treatment processes has not been adequately considered in past years. However, computer simulation has largely eliminated this bottleneck, and the current problem is not so much one of being able to obtain a solution as it is to insure that the model adequately describes the dynamic behavior of the process being simulated.

When the dynamic behavior of a plant has been defined, the environmental engineer should then become interested in modifying this behavior so that it will conform to some desired behavior. This can be accomplished through either process design or the incorporation of control systems. In the past, the major research efforts have been devoted to improvements in process design with little attention being paid to process operation. Consequently, most control systems reported on in the literature have been, of necessity, selected on an empirical basis because of a lack of fundamental knowledge regarding dynamic behavior or control strategies.

A brief paper such as this must pass over much of the detail of current and proposed dynamic models and control strategies, and yet it is this detail in which the serious researcher is interested. For more comprehensive survey papers, the reader is referred to the papers of Andrews (1), Olsson (2) and Beck (3). A more detailed description of research needs may be found in the Proceedings (4) of a workshop, sponsored by the Environmental Protection Agency in 1974, which had the objective of defining and establishing priorities for research needed in the automation of wastewater treatment systems. For a mixture of theory and practice on the dynamics and control of wastewater treatment plants, the reader is referred to the Proceedings (5,6) of two workshops sponsored by the International Association on Water Pollution Research.

The objective of this paper is to briefly describe each process found in a typical municipal wastewater treatment plant, point out the status of research and research needs on dynamic models and control strategies for each process, and examine the interactions between the different processes and the interactions of the plant with other systems. The author wishes to apologize in advance for referring primarily to his own work and that of his associates. The briefness of this paper does not permit otherwise, and the research of others is cited in the references given.

MUNICIPAL WASTEWATER TREATMENT PLANT

A simplified flow diagram for a typical municipal wastewater treatment plant is shown in Fig. 1. The major processes for treatment of the wastewater (fluid processing train) are primary sedimentation, activated sludge, and chlorination. It will be noted that the activated sludge process is physically composed of two units, these being the biological reactor and the solids-liquid separator. The organic solids either removed or generated in the fluid processing train serve as inputs to the solids processing train which consists of thickening and anaerobic digestion with the thickener being most appropriately thought of as

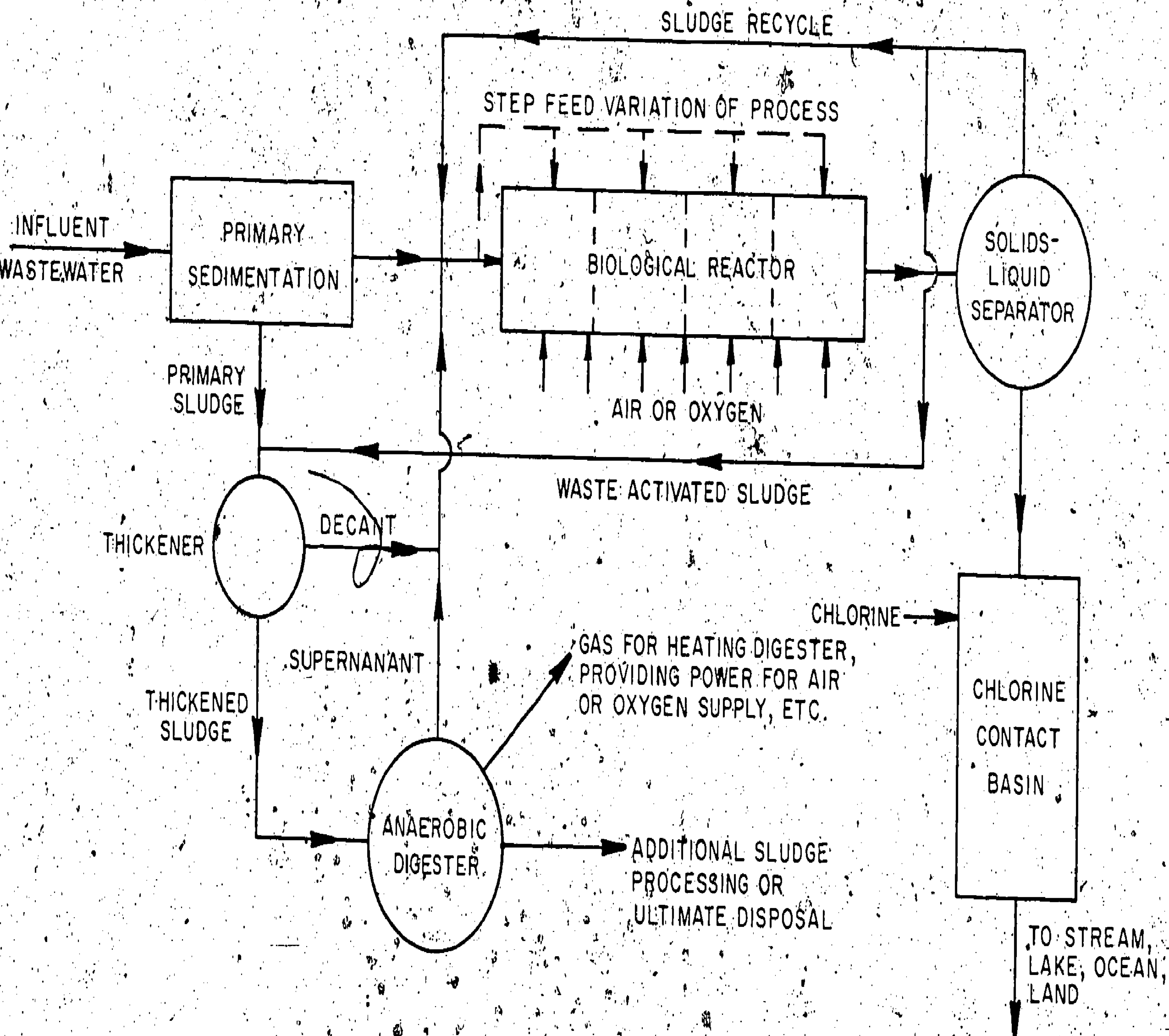

Figure 1. Simplified Flow Diagram for a Conventional Wastewater Treatment Plant.

representing the interface between the liquid and solids processing trains. While many other processes are available for both fluid and solids processing, they will not be discussed herein. This should not be taken to imply that research on the dynamics and control of these alternate processes is not needed. The system to be discussed in this paper has been restricted to permit some increase in detail and also to remain within the area of expertise of the author.

The processes illustrated in Fig. 1 have a long history in wastewater treatment, and a sizeable body of literature is available on each. However, the majority of the research literature on these processes is oriented toward design and moreover is usually based on the assumption of steady state. The use of the information contained therein for guiding research on dynamic modeling and control strategies is consequently limited. An alternate source of information is the work of operations engineers who are daily faced with the dynamic phenomena exhibited by these processes and the need to develop practical control strategies. The author has found the advice of these operations engineers to be of great value both in establishing hypotheses and in qualitative validation of dynamic models and control strategies.

Primary Sedimentation

The primary settler is the first process in a wastewater treatment plant which has a significant influence on the wastewater characteristics. As its name implies, the principal function of this unit is to remove, by gravity sedimentation, the larger organic solids from the wastewater.

Development of a mechanistic model for this process is beset with problems. The solids in wastewater are not discrete particles of uniform size, density, specific gravity or shape. They also vary in surface characteristics and tend to flocculate as they settle. As discussed by Camp (7), the process operates close to hydraulic instability. The hydraulic regime of the settler is therefore difficult to describe, since it is influenced by inlet and outlet feed and withdrawal patterns and currents due to changes in temperature, wind, solids concentration, salinity, etc. Because of these characteristics, it appears to the author that stochastic modeling techniques have much to offer in the development of a dynamic model for primary sedimentation. An entry to the literature on this type of modeling for the process is provided by the work of Silveston and co-workers (8,9).

Bryant (10), from a review of the literature, developed an empirical model for the primary settler. The objective of this model was to modify the plant influent wastewater characteristics so that they could serve as appropriate inputs to the activated sludge process. This is admittedly a crude model in that it does not consider the effect of several important variables, such as influent suspended solids concentration and short term hydraulic transients, on the effluent suspended solids concentration and is also not able to predict the concentration of solids in the underflow. However, it does provide appropriate attenuations and phase shifts for concentrations so that the inputs to the following process, activated sludge, are more realistic.

The process is usually so designed that very little control is possible, with the only manipulatable variable usually being the underflow rate. The normal reason for manipulating this variable is to control the density of the sludge underflow, thus avoiding the pumping of too dilute a sludge to the anaerobic digester. It should be noted that the need for this control loop has been reduced in many plants by placing a sludge thickener between the primary settler and the anaerobic digester.

The exploration of control strategies for this process by computer simulation is strongly dependent upon the development of a more realistic model to consider both the deterministic and stochastic components of the process. Also, even if such a model becomes available, the number of variables which can be manipulated is very limited. One of the first steps in the development of a control strategy, as is true for many other wastewater treatment processes, must, therefore, be a search for process modifications which will permit more control to be exerted. Two modifications which have been suggested by Andrews, *et al.* (11) are the installation of a moveable baffle at the inlet to the settler which could be automatically positioned to offset the effects of density currents, and incorporation of a moveable submerged weir which could be automatically positioned in the vertical dimension so as to permit withdrawal of the clearest possible effluent. In the absence of a more [illegible]tic model, ideas such as these would be best explored by p[illegible] or full scale physical experimentation instead of by computer simulation.

Activated Sludge Process

The activated sludge process is the heart of the fluid processing train and consists of two units, a biological reactor and a solids-liquid separator, as illustrated in Fig. 1. The three major inputs to the biological reactor are the wastewater from the primary settler, concentrated activated sludge from the solids-liquid separator, and air or high purity oxygen. The microorganisms in the activated sludge react with the organic pollutants in the wastewater and oxygen to produce more activated sludge, carbon dioxide, and water. Newer versions of the process are also capable of removing nitrogen and phosphorus if this is desired. The effluent from the biological reactor flows to the solids-liquid separator where the activated sludge is separated from the fluid phase. The solids-liquid separator serves three functions, these being the production of a clarified overflow, limited storage of sludge, and concentration of the sludge for recycle to the biological reactor.

The recycle of concentrated sludge is an essential feature of the process since it serves to both increase the concentration of microorganisms in the reactor, thus increasing reaction rates, and maintain these organisms in a physiological condition such that they will readily flocculate and settle. However, recycle has also resulted in difficulties in understanding and modeling the process since it creates a feedback loop, thereby causing a strong interaction between the biological reactor and the separator. The two units must, therefore, be modeled as a system, a fact which has not been widely appreciated in past years.

Busby and Andrews (12) have developed a model for the process which couples a dynamic model of the biological reactor with the dynamic model of the separator, thus accounting for the strong interactions between the two units. Other key features of their model are the addition of structure to the activated sludge with this being divided into storage, active, and inert mass, and consideration of the removal of pollutants as a sequential process as indicated in Eq. 1. These additional features provide a rational basis for

$$\text{Pollutants} \rightarrow \begin{matrix}\text{Stored}\\ \text{Mass}\end{matrix} \rightarrow \begin{matrix}\text{Active}\\ \text{Mass}\end{matrix} \rightarrow \begin{matrix}\text{Inert}\\ \text{Mass}\end{matrix} \qquad (1)$$

changes in activity of the sludge, permit a variable time lag which is important in predicting dynamic behavior, and allow the model to exhibit a "rapid uptake" phenomena which is essential for explaining the behavior of the contact stabilization version of the process. The biological reactor is also subdivided into several stages in series with provision for the separate addition of wastewater to each stage. This feature, with those previously mentioned, provides a wide spectrum model which can be used to simulate several versions of the process including conventional, extended aeration, high rate, contact stabilization, and step feed activated sludge.

Although the above described model does incorporate substantially more features than previous models, it is by no means complete, and considerable additional research, especially pilot and field studies for validation, is needed before application to full scale plants. Major modifications which should be explored are structuring of the influent pollutants into soluble and suspended fractions, an improved expression for predicting the concentration of solids in the overflow from the separator, a quantitative relationship between the settling characteristics of the sludge and the biological process parameters, consideration of the effect of spatial differences other than depth in the separator, and incorporation of the interactions between the biological, liquid and gas phases for closed reactors. If nitrogen and phosphorus are to be removed, these reactions will also have to be incorporated into the model. Stenstrom (13), using the dynamic model for nitrification developed by Poduska and Andrews (14), has incorporated the conversion of ammonia to nitrate into the model.

The activated sludge process has a significant number of manipulatable variables, and a large number of control strategies are, therefore, possible. Control strategies in common use include control of the recycle sludge flow rate to maintain a constant ratio between this flow rate and that of the influent wastewater, control of the waste activated sludge flow rate so as to maintain a constant ratio (sludge age) between the mass of sludge in the reactor and the mass of sludge wasted per day, and control of the air flow rate to maintain a constant concentration of dissolved oxygen in the biological reac[illegible]ndrews and Lee (15) have suggested an additional co[illegible]tegy based on regulation of the distribution of [illegible]ass between the reactor and separator by m[illegible]ng the point(s) at which the wastewater is added along the length of the reactor. The basic idea for this strategy came from a pursual of the plant scale research of Torpey (16) in New York City, thus indicating the value of searching the literature for reports by operations engineers. By use of this strategy, Torpey was able to prevent gross process failure (discharge of substantial quantities of sludge in the overflow from the separator) as well as to exert long term control over the settling characteristics of the sludge.

More recently (1976), Andrews, Buhr and Stenstrom (17) have proposed that the specific oxygen utilization rate (SCOUR), which should be an excellent indicator of sludge activitiy, be used in set point control of the process. SCOUR is a variable which illustrates the value of having computing power available for plant control, in that for the oxygen activated sludge process it can be computed from on-line material balances on oxygen and sludge using only four readily available measurements. Another possible control strategy, which has been reported on by Olsson and Andrews (18,19), is the use of the dissolved oxygen profile along the length of an air activated sludge reactor for process control.

Although the above mentioned strategies show considerable promise for improving control of the activated sludge process, there is much still to be learned. The process is a complex biological system with several choices of variables which can be measured, computed, and manipulated. Still another factor which complicates the picture is the large difference in time scales (minutes to days) over which the strategies must be effective.

The author would like to again apologize for referring primarily to his own work and that of his associates. Space simply does not permit a detailed review of the literature on the dynamics and control of the activated sludge process. Suffice it to say that even though only a small number of researchers have been involved, they have made significant contributions.

Chlorination

The chlorine contact reactor is the last process in the fluid treatment train for a conventional treatment plant. Although the primary objective of chlorination is disinfection, chlorine also reacts with the organic pollutants and nitrogen compounds in the wastewater, and these reactions must be considered in the development of a model for the process. For example, oxidation of the organics in the wastewater results in a reduction in the free chlorine residual, whereas reactions with ammonia yield chloramines which are disinfectants but are considerably less effective than free chlorine. Successful disinfection is dependent on maintaining some minimum residual chlorine concentration for sufficient time to achieve the required bacterial kill.

A preliminary dynamic model of the chlorine contact reaction has been developed by Bryan (10). However, this model did not consider the complex chemical reactions of chlorine with ammonia in the wastewater, nor was it designed to predict coliform concentrations upon which many plant effluent standards have been based. Stenstrom (13) has developed a model which overcomes these deficiencies as well as providing a better representation of the hydraulic regime of the reactor. His model is based upon the reactions of chlorine with ammonia which have been reported by Morris and co-workers (20,21) and kinetic coefficients for disinfection estimated from the data of Butterfield, *et al* (22). The model is composed of several second-order partial differential equations, and since these were stiff equations, several numerical analysis techniques were evaluated for simulation of the process.

Automatic control of the chlorination process is widely practiced in wastewater treatment with the primary reasons for this being (a) the high cost of chlorination, and (b) the availability of technology from the potable water treatment field. Three commonly used control strategies are control of the chlorine feed rate to maintain a constant ratio between this rate and that of the influent wastewater, feedback control based on the signal from a residual chlorine analyzer at the process effluent, and compound loop control consisting of a combination of the two control strategies mentioned above.

Both Bryant (10) and Stenstrom and Andrews (23) have reported upon advanced control strategies for the process based upon the availability of computing power for process control. In both of these strategies it was necessary to base control upon measurement of residual chlorine, since long time delays are involved in measuring coliform concentrations. It should be noted that the models and control strategies of both Bryant and Stenstrom have not been validated, other than by literature searches, and should, at best, be considered only semiquantitative (responses in the right direction and order of magnitude).

Thickener

As illustrated in Fig. 1, the sludge thickener serves as an interface process between the fluid and solids processing trains. As usually stated, the purpose of the thickener is to reduce the volume of sludge to be handled by the anaerobic digester and to smooth out fluctuations in flow rate and concentration of sludge. However, it also serves to improve the performance of the primary sedimentation process and the solids-liquid separator in the activated sludge process, since a portion of the thickening function of these units can be transferred to the thickener, thus permitting them to concentrate more on the clarification function.

Two types of thickeners (gravity and flotation) are in common use, and the operation of either is still very much an art, with neither dynamic models or control strategies based on fundamental principles being available, although work is in progress. However, much of the basic theory for the solids-liquid separator in the activated sludge process should be applicable to gravity thickening. Complicating factors will be the need to modify the basic model to account for the movement of displaced water upward through the concentrated sludge and the greater significance of interparticle forces in the compression zone.

Despite the lack of fundamental dynamic models or control strategies, considerable qualitative knowledge concerning what variables should be measured and manipulated can be gained from the literature. Included among these variables are the feed sludge concentration and sludge blanket depth for gravity thickening and depth of float, air pressure, air/solids ratio, and recycle of subnatant for flotation thickening.

Anaerobic Digester

The anaerobic digestion process is widely used for the treatment of organic sludges either removed in primary sedimentation or generated in the activated sludge process. It has several significant advantages over other methods of organic solids processing, and among these are the formation of useful by-products such as methane gas and humus-like slurry well suited for land reclamation. The energy contained in the gas is not only sufficient to operate the reactor at an elevated temperature; it is also produced in sufficient quantities to serve as the major source of power for the remainder of the treatment plant. Unfortunately, even with these advantages, the process has not, in general, enjoyed a good reputation because of its poor record with respect to process stability.

The present dynamic model of the process has evolved over the past twelve years, and this evolution can be followed through the publications of Andrews (24,25), Andrews and Graef (26), Graef and Andrews (27), and Buhr and Andrews (28). The model, which is summarized in Fig. 2, was developed from material balances on components in the biological, liquid and gas phases of a CFSTR. Although not shown in Fig. 2, the latest modification of the model (28) incorporates the effects of temperature to permit simulation of the thermophilic version of the process.

As indicated in Fig. 2, there are strong interactions between the phases, as well as internally within each phase. These interactions must be considered if the model is to predict the dynamic response of the five variables most commonly used for monitoring the condition of the process with respect to stability. These variables are (a) volatile acids concentration, (b) alkalinity, (c) pH, (d) gas composition,

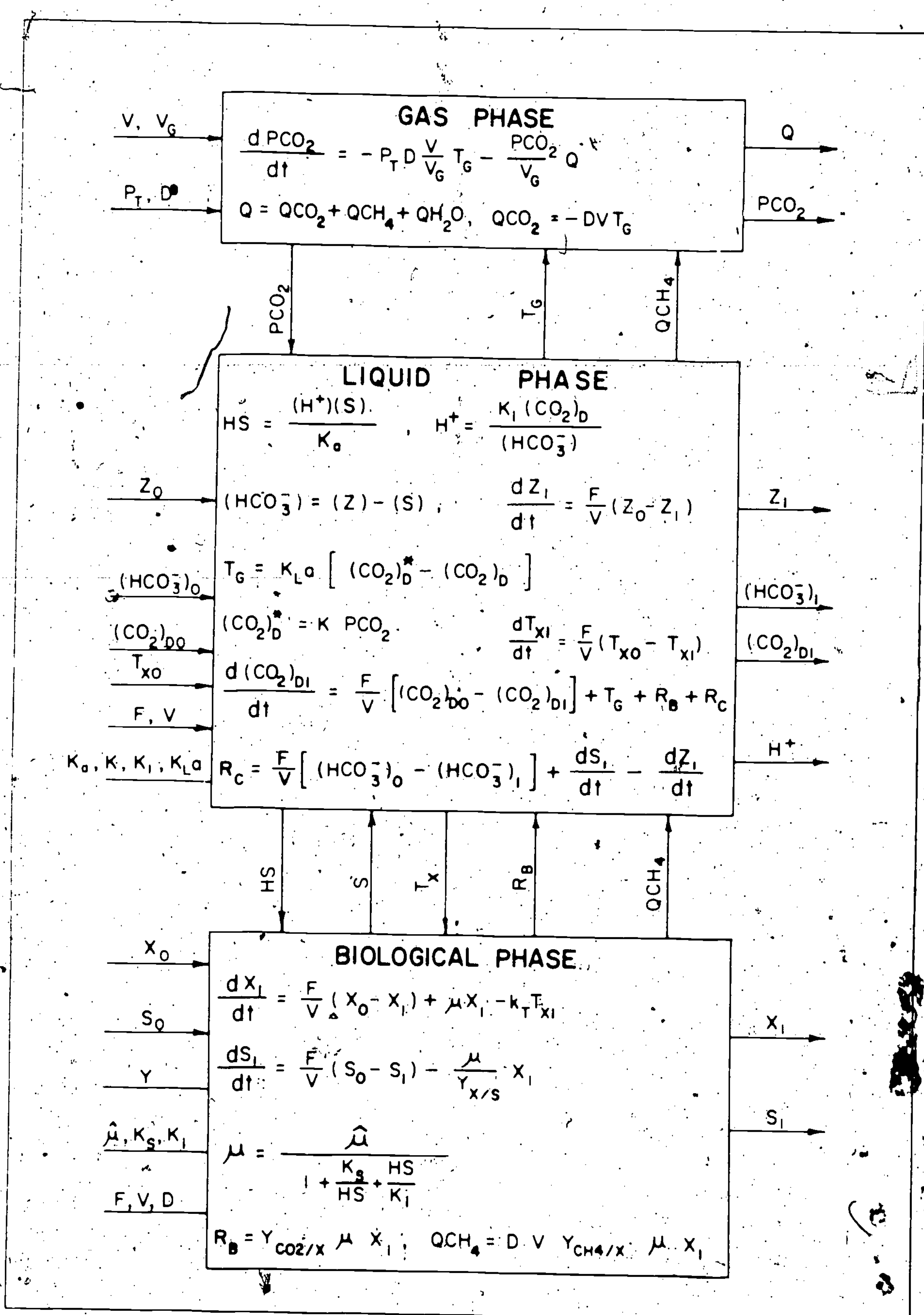

Figure 2. Summary of Mathematical Model and Information Flow for the Anaerobic Digester.

and (e) gas flow rate. Relationships such as stoichiometric coefficients, kinetic expressions, equilibrium relationships, charge balances, and mass transfer equations are used to reflect these interactions.

Two key features of the model are the use of an inhibition function (24) in lieu of the Monod function to relate volatile acids concentration and specific growth rate for the methane bacteria, and consideration of the unionized fraction of the volatile acids as both the growth limiting substrate and inhibition agent. These are important modifications since they permit the model to predict process failure by organic overloading and reflect the relative importance of both pH and volatile acids concentration as indicators of process condition. The model can also predict failure brought about by the introduction of toxic materials, and the latest modification (28) permits the evaluation of process stability with respect to changes in operating temperature.

Hybrid computer simulations have been used (27) to analyze process stability by simulating digester overloading and observing what changes in design and operational characteristics provided the best buffer against process failure. The analysis procedure involved making a change in a digester parameter, such as residence time, followed by simulating larger and larger step increases in digester loading until failure occurred. By plotting the focus of points of critical organic loading rate vs. reactor residence time or other parameter, it was possible to obtain a semiquantitative measure of digester stability.

Simulation studies have provided qualitative evidence for the validity of the model by predicting results similar to those commonly observed in the field. However, in using this model it must be remembered that it was developed with the objective of predicting (and indicating techniques for preventing) gross process failure. It is not adequate for the prediction of process performance as measured by the destruction of organic solids or the dewatering characteristics of the residual sludge, both of which are important in digester operation. Prediction of the destruction of organic solids will require expansion of the model to consider the conversion of organic solids to methane and carbon dioxide as a series reaction with intermediates. An especially interesting intermediate may be hydrogen since the recent microbiological literature (29,30) implicates hydrogen as a major intermediate in the production of methane. Prediction of the sludge dewatering characteristics will most likely have to be accomplished through empirical relationships.

In selecting a control strategy for the anaerobic digester, a wide variety of output variables, and combinations thereof, are available for initiation of the control action. Similarily, a wide variety of control actions are available. Graef and Andrews (27) have shown that the type of control strategy to be utilized is dependent upon the type of overloading to which the digester has been subjected. Obviously, the availability of suitable sensors must also be considered in selecting a suitable strategy.

A new control strategy proposed by Graef and Andrews is the scrubbing of carbon dioxide from the digester gas with subsequent recycle. This is especially attractive since the most common technique for digester mixing is gas recirculation. This would provide process control through the removal of a weak acid, carbonic, instead of by the addition of a base as is the usual practice, and should be effective in preventing failure by organic overloading. Another proposed control strategy was the recycle of concentrated sludge from a second stage digester using the rate of methane production, a calculated variable, as the control signal. This strategy should be effective in preventing failure due to an overload of toxic materials. Although the control action proposed is not new, having first been suggested by Buswell (31) in 1939, the proposed control signal, rate of methane production, is new. The rate of methane production has the advantage of being easily calculated from common gas phase measurements (flow rate and composition) and should serve as an excellent indicator of the activity of the methane bacteria which are the most sensitive organisms in the digester. An analogue can be drawn here between the use of methane production rate as an activity indicator in the anaerobic digestion process and oxygen utilization rate as a measure of microbial activity in the activated sludge process.

PROCESS INTERACTIONS

The author and co-workers (10,12,32) have been attempting for the past nine years to put the individual process models together into an overall plant model for the exploration of process interactions. However, this task has not been fully completed since each study of the plant as a system has indicated a lack of fundamental knowledge of the dynamic behavior of one or more critical processes. In Bryant's study (10), it rapidly became obvious that an understanding of the dynamic behavior of the solids-liquid separator was crucial to an understanding of the dynamic behavior of the activated sludge process, and most of Bryant's attention was, therefore, focused on the development of a dynamic model for the separator. However, Bryant did link together models for three processes in the fluid processing train. He also developed a dynamic model for the hydraulics of this train to permit the prediction of flow rate response to hydraulic forcings.

Busby (32) was the next to attempt the development of an overall plant model but stopped far short of this. However, he did develop a substantially improved model for the biological reactor in the activated sludge process and used this, in conjunction with Bryant's model of the solids-liquid separator, to explore the dynamic interactions between these two units and to simulate control strategies for the process as a whole. As previously mentioned, these are very strong interactions which necessitate the consideration of the two units as a system.

Stenstrom (13) has performed the latest study on the integration of the individual process models into an overall plant model and has succeeded in incorporating all of the processes shown in Fig. 1. However, it was necessary for him to spend a substantial amount of time in developing a dynamic model for the chlorine contact reactor as well as making substantial improvements in the activated sludge process model and exploring the use of SCOUR for the control of this process. Consequently, his models for the sludge thickener and anaerobic digester are simplified models based primarily on empirical steady state relationships. However, since these processes have substantially slower time responses than the fluid processing units, the use of these steady state relationships should not be a serious handicap in the use of the model for exploration of interactions between the two treatment trains.

The key interaction studied by Stenstrom (13) is the forward interaction (sludge feed to the digester) between the activated sludge process and the anaerobic digester. This interaction has a substantial influence on the external energy requirements for the entire plant and is, therefore, of substantial importance in these days of energy shortages. It is possible to operate many plants so that a large portion of the organic solids generated in the activated sludge process are either aerobically oxidized in this process or anaerobically oxidized in the anaerobic digestion process. Aerobic oxidation requires energy (air supply), whereas anaerobic oxidation produces energy (methane gas). Consequently, too much oxidation in the activated sludge process represents a double loss of energy since more energy is required for the activated sludge process and less is produced by the anaerobic digester.

Another interaction of importance is the feedback of digester supernatant to the activated sludge process. Since digester supernatant contains high concentrations of organics and ammonia, it can create problems when it is transferred from the solids to the fluid processing train. However, this interaction can sometimes be used to advantages as has been demonstrated by Kraus (33), an operating engineer who was faced with the problem of operating a plant which was subjected to seasonal loads of wastes containing high concentrations of carbohydrates. Kraus modified his plant so that he could add digester supernatant to a reactor in which recycled sludge was reaerated prior to being returned to the biological reactor. The ammonia was converted to nitrate in this additional reactor. The nitrate then served both as an additional source of nitrogen for the high carbohydrate waste and as a supplement to the oxygen supply (by serving as an alternate hydrogen acceptor in front portion of the biological reactor).

PLANT INTERACTIONS WITH OTHER SYSTEMS

Although the wastewater collection system, the receiving body of water for discharge of the treated wastewater, and the receiving system for ultimate disposal of the residual solids are outside the boundaries of the system under consideration herein, they must obviously be taken into account since they represent the inputs and outputs for the plant, are essential for the establishment of a plant objective function, and modify both the dynamic behavior of the plant and type of control strategy to be employed.

In the long term, dynamic models of treatment plants should be coupled with dynamic models of the wastewater collection system, receiving bodies of water, and the receiving system for the residual solids. A substantial amount of research has been conducted on dynamic models of the receiving waters and an excellent introduction to this subject is provided by the book of Thomas (34). Research on the dynamics of wastewater collection systems is more recent, with an example being the report by Beck (35). Dynamic models are not available for the ultimate disposal of residual solids, perhaps because these have much longer time constants than the other systems discussed and their dynamic nature has not been as well recognized.

Modification of the inputs to the plant from the wastewater collection system may be obtained by control of the collection systems, as exemplified by the work of Anderson (36) on the Minneapolis-St. Paul system, or the use of equilization tanks on plant inputs as given in the papers of Andrews, Buhr, and Stenstrom (17) and Di Toro (37). Studies on the modification of plant inputs have been primarily concerned with maintaining input wastewater flow rates and compositions as constant as possible. This may be the correct approach; however, it is based on the unproven assumption that steady inputs would be best for maximizing plant performance. On the other hand, there are chemical processes, as illustrated in the review by Bailey (38), for which periodic inputs are best for maximizing performance. For wastewater treament plants the possibility, therefore, exists that there may be some optimum waveform and frequency for the plant inputs which is not necessarily a steady input.

The possibility also exists for control of the plant so that it operates at a variable efficiency in order to match the assimilative capacity of the receiving body of water which usually varies with time. An example of this type of interaction would be that given by Shieh (39), who simulated the effects on the upper Delaware Estuary of discharging wastewater on the seaward movement of the tide and storing the wastewater during landward movement.

SUMMARY

There is a need for more consideration of dynamic behavior in both the design and operation of wastewater treatment plants. A better understanding of dynamic behavior and the application of modern control systems offers many potential benefits. Included among these are improved performance, reduction in size and therefore construction costs of new plants, improved reliability, more efficient use of operating personnel, and lower operating costs. However, these benefits are still clearly "potential" since the application of dynamic modeling and control to wastewater treatment plants is in its infancy.

Specific research needs for the individual processes in a typical wastewater treatment plant have been presented in this paper. Many other processes are used in wastewater treatment, and there are obviously similar research needs for these processes. A logical progression of research on the dynamics and control of wastewater treatment plants is as follows:

a. Dynamic mathematical models should be developed for the individual processes. Both the mechanistic and stochastic approaches should be used since both deterministic and stochastic components will be needed to adequately describe process performance. Literature searches, discussions with operations engineers, and bench, pilot, and full scale experimentation will all be needed for the development and continued improvement of individual process models.

b. Tentative control strategies should be explored using dynamic models and computer simulation. The more promising of these control strategies should then be tested at pilot and/or full scale. Maximum use should be made of the newer tools of the control engineer such as on-line state/parameter estimation, [illegible]ation.

c. The individual process models and control strategies should be combined into an overall plant model and control strategy so that the interactions between the processes can be explored and taken into account.

d. In the long term, dynamic models of treatment plants should be coupled with dynamic models of the wastewater collection, receiving bodies of water, and the receiving system for the residual solids.

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DISCUSSION

C.P.L. Grady (Purdue University) for Dr. Perry McCarty

Please comment on where basic microbiological research leaves off and where our more applied microbiology begins. Where do we have to pick up the gauntlet?

Stanley Klemetson (Colorado State University) for Dr. W.J. Weber, Jr.

During our visit to the Stander Wastewater Reclamation Plant in South Africa, we saw applications of water reuse. What research do you see necessary to transfer some of this knowledge in South Africa and other parts of the world to encourage large scale application of water reuse in this country?

Weber's Reply:

The Stander Wastewater Reclamation Plant is exemplary of recent progress in the application of advanced technology to wastewater reclamation and reuse. The success of this project is due in one measure to the urgency of water supply problems in South Africa, and in another to the acumen of those involved in developing and demonstrating reclamation and reuse as a viable water resource alternative.

You ask what research is necessary to encourage large scale application of reuse. The research needs I have discussed relative to continued refinement of those factors which ultimately ensure and advance the technical feasibility, rational design, and cost effectiveness of reclamation technology. In so far as research accomplishes these objectives, it will ultimately encourage public acceptance of water reuse.

To turn a common phrase, necessity is the mother not only of invention but of application as well. For large scale application of any technology there must be a recognized need which it addresses, and a public confidence in its ability to resolve that need. Necessity in the present context involves identification of manifest or potential problems associated with water supply, increasing pollution, and/or unplanned reuse in a particular area, and recognition of reclamation and purposeful reuse as a potentially attractive water management strategy.

Application of water reclamation and reuse demands as much by way of public education and social acceptance as it does by way of basic research and technology. South Africa has apparently been successful in both regards.

E.H. Ted Curtis (U.S. Department of the Interior) for Dr. W.J. Weber, Jr.

Walt Weber indicated we were responsible for Technology Transfer—if 1/3 of the W.W.T. plants aren't working properly, perhaps we have dropped the ball on Technology Transfer, and we ought to be spending our time there.

Weber's Reply:

I have no basis to confirm or repudiate the fraction you indicate for plants that are not functioning properly, Ted. On the basis of personal observations, I concur that there are many which do not meet performance specifications and/or treatment criteria.

This relates in some instances to inadequate design and/or construction, in others to improper operation and/or maintenance, and in yet others to changing waste load characteristics and/or treatment requirements. There are thus several levels of both technology and personnel at which more effective transfer of information must take place.

If by suggesting that—"we ought to be spending our time there"—you mean in technology transfer *rather* than basic research, I say no; major efforts in both areas are required for successful development and implementation of measures for environmental protection. Researchers and educators have neither the philosophical charter nor physical capacity to assume responsibility for effective technology transfer at all levels. Nonetheless, we do have definite obligations in this regard.

It is incumbent upon the researcher to make available his essential findings, and to address, as far as possible, their applications context. I underscore the word essential, for we are all aware that the literature can become cluttered - and the reader discouraged or misled - by fragmented and/or trivial publications; researchers have responsibilities as both authors and peer reviewers in this regard.

The educator, be he active researcher or not, is responsible for integrating research developments into his teaching. A larger responsibility of the educator is to imbue his students with an openness to innovation, a sense of - and ability for - critical evaluation, and a sound scientific basis upon which to evaluate and incorporate new developments in design and operation applications.

David H. Howells (North Carolina State University) for Dr. W.J. Weber, Jr.:

We might all agree with your remark that "Only fundamental research can establish a firm basis for the evolution of methods and procedures through which we can improve levels of wastewater treatment and reclamation." There are many research needs associated with improving unit processes. This work can be costly with long delays in, or marginal payoff. From the standpoint of the funding agencies and their need to support appropriation requests, it seems to me that we must sort out these needs in the sense of priorities which will have the highest payoff. Can we develop adequate criteria to set such priorities?

Weber's Reply:

You pose a difficult question, David. I am tempted to answer, in the infinite wisdom of Caterpillar Tractor Company's popular advertisement, that "there are no simple solutions, only intelligent choices."

Each funding agency has a chartered set of missions, presumably rank-ordered according to the functions and responsibilities of the agency; these necessarily define priorities in the broadest sense. Beyond this, refined prioritization of research needs might best begin with critical examination of the "payoffs."

Qualitatively, payoff in the present context means solution of a technical problem. Quantification of payoff is quite another matter; this indeed is the crux of the question you ask. To develop appropriate criteria an agency must: 1) weight the problems associated with its mission(s); 2) define alternative solutions and the respective degrees to which they address the problems; 3) evaluate the potential or likelihood of each alternative for success; and, 4) evaluate the sensitivity of each problem solution to the research effort required to effect it. This creates a complex evaluation matrix, for which some characteristic values may be indeterminate.

Clearly there are no simple answers to your question; just as clear, however, is the fact that those making decisions on research priorities should be as technically competent and aware as those doing the research.

9

FUNDAMENTAL RESEARCH NEEDS FOR EVALUATING THE INTERACTIONS OF TREATMENT PLANTS WITH AQUEOUS SYSTEMS

Conference on Fundamental Research Needs
Water and Wastewater Treatment Systems

THE RELATION BETWEEN TREATMENT PLANT EFFLUENTS AND THE QUALITY OF NATURAL WATERS

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INTRODUCTION

The relation between treatment plant effluents and the quality of natural water systems, by definition, covers a large number of factors in both the engineering and natural sciences. This evaluation of research needs concerning this relationship is based primarily on four considerations:

1. The nature and characteristics of the generic types of natural water systems.
2. Heterogeneous kinetic systems and air-bed boundary conditions within natural water bodies.
3. Those constitutents which are naturally part of the biogeochemical cycles by contrast to those synthetic substances which are incorporated in, but are not natural components of, these cycles.
4. The inputs, originating from man's activity as well as natural processes.

Each of these factors are considered in the following sections, which include a brief observation concerning the present state of the art, an analysis of the adequacies and an appropriate recommendation.

TYPES OF NATURAL WATER SYSTEMS

It has been convenient to categorize natural water systems on a geophysical-hydrodynamic basis: fresh water streams and lakes and saline estuaries and near shore oceans. Within each of these systems, specific water quality constituents have been studied and analyzed. Notable progress, particularly in the field of engineering, has been made over the past few decades in quantifying the spatial distribution, usually under steady-state conditions, of plant effluents in these water bodies—a quantification which in many cases led to rational water quality management. This type of categorization is typical of the science and technology of our western culture, and it served us well in the formative stages of understanding the phenomena of the earth sciences and its engineering application in planning and management.

That model, although still useful in many ways, is becoming progressively more antiquated—we should not look to the interrelationship and interactions between these systems—i.e., the transition between the non-saline river and the saline estuary on their respective interactions with a lake and the near shore ocean. In each of these transitional geophysical zones, significant reactions occur which affect physical, chemical and biological forms, which are transported through them. In many instances, these reactions are unique to that zone and are evident neither in the upstream and downstream bodies of water.

HETEROGENEOUS KINETIC SYSTEMS AND BOUNDARY CONDITIONS

As the water bodies were viewed in a discrete independent fashion, so, too, the constituents. Again, in many cases, this viewpoint was quite justified. Within the framework described above, notable advances were made from both the scientific and engineering viewpoints. Both fundamental understanding was developed and rational planning was effected—notably with respect to such water quality parameters as bacteria, dissolved and suspended solids, dissolved oxygen and eutrophication. However, present and future water quality problems are indicating the necessity of analyses, which interrelate the dissolved, colloidal and suspended components affecting a particular constituent—e.g., the advances made in sediment transport in both rivers and estuaries may have significant bearing on other water quality items such as bacteria, dissolved oxygen and nutrients. While certain substances are particularly affected by bad conditions, others are more influenced by conditions existing at the air-water interface—e.g., accumulation at such interfaces and the transfer through the gas and liquid phases of these interfaces.

NATURAL VERSUS SYNTHETIC CONSTITUENTS

Advances made in the areas described above, related primarily to those constituents, which are normal components of the biogeochemical cycles. Relatively little of comparable value has been accomplished with respect to those constituents, which are characteristic of man's technological society. These substances, which are generally end products of industrial synthesis or breakdown—e.g., heavy metals, and synthetic organics, are not part of natural cycles. A fundamental understanding of the modes of transport, accumulation and transfer of these materials through food chains and biogeochemical cycles in natural water systems is of prime importance.

INPUTS—POINT-DISTRIBUTED AND STEADY STATE-TIME VARIABLE

The analysis and quantification of time-variable inputs such as urban runoff, agricultural discharges and natural drainage from areas essentially unaffected by man, have generally been classified as non-point or distributed sources, and their impact in many areas is greater than that of the treated effluents. Therefore, before proceeding to a further development of the analysis of treatment effluents, much more attention should be given to the non-point effects. Ostensibly, this issue was to be addressed in the 208 planning under EPA auspices, but it is not premature to report that in the majority of cases it has been inadequately analyzed. Neither the proper data base nor an adequate theoretical structure exists to properly account for the impact of these inputs. Ancillary to this point is the effect which treatment has on the rates of degradability of the constituents in the effluents.

Increasing levels of treatment frequently yield materials which are more stable and less reactive. In a comparable manner, changes in land use may bring out changes in the nature of inputs from these areas. Understanding of the changes and the impact on the quality of natural systems are of great importance, both theoretically and practically. The fields of science and engineering which deal with the effect of man's and nature's inputs on water quality are highly segmented—both academically and professionally—limnology, oceanography, meteorology and soil sciences and environmental, chemical and hydraulic engineering. The former are further divided into the biological, chemical and physical components (most significant is the absence of potomology—the study of fresh water streams and rivers). What is needed is a coordinated study and research plan of the interrelationships between and among these disciplines. Certain schools of environmental engineering and earth sciences are presently making some progress in this direction. More significant is the lack of applied science or engineering analysis in this regard, comparable with the previous work environmental (sanitary) engineers did in the past 20-30 years. The most glaring gap is the application of fundamental and earth sciences to practical water quality management problems (the NSF RANN program was one step in the direction of correcting this condition). In spite of the perhaps vague generality of this observation, it may serve as a guideline for the type of research which the NSF should support. One of the prime prerequisites for such a program is the participation of highly competent scientists and engineers, who have a recognized specialized area of competence and, in addition, are willing and able to relate that competence within a broader framework (a goal which was sought in the NSF RANN program, but was frequently lacking in the various projects).

GENERAL CONCLUSION AND RECOMMENDATION

In addition to the scientific-technological factors described above, these recommendations are also based on a consideration of those research areas which have not formed a significant part of programs of such agencies as EPA, USGS and NOAA, and which properly are part of a NSF program. The primary characteristic of such a program, to reiterate and conclude, is that it not only be fundamental and scientific, but equally important, that it bridges the gap between theory and practice. The application of such research programs plays an extremely practical and significant role in the assessment and protection of our aquatic environment.

DISCUSSION

Roy O. Ball (University of Tennessee):

As a reviewer for the Journal of the Water Pollution Control Federation, I endorse and applaud Dr. O'Connor's suggestion to publish less and review the work of others more carefully. Dr. Rohlich and other speakers presented similar thoughts which represent, in my opinion, a major theme of this conference.

O'Connor's Reply:

I strongly endorse Dr. Ball's suggestion about publications. I spoke about this topic extemporaneously at the meeting, but I did not include any formal writeup in my manuscript. My present comment is simple a strong endorsement of this point if the committee agrees.

Donald B. Aulenbach (Rensselaer Polytechnic Institute) for Dr. Donald J. O'Connor:

Have you also considered the needs for research in bed load? Some researchers (not I) feel that evaluation of the bed load can be used to describe the quality of the stream.

O'Connor's Reply:

In response to Dr. Aulenbach's reference to research needs in the bed load, I believe the evaluation of bed load may be important in describing the water quality of streams and estuaries, particularly with respect to heavy metals and strong chemicals which may be absorbed through them. I alluded to this point in item 2 of my talk and recommend further research in this area.

FUNDAMENTAL RESEARCH NEEDS FOR EVALUATING THE INTERACTIONS OF TREATMENT PLANTS WITH ULTIMATE SLUDGE DISPOSAL SYSTEMS

FUNDAMENTAL RESEARCH NEEDS FOR WASTEWATER SLUDGE MANAGEMENT

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INTRODUCTION

Current annual expenditures in the United States for management of residues produced by municipal wastewater treatment alone are approximately $500 million and may soon be expected to increase to nearly $1 billion*(1). Numerous factors contribute to the intensification of sludge management problems: requirements for higher degrees of wastewater treatment result in increased residue production; advanced wastewater treatment practices tend to produce sludges with properties which make sludge treatment and convenient reclamation or disposal difficult; industrial wastewater pretreatment requirements will result in production of massive quantitites of sludges laden with potentially toxic materials; changes in energy costs have invalidated many concepts of sludge management which were developed from experience over the years; and adoption of more rigorous standards of environmental quality require changes in some former sludge disposal practices.

These factors require reexamination of past practices for management of residues from wastewater treatment. Yet, timely response to the challenges in managing sludges in a manner which is efficient in terms of cost, energy, and resources, and consistent with environmental quality requirements is precluded by the lack of understanding of fundamental factors influencing sludge quality, the performance of sludge treatment processes, and the behavior and significance of sludge constituents in the environment. In addition to the inefficient allocation of resources, energy, and funds and the environmental degradation which may result from response to current sludge management challenges in an era of uncertainty, an incalculable waste of limited professional resources has also occurred in recent years as leaders in water pollution control have attempted to grapple with definitions of acceptable sludge management practices. [The four year debate preceding publication of 24-page draft of a "Technical Bulletin" on sludge management by the U.S Environmental Protection Agency (2) provides a timely illustration.]

As in many areas of water pollution control, present practices in sludge management primarily were developed empirically by practitioners. Subsequent elaboration of fundamental factors involved in sludge management has not generally proceeded to the extent that basic research has been carried out concerning the wastewater treatment processes which generate the sludge. The reasons for this are unclear but likely include the facts that sludge management caused fewer problems in the past era of minimal public scrutiny and that the subject of sludge has not titilated the imagination of researchers. Because of this historic neglect of the need for basic research concerning the properties, treatability, and environmental effects of sludges, the fundamental foundation required to effectively solve contemporary problems in sludge management does not exist. Certainly, legislative mandates will require progress toward effective wastewater management in the absence of desired basic information on sludges. However, sludge management problems will not disappear with the achievement of national goals for water pollution control. Long-term advances in basic understanding of sludges and their management will reap appreciable future rewards.

The discussion of basic research needs in support of improved capabilities for managing sludges is oriented toward sludges produced in municipal wastewater treatment. Many of the results of the proposed research would also be applicable to industrial wastewater treatment

*Based on an estimate by Bastian (1976) of 5 million tons per year of waste sludge on a dry solids basis in 1976 and 9 million tons per year when secondary treatment is practiced by all municipal facilities. An average cost of $99 per ton of dry solids were used as given by the average estimated cost of 46 sludge management options considered in recent engineering reports for 11 major cities (New York City, Washington, D.C., Washington Surburban, Corpus Christi, Boston, Knoxville, Southern California Regional, East Bay MUD, Denver, Sacramento, and Tampa), with prices adjusted to November 1977 using the Engineering News Record Construction Cost Index.

sludges, sludges from industrial and municipal water treatment, and sludges produced in the course of air pollution control. However, basic research needs which are unique to these other sludges have not, in general, been considered. Additionally, basic research needs related specifically to other residues such as dredging materials, solid wastes, and brines are not included.

BASIC PROPERTIES OF SLUDGES

The basic physical, chemical, and biological properties of sludges are complex and poorly understood. The fact that so little is known about the fundamental properties of sludges the better part of a century after sludges began to be commonplace materials testifies to the complexity of the material. Knowledge of basic properties of sludges could lead to improved capabilities for control of those properties, more effective and economical treatment of sludges, greater realization of reclamation possibilities, and reduction of the environmental impact associated with sludge disposal.

Physical Properties

In some biological sludges, the solid phase is capable of defying the downward force of gravity when it exists at a weight concentration of less than one percent. This is an extreme illustration of the problems encountered in attempting to remove water from all types of sludges by techniques such as thickening and dewatering. Chances for significant advance in capabilities for thickening and dewatering sludges would be appreciably enhanced by an understanding of the basic physical characteristics of sludges and of the physical, chemical, and biological conditions which establish those characteristics.

Investigation of the basic forces (e.g., chemical bonds, capillary attraction, etc.) which hold water in sludges and identification of the factors which determine the relative significance of such forces are illustrations of basic research needs concerning physical properties of sludges. Such research would be closely related to other important studies of the surface properties of solids in sludges and the variables in wastewater and sludge treatment processes which influence those surface characteristics.

Other essential fundamental studies concern the physical properties of sludges manifested as a result of the basic forces between particles and between particles and fluids considered in the previous paragraph. Rheological properties (like plastic viscosity) of the combined liquid and solid phases; properties (like permeability) related to flow of the liquid phase through the solid phase; and properties (like compressibility) related to the structural characteristics of the solid phase are some of the principal physical properties of sludges which control their performance in piping-pumping systems and in many processes for sludge treatment. Factors which operate in control of these important physical properties require elaboration. Knowledge of the influence of wastewater treatment processes, shear history, conditioning, stabilization, and other variables influencing the properties could lead to significant improvement in capability for controlling sludge quality and for tailoring the design of sludge treatment facilities to the characteristics of particular sludges.

Chemical Properties

Some of the fundamental research needs concerning the chemical properties of sludges relate to the physical studies described previously in that they involve development of an understanding of the forces between solids in sludges and of forces between the liquid and solid phases. Elaboration of surface properties of sludges and of the resulting nature of the flocculant organization of sludge solids could ultimately lead to an ability to control the physical properties of sludges through control of wastewater and sludge treatment processes.

Chemical characterization of sludges is also needed to develop an understanding of the forms in which chemical constituents of concern in environmental quality control exist. Knowledge of the partitioning of various heavy metals between insoluble organic complexes, chemical precipitates, exchanged ions, aqueous solution, and other forms is, for example, necessary. Recalcitrant organic compounds are examples of other chemicals for which better characterization is needed. Many additional research needs concerning chemical properties are suggested by discussions in later sections.

Biological Properties

Research needs concerning biological characteristics of sludges relate to biodegradability; the influence of biological transformations on the physical and chemical properties of sludges; and the presence of agents of infectious diseases. More complete evaluation of the prevalence of viruses, bacteria, protozoans, and helminths in waste sludges and exploration of factors influencing their survival is necessary. Additional research needs concerning biological properties are considered in sections which follow.

CONTROL OF SLUDGE QUANTITY AND QUALITY

Current problems in sludge management can be related to specific physical, chemical, and biological properties of sludges. Appreciable reduction in environmental impact, improvement of process performance and/or reduction in the cost of sludge management would be rendered possible by control of sludge quality. Opportunities for such control include life style alteration, industrial source control and pretreatment, and the selection, design and operation of wastewater and sludge treatment facilities.

Basic research needs in the area of sludge quality and quantity control range from studies of the effect of various types of consumer products on sludge quality and attitudinal studies related to public acceptability of alternative products to detailed studies of the influence of specific sludge treatment processes on sludge quality. Studies on wastewater source control and pretreatment for purposes of control of sludge quality and quantity would be similar to studies oriented toward alleviation of problems attributable to industrial wastes in wastewater treatment processes, but would emphasize basic changes in production practices, raw material selection, and industrial wastewater pretreatment procedures which could reduce sludge quantity and/or improve sludge quality. In addition to control of sludge quality, such research should address energy and other resources conservation.

For given source control and industrial pretreatment practices, the quantity and quality of sludges produced in wastewater treatment depend upon the treatment processes selected and the manner in which those processes are designed and operated. While research on wastewater treatment processes has, in the past, tended to emphasize pollutant removal without substantial concern for the nature of the residues produced, a focus on residue-related problems is appropriate for the future. Is it possible to develop wastewater treatment processes which will segregate in a small volume those constituents of wastewaters (such as heavy metals) which interfere with effective management of sludges? What factors in design and operation of biological wastewater treatment processes influence the physical characteristics of the excess sludge produced? What organic loading intensity in biological wastewater treatment processes results in least overall cost for wastewater treatment including residuals management? Can phosphorus removal be accomplished by means which do not result in massive accumulation of chemical sludges? These are but illustrations of questions related to control of sludge quantity and quality through manipulation of wastewater treatment processes for which a foundation of basic research results is needed.

The final opportunity for altering the quality of sludge occurs after the sludge is formed. Present practices in this regard involve primarily the alteration of physical properties through conditioning processes, and alteration of chemical properties through stabilization and incineration. The availability of practical approaches to segregating selected constituents of sludges could simplify current sludge managment problems. For example, basic research which would lead to an ability to selectively remove heavy metals or recalcitrant organic compounds from sludges would be extremely valuable.

SLUDGE TREATMENT PROCESSES

The purpose of sludge treatment processes is to prepare sludge for ultimate disposal or reclamation. Processes commonly used are for the purpose of removing water from sludges or for alteration or conversion of sludge properties (for example, by stabilization or combustion of organic materials).

Processes for Removing Water

Removal of water from sludges serves to reduce the required size of treatment, storage, and transportation facilities. Thickening and dewatering processes conventionally have been used for the removal of moisture from sludges, and conditioning processes have been used to alter the physical properties of sludge so as to facilitate removal of moisture. Novel processes such as ultrafiltration and electroosmosis have been tried, but do not account for a significant portion of the moisture removal facilities presently in operation.

Research on fundamental sludge properties (see previous sections) should be conducted and interpreted with appreciation for the need for improved methods for removal of water from sludges. Innovative approaches which could stem from investigations of the basic physical, chemical, and biological properties of sludges are needed.

Current research needs related to gravity thickening include improved understanding of basic mechanisms involved in gravity and flotation thickening of compressible slurries, elaboration and improvement techniques for characterizing thickening properties of suspensions in a way which links fundamental thickening behavior to process design and operation.

Sludge conditioning is perhaps the process for which the least amount of fundamental information is available. As presently practiced, sludge conditioning by chemical, physical, or biological means is an art. From basic research results on fundamental properties of sludges should come an understanding of the mechanisms involved in sludge conditioning.

Fundamental research is needed which would lead to description of conditioning requirements for a particular sludge and allow intelligent selection of specific conditioning techniques or conditioning chemicals. Capability for selectively controlling particular sludge properties (like floc density, compressibility, or permeability) to improve performance of particular thickening or dewatering facilities is needed.

Dewatering as conventionally practiced involves imposing a pressure gradient on sludge by one means or another to cause flow of water through the porous media contributed by the solid phase of the sludge. Fundamental understanding of such processes currently breaks down when the porous media is compressible (as sludge solids are). Improved understanding of flow through compressible porous media is required to permit improved design of dewatering facilities, to allow sensitive interpretation of the significance of basic physical properties of sludge, and to enable effective selection of dewatering techniques. Currently, sludge dewatering facilities are designed on the basis of prior experience, crude laboratory testing, or pilot scale operation. A means for characterizing the basic properties of sludges which determine performance of dewatering facilities could lead to development of design techniques which produce far more economical and effective dewatering installations than at present. Other basic research needs in the area of dewatering relate to the characteristics of particular classes of dewatering equipment. For example, design of a vacuum filtration installation based on understanding of fundamental factors influencing performance would require rigorous analysis of the "drop off" phenomenon, and a fundamental evaluation of the scrolling process is needed to facilitate design of centrifuges.

Conversion Processes

Conversion processes are used to change sludge properties to a more desirable form prior to ultimate disposal or reclamation. Basic research should relate both to establishment of the need for the conversion processes and to improvement of fundamental understanding of the processes themselves. Examples of research needs related to establishment of the need for conversion processes include rigorous epidemiological studies to evaluate the hazard of disease transmission associated with land disposal of sludges. In addition, the extent to which organic compounds in sludges need to be stabilized prior to application on agricultural land requires examination. Traditional practices call for stabilization of sludges to be applied to land, but it might be argued that such stabilization could be accomplished *in situ* in properly managed soil systems. Similarly, there is current interest and research on inactivation of viruses and organisms prior to land application. The perceived need for such treatment is, however, intuitive, and epidemiological studies would aid in clarifying when inactivation is, indeed, required. These are but an illustration of needs for research to rationally assess the circumstances under which specific conversion processes are required prior to specific ultimate disposal or reclamation schemes.

In spite of the common usage of anaerobic digesters for sludge stabilization, there is need for basic research to allow improved operational control of the process. Similarly, research on themophillic anaerobic digestion is needed to assess its performance characteristics, its role in efforts to minimize net energy consumption in wastewater management, and its possible advantages from the standpoint of inactivation of organisms and improvement in physical properties of sludges. Through understanding of factors controlling the performance of other sludge stabilization processes such as aerobic digestion, composting, and chemical stabilization would lead to improved capabilities for more effective and efficient process design and operation.

Changes in energy availability and cost require vast improvements in performance of sludge treatment processes which involve destruction of organic co[illegible]nds by thermal means. Many such improvements [illegible]lemented without need for basic research, but [illegible] on sludge properties and on fundamental [illegible]nfluencing performance of sludge treatment [illegible]d lead to enhanced capability for increasing [illegible] sludges introduced to incinerators. More needs [illegible] about the fate of constitutents in sludges (such as h[illegible]ls and toxic organic substances) under various [illegible]ons of combustion and pyrolysis in order that design a[illegible]ration of such facilities can be carried out to minimize environmental risks.

Just as wastewater treatment processes have tended to be developed without adequate concern for management of the residues produced, sludge treatment practices have tended to be developed without adequate concern for potential effects of recycle streams on wastewater processes. Basic research on factors influencing the quality of recycle streams from various sludge treatment processes is needed to allow evaluation of the interactions between sludge treatment and wastewater treatment processes. Design and operation of combined wastewater sludge treatment systems can be optimized only if the interactions between the various processes are fully described.

RECLAMATION

Aside from application of sludges to agricultural land, beneficial use of sludge has not been common. The desirability of reclaiming sludge constituents is clear, and improved source control and pretreatment of industrial wastewaters, increased awareness of the need for energy and resource conservation, and improved understanding of basic chemical properties of sludges could lead to more frequent reclamation in the future.

Waste biological sludges contain about forty percent protein, and clean sludges have been used as animal food supplements. Basic research to identify the opportunities and limitations associated with this practice and research on means to cleanse sludges before they are introduced to the food chain or means to extract protein is warranted.

The large variety of chemicals contained in sludges justifies exploration of possibilities for economical extraction of a variety of constituents. Some possibilities which would seem to warrant priority include recycle of chemicals used in wastewater treatment, separation of phosphorus from sludges produced in nutrient removal, reclamation of constituents which are in high concentration in industrial sludges, and extraction of sludge constituents with extremely high value. Vigilance is needed in developing reclamation schemes to ascertain that waste products produced in the process of reclamation do not seriously influence wastewater or sludge treatment processes.

ULTIMATE DISPOSAL

Sludge constituents which cannot be used must ultimately be returned to the environment. Many of the sludge problems and controversies of recent years involve ultimate disposal, and could have been avoided or minimized by the availability of basic information on sludge constituents and their behavior in air, land, and water.

A major basic research need concerns the food chain implications of sludge utilization on agricultural land. Knowledge of short term and long term interactions of heavy metals contained in sludges with soil constituents, organics in the sludge, and plants would permit more intelligent design of land application systems. Similar understanding is necessary for recalcitrant organic compounds. Related uncertainties associated with land application such as fate of viruses and pathogenic organisms also could be reduced by fundamental studies.

While much sludge is disposed of in landfills, little is known of the fate of sludge constituents in landfills. Basic research on this question should be coupled with work on improved landfill design and operation practices.

The desirability of conducting basic research on the fate of pollutants contained in sludges when they are discharged at sea is clouded by what is effectively a federal ban of future ocean disposal. Nevertheless, basic studies on fate of pollutants, food chain implication, etc. would permit rational evaluation of the advisability of the policy. Also, the attractiveness of sludge management schemes which would make use of sludge constituents in mariculture projects warrants basic research.

PLANNING, DESIGN, AND OPERATION OF INTEGRATED SLUDGE MANAGEMENT FACILITIES

"The treatment and disposal of sludge . . . are most difficult and expensive problems and, as much as any have been the child of fashion and have suffered from a lack of planning" (3). Improved planning for management of residuals from wastewater treatment is essential on international, national, regional, and local levels. These planning activities would benefit from basic research of the type described in previous sections, but, in addition, basic research on aspects of planning unique to sludge management is warranted.

In recent decades, more sludge management schemes probably have failed for social reasons rather than for technical ones. Basic behavioral studies might be useful on identifying factors which shape reaction to sludge management schemes, and, thus, enable technically feasible solutions to be implemented. Basic work on analysis of risks associated with management of sludges by various techniques is warranted to enable regulations and judgments to be founded on a more rigorous analysis than has typified past decision making processes.

Analysis of basic interactions between sludge production facilities and sludge treatment, utilization, and disposal facilities is warranted to improve the effectiveness of overall systems for wastewater management. Such analyses should include evaluation of interactions between individual sludge managment processes so as to identify efficient means for integrating individual processes into efficient overall schemes of sludge management.

Operational control of existing and future sludge management facilities could benefit in many ways from basic research results. Such benefits would stem, for example, from increased understanding of the effect of individual control variables on performance, from sludge testing procedures which are sensitive to factors influencing process performance, and, in the long term, to computerized process control procedures based on fundamental understanding of the performance of wastewater treatment and sludge management processes under dynamic conditions.

SUMMARY

Sludge management has suffered from a lack of basic research more than most areas of water pollution control. This historic lack of basic research on sludge is particularly unfortunate at the present time because of challenges to effective sludge management created by the generation of greater quantities of sludge, increased production of sludges from treatment of toxic substances, changes in energy availability and prices, and the adoption of more rigorous standards for control of environmental quality.

No attempt has been made in this brief paper to develop a comprehensive list of basic research needs concerning the residues created in wastewater managment. Instead, broad areas of desirable research have been suggested, and illustrative basic research topics have been mentioned.

Research on fundamental chemical, physical, and biological properties of sludges would be useful in developing means for control of sludge properties, for achieving improved understanding of factors influencing performance of sludge treatment processes, and for evaluating possible environmental impacts from sludge reclamation or disposal techniques. Basic research on factors influencing performance of processes used in sludge treament would enable evaluation of the interactions between processes, and allow more effective and cost effective designs. Similar rigorous research is needed to explore opportunities for reclamation of sludge constituents and to evaluate factors influencing the fate of sludge constitutents in ultimate disposal schemes to provide a basis for controlling those facilities to achieve effective management of residues without adverse environmental impact.

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DISCUSSION

P. Aarne Vesilind (Duke University) for Dr. Richard I. Dick:

Richard Dick has adequately and accurately described the dismal state-of-the-art in sludge managment, and has demonstrated that the problems associated with the treatment and disposal of this somewhat embarrassing by-product of our society has too long been ignored by researchers.

I would like to suggest that there are two basic reasons for the lack of research effort in sludge managment: a) It is not a very esoteric or romantic field of endeavor. b) The variability and complexity of sludges greatly complicate the undertaking of fundamental research.

The first reason is not too difficult to understand. A graduate student needs only experience mild ribbing about his chosen area of professional competence to make him wish he had chosen a more sophisticated area like activated carbon or rapid sand filtration.

The second reason why fundamental research in sludge management is scarce is that many potential researchers are scared off. Probably the most frustrating aspect of conducting sludge research is the variability of sludges. Not only is there a tremendous difference in physical, chemical and biological properties of sludges from different treatment plants, but the *same sludge*, given a little time, will change in character. Running controlled repetitive experiments thus becomes a true test of character.

Again, I want to reinforce the main point in Dr. Dick's paper: We simply know far too little about sludge properties and characteristics, and thus cannot apply fundamental knowledge to the very real problem of sludge treatment and disposal. In my opinion, the two main reasons for this gap in our knowledge is the seemingly unsavory nature of the needed research, and the difficulty of the problem.

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