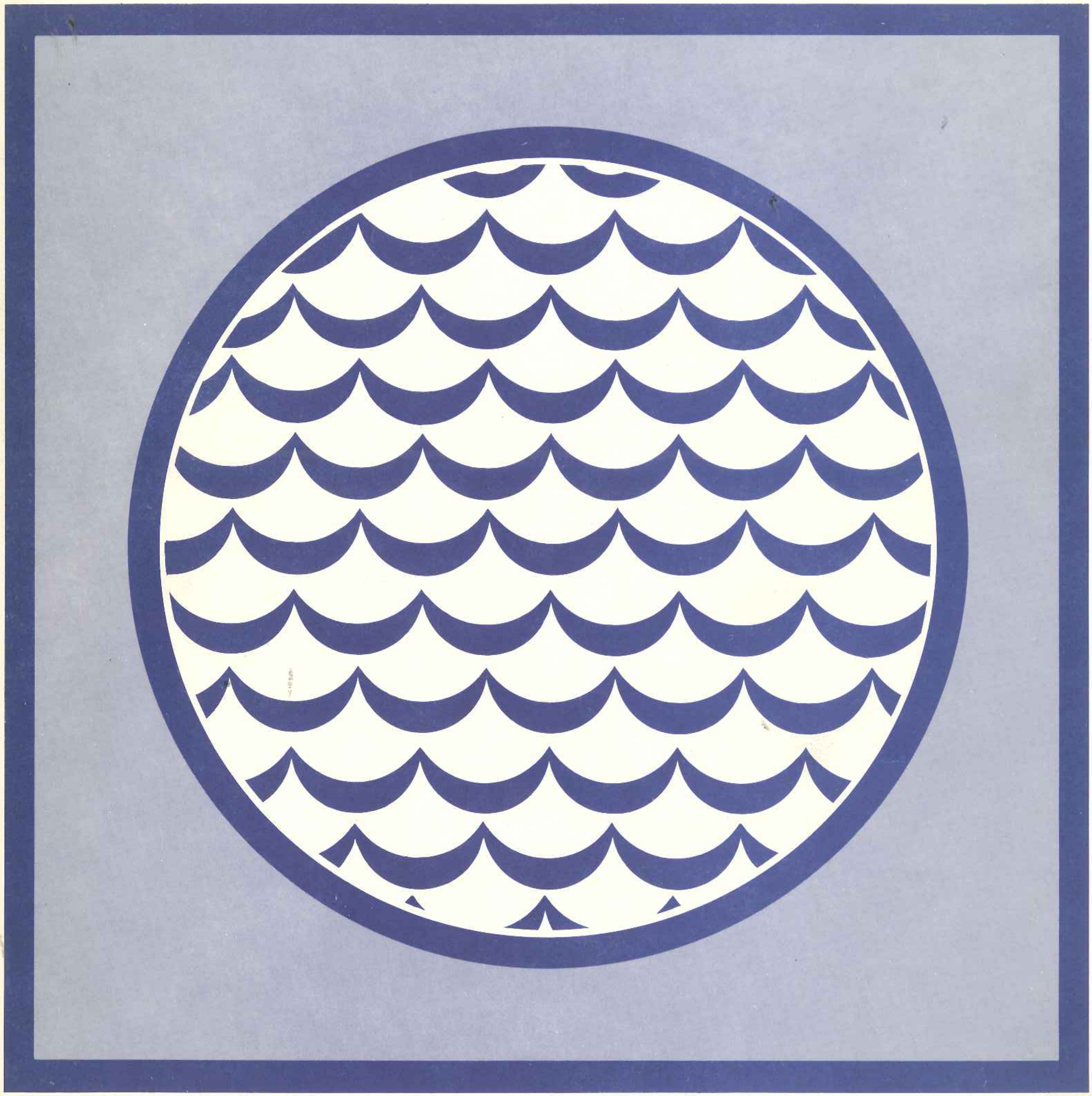
HAAS

Proceedings Of:

Fundamental Research Needs For Water And Wastewater Systems

December 1 & 2, 1982 Marriott Key Bridge Hotel Arlington, Virginia

Association Of Environmental Engineering Professors



A conference organized by *The Association of Environmental Engineering Professors*, and supported by the *National Science Foundation* in cooperation with The Office of Research and Development, *U.S. Environmental Protection Agency.*

FUNDAMENTAL RESEARCH NEEDS FOR WATER AND WASTEWATER SYSTEMS

Proceedings of the
AEEP/NSF Conference

held
December 1 & 2, 1982

at the
Marriott Key Bridge Hotel
Arlington, Virginia

A conference organized by the ASSOCIATION OF ENVIRONMENTAL ENGINEERING PROFESSORS and supported by the NATIONAL SCIENCE FOUNDATION in cooperation with the Office of Research and Development, U.S. ENVIRONMENTAL PROTECTION AGENCY.

F.A. DiGiano
N.L. Clesceri
Conference Co-Chairmen

M.S. Switzenbaum
Editor of Proceedings

EDITOR'S PREFACE

These are the proceedings of a conference organized by The Association of Environmental Engineering Professors on *Fundamental Research Needs for Water and Wastewater Treatment.* The conference was held in Arlington, Virginia, on December 1 and 2, 1982, and was sponsored by the National Science Foundation in cooperation with the Office of Research and Development, U.S. Environmental Protection Agency. A similar conference was held five years earlier.

The organizing committee for the conference consisted of Edward H. Bryan, National Science Foundation; Nicholas L. Clesceri, Rensselaer Polytechnic Institute; Francis A. DiGiano, University of North Carolina; James K. Edzwald, Clarkson College of Technology; John P. Lawler, Lawler, Matusky and Skelly Engineers; Paul V. Roberts, Stanford University; William A. Rosenkranz, U.S. Environmental Protection Agency; and F. Michael Saunders, Georgia Institute of Technology. Francis A. DiGiano and Nicholas L. Clesceri acted as Conference Co-Chairman.

The cover of these proceedings was designed by Joseph King of Joseph King and Associates of Charlotte, North Carolina. It conveys the image of "fluid" in a controlled and absolute environment—a common denominator in the context of the conference.

The editor would like to express his sincere gratitude to a number of individuals who helped in assembling this publication. These include: the authors of these papers for their cooperation; Edward H. Bryan and Francis A. DiGiano for their advice and encouragement; Peter M. Huck for his help with the Discussion sections; Gail L. Schumann for her help in proofing; and finally to Ross Scott and the rest of the staff at Word Pro Inc., of Amherst Massachusetts, who provided extensive service in the typesetting and printing of this publication.

Michael S. Switzenbaum
Editor

FOREWORD

The Nation set upon a course of action in the 1970's which was aimed at improving the quality of its water resources. After several changes in this course of action, and more likely in the near future, the pathway is still uncertain. In fact, the complexity of the problem seems to grow as more is learned about the interactive forces of economics, technology and politics. It is clear, however, that without better knowledge of the fundamentals of water and wastewater treatment technology, government decision-makers at the highest level cannot act with confidence. To turn away from the goal of improving the quality of the Nation's water resources would be a mistake. But to go forward requires that fundamental research be sustained and, in fact, nurtured so as to provide decision-makers with convincing arguments with which to support water pollution control and water supply policies.

This conference was organized to provide the opportunity for leading academic researchers and users of their findings to assess progress toward fundamental understanding and to identify critical research needs. There were over 150 attendees from universities, private research organizations, consulting firms and federal government agencies. The proceedings include papers which were presented as well as questions and comments of the audience participants and responses of the speakers.

The excellent quality of these conference papers is proof of the dedication and talent of researchers in this field. They understand the vital link between fundamental and applied research and they appreciate the importance of practical problem solving. There is great concern that over-emphasis of applied research in support of the federal regulatory effort is detrimental to the long term goals of the nation's environmental policy. For without sustaining fundamental research, new and innovative ideas for design and operation of water and wastewater treatment systems will not evolve. As noted in this conference, even conventional technology is not well understood; with greater emphasis on fundamental research vast improvements in performance may be possible. Moreover, new and complex issues such as the fate of specific organic contaminants in groundwater demand that process fundamentals be understood before putting forth environmental policy. Notwithstanding the complexity of these and other technical problems, investments in good quality research now will lead to solutions that best fit into the technological age of 1990s.

Francis A. DiGiano
Co-chairman

CONTENTS

CONTENTS

RESEARCH PRIORITIES AND FUNDING AS VIEWED BY GRANTING AGENCIES

WATER AND WASTEWATER RESEARCH IN THE '80's

Courtney Riordan
Acting Assistant Administrator for Research and Development
U.S. Environmental Protection Agency
Washington, D.C.

It is a pleasure to participate in this NSF conference on the Fundamental Research Needs in Water and Wastewater Systems.

In recent years, the national requirements and priorities of our research needs in this area have changed. Limited resources have required us to limit our research activities. No longer can we fund research proposals because the concepts "sound good. "We now have to be more sensitive to the utility of the research product and its relationship to the goals of the regulatory program. The focus of our research at EPA has already been narrowed to keep in step with the current political trends. We now have only a few specific, interrelated objectives instead of a wide range of unrelated activities.

Today, I would like to discuss three aspects of EPA's research program as they relate to our newly defined objectives. First, I will talk about some of the research that we are doing "in-house". Then, I will talk about our newly developing centers program. And finally, I will discuss our grants program.

Our "in-house" research actually refers to the money contracts which are monitored by our laboratory staff. These activities are focused on projects which will make wastewater management and the provision of safe drinking water more effective and less costly. We believe that this can be done by improving the treatment processes to increase operating efficiency, to improve reliability, and to reduce energy requirements. We are also working on new ways to make it easier to operate and maintain our existing treatment facilities.

For example, in wastewater management, you are all aware that anaerobic systems have been utilized for a long time; but, their application is limited due to process residence time and the perception that they are inherently less efficient. However, anaerobic systems for wastewater treatment are also less energy intensive (can be net energy producers), have the potential to produce less sludge, and can be far easier to maintain and operate. We believe that the improved design of our sludge digesters can be made more efficient. At the same time, we are attempting to use genetic engineering approaches to improve both our current aerobic and anaerobic systems.

In the area of water pollution control, our laboratory activity is moving away from technology based standards and it is moving toward a management approach based on local water quality rquirements. This is a familiar stomping ground for most of you, but keep in mind that cost is, in fact, the driving force behind this directional change. As the change takes place, I think we will find that consideration of toxics in wastewater and their impact or potential impact on health and the environment will be a major concern; and therefore, our scientists will be examining the capabilities of the existing treatment processes to remove toxics as well as looking at other control measures.

Ground water contamination and its control or prevention are of great concern. In the area of ground water, one of the principal concerns of recent years has been the contamination of our aquifers with industrial solvents which move at different rates through the soils. A major objective of our groundwater research program is to develop a better understanding of that movement under different conditions. In 1983, we hope to have the first generation, two-dimensional model to help us predict groundwater quality at a given point of withdrawal after after a release of contaminants into the subsurface environment. In 1984 we expect to have that model marginally field-validated; and by 1985, we plan to complete the field evaluations, to make certain improvements to the model, and to publish a user's manual.

We will also look for ways to remove existing contamination of our groundwater. We plan to examine stripping and adsorption processes for small communities at a very low cost. In 1984, we will issue a report on the applicability of predicting sorption behavior of organic chemical classes using octanol/water partition coefficient. Also in 1984, we will issue a report on the cost-effectiveness of various technology and management alternatives for *in-situ* cleanup of already polluted aquifers.

The second topic I wanted to address is our research centers. Since 1978, eight exploratory research centers have been established. Each center focuses on a specific environmental problem. For example, there is a center for advanced control technology, one on groundwater, and one on industrial waste elimination. Each center was created to address the long-term research needs of the Agency using a unique mix of multi-media and multi-disciplinary approaches. Each center is located at a major university or it is a consortium of a number of universities. Each center was chosen competitively; and each center received a five-year cooperative agreement.

The Control Technology Center was established at the University of Illinois in 1979. Its focus is on separation technology, plus contaminant detoxification and destruction as related to remedial or add-on technology associated with the control of air and water pollution and solid wastes.

The Groundwater Research is a consortium which includes the University of Oklahoma, Oklahoma State University and Rice University. The basic concept of this cooperative agreement is to bring the universities and EPA into a working partnership to identify, plan, and conduct research in the following areas:

transport fate of pollutant materials
subsurface characterization, and
methods development.

The Industrial Waste Elimination Consortium was established in 1980. It combines the unique resources of the Illinois Institute of Technology and the University of Notre Dame. The focus of their research is the fundamental aspects of industrial wastes management. In particular, they will look for new ways to reduce pollutant discharges through changes in production that will allow for in-plant recovery or containment. We hope that this particular consortium with its special industry advisory council will contribute a better understanding of the appropriate technologies to reduce the need for and/or costs of end-of-pipe control.

The third aspect of EPA's research activities that I wanted to discuss today is our grants program. The research grants program has a centralized office in headquarters which awards and manages all of the grants issued from ORD. These grants are investigator-initiated and thus are not as immediately directed toward program office research needs as are the in-house activities. We manage this by issuing a document called "Solicitation for Research Grant Proposals" which lists the needs of the programs, and hence, encourages investigators to submit proposals in the areas of need.

This solicitation describes the principal areas of interest and specific research needs and topics that we are willing to fund. This year's publication divides our research needs into the following five principal areas or panels (we have a panel in each area) of interest:

1. Environmental Chemistry and Physics
2. Environmental Biology
3. Environmental Health
4. Environmental Engineering
5. Environmental Measurements

Submitted proposals are reviewed by the above panels of your peers and then by the related EPA program offices. Each proposal receives a grade—just like you give your students. All grades are composited and the proposals ranked from high to low. The high ranking applications may then be funded provided funds exist in their area of activity. For example, an application concerning wastewater may receive very high panel and program grades, but if no funds exist in the wastewater area, it cannot be funded. We cannot take money out of an abundantly endorsed acid rain category to fund wastewater or visa versa.

The following are examples of Environmental Engineering proposals we funded in FY 82:

1. Characterization of Aqueous Solutions for Multicomponent Adsorption and Design Calculations - $200K (Syracuse University)
2. Dephenolization of High Strength Phenolic Wastewaters by a Catalyzed Aerobic Coupling Reaction - $150K (North Carolina State University)
3. Role of Chloramines in the Oxidation of Organic Compounds Present in Water During Disinfection - $140K (University of Minnesota)
4. Chemical Degradation of Carbamate Pesticides: Water Water Detoxification Using Reactive Ion Exchange– $140K (University of Massachusetts)
5. Digested Sludge: Particle Characteristics and Dewaterability–$77K (University of Texas)

Grants in the areas of Environmental Chemistry and Physics, Environmental Biology, and Environmental Health are also of interst to this association. Examples of such grants funded in 1982 include:

1. Empirical Model for Predicting Trihalomethane Formation in Synthetic and Natural Waters– $113K (University of Arizona)
2. Electrostatic and Hydrophobic Interactions in Virus Adsorption to Solids - $150K (University of Florida)
3. Use of Wetlands to Modify Acid Mine Drainage - $173K (West Virginia University)
4. Computer Modeling of Aquatic Microcosms–$181K (University of Washington)
5. Visual Indices of Neurotoxicity - $231K (University of Rochester)
6. Behavioral Effects of Low Level Pb Exposure $220K (University of Wisconsin)

In our last Solicitation, which was distributed in October, we are asking proposals to address the following reseach needs:

1. Studies of the reaction kinetics, by-products identification, and reaction mechanisms of drinking water and wastewater disinfection alternatives to chlorine.
2. Determination of the potential of chemicals for leaching from materials and surfaces intended for contact with drinking water, as a result of interaction of microorganisms, instability, disinfection agents, and other water treatment chemicals.
3. Fundamental studies on the surface properties of sludges and the means for controlling those properties; the relationship of molecular structure of sludge constituents to the performance of biological sludge stabilization processes; and techniques for predicting sludge thickening and dewatering performance.
4. Studies on biotechnology, including genetic engineering, to improve the efficiency of biological wastewater treatment processes for phosphorus removal, nitrification, and anaerobic sludge digestion.
5. Better methods for monitoring wastewater and drinking water treatment processes.
6. Development of new theories or concepts concerning the transport and transformation processes in surface and subsurface environments in order to predict the impact of surface conditions on groundwater systems.

In closing, I would like to reemphasize a theme which was inherent in my discussion of the center's program, namely, multidisciplinary research. I think that our environmental problems are so complex and overlapping that we must devote more energy to devising methods of encouraging the various discplines to work together. Consider the needs in successfully controlling any type of hazardous waste product. How to analyze for it in different media, how does it degrade under different conditions, how does it affect a host of different aquatic or terrestrial organisms, plants, animals, or different materials, how does it respond to conventional treatment processes under different conditions, how does it affect health using a variety of exposures, what are the costs of the various control alternatives, etc. This is indeed a very complex problem and provides enormous opportunity to utilize a multidisciplinary team approach with environmental engineering serving as the cornerstone.

ENVIRONMENTAL ENGINEERING RESEARCH SUPPORTED BY THE NATIONAL SCIENCE FOUNDATION

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

INTRODUCTION

This conference follows one held in 1977 to assist in identification of fundamental research needs on topics of interest to environmental engineers. In that year, environmental engineering research was largely supported by the National Science Foundation (NSF) through operation of two, distinctly different programs. Support of basic research was conducted through the Water Resources, Urban and Environmental Engineering Program in the Division of Engineering, one of five divisions in the Directorate for Mathematical and Physical Sciences, and Engineering. Support for applied, problem-focussed research relating to environmental engineering was provided through the Residuals Management element of the Program in Regional Environmental Systems, in the Division of Advanced Environmental Research and Technology, one of five divisions in the Directorate for Research Applications.

The activities of NSF most relevant to the interests of environmental engineers were described in the Proceedings of the 1977 Conference (1). That Conference was effective in guiding the research community over the past five years. It was appropriate that the Association of Environmental Engineering Professors again assume leadership responsibility in reassessment of research needs in the context of progress made since the time of the first conference. Because of the important mission responsibilities of the U.S. Environmental Protection Agency in matters relating to regulation, monitoring, enforcement, financing the construction of treatment works, and in conduct of research that is supportive of those mission responsibilities, the Environmental Protection Agency was significantly involved in the planning and conduct of the 1977 conference and this sequel to it.

At the time of the prior conference, 18 projects were in progress with support from the basic research program (2) and 36 were underway with support from the problem-focused program (3). In the fiscal year just ended, approximately 60 new and continuing awards were made for projects dealing with groundwater, erosion and sediment transport, hydrology and water resources, civil engineering systems, water and wastewater treatment, and the diffusion, dispersion and interactions of environmental pollutants. (See Tables 1 and 2.) It seems apparent that NSF-support of research on a strictly numerical basis has held steady over the past five years. However, the emphasis has clearly shifted from a preponderance of problem-focussed projects in 1977 to support of research that is more basic in its approach to environmental engineering. Given this shift in emphasis, it seemed appropriate to summarize some of the accomplishments of the problem-focussed programs that relate to the topics of this conference in 1982.

SELECTED ACCOMPLISHMENTS

More than half of the 36 projects cited in 1977 that were supported by the problem-focused program in Residuals Management addressed issues that related to the management of domestic wastewater and sludges derived from its treatment. The others were evenly divided into a group that addressed issues relating to management of diffuse, non-point sources of pollutants, and a group that dealt with management of selected industrial wastes.

Wastewater Treatment

An achievement of one project in this group was the revitalization of batch-treatment as a viable, innovative alternative to continuous-flow systems for treatment of wastewater which had been the preferred concept for several decades. Research at Notre Dame University (4) led to an EPA-funded demonstration project at Culver, Indiana where a conventional, continuous-flow activated sludge treatment plant was successfully modified and operated as a sequencing batch reactor system (5).

Effluent Management

A sequence of NSF-supported research projects at the University of Michigan that investigated the possible role that wetlands might play in providing the equivalent of advanced treatment for effluents from secondary treatment processes led to a full-scale experiment at Houghton Lake, Michigan (6) now in the sixth year of operation. Results of this experiment and related projects in Michigan and Florida were utilized in the formulation of engineering design criteria for general application of the concept as an alternative to capital and energy-intensive, physical-chemical processes for meeting nutrient-removal objectives of tertiary or advanced treatment systems (7).

Sludge Management

Research supported by NSF relating to management of sludges derived from treatment of domestic wastewater was summarized at the recent, Eighth United States/Japan Conference on Sewage Treatment Technology (8). This research led to sufficient characterization of the use of high-energy electrons for disinfection of sludges to permit engineering design of a large-scale system at the Miami-Dade Water and Sewer Authority's Virginia key Wastewater Treatment Plant in Florida. The initial, one-fourth of full-scale system will be operational early in 1983 with a nominal, design capacity for disinfecting 170,000 gallons of sludge per day. The operation of this initial stage is expected to provide engineering design information for the remainder of the installation. The original experimental facility at the Metropolitan District Commission's Deer Island Wastewater Treatment Plant in Boston, which was used by Massachusetts Institute of Technology and University of New Hampshire personnel in their research on the concept (9), was modified by the High Voltage Engineering Corporation to serve as the prototype for its design of the initial, experimental stage in Miami.

The sludge research program supported by NSF led to new insights into the concept of stabilization as applied to management of sludges (10) and to conceptual development of a system for management of sludges based on use of the electron-beam for disinfection, pipeline transport and direct injection into topsoil for stabilization (11). A possible variant of this concept was suggested which would substitute ocean-placement of sludge after disinfection, and dispersion to meet other environmental constraints (12).

Research at the University of Maryland supported by NSF's Program in Appropriate Technology led to a full-scale experiment in which 35,000 bricks were manufactured from a mixture of clay, shale and sludge (13)(14). Results of this experiment were sufficiently encouraging to result in a cooperative project between the Washington Suburban Sanitary Commission and Maryland Clay Products, Inc. of Laurel in which 500,000 bricks were produced in September of 1982 (15). The Commission plans to specify the use of bricks produced in construction of a new electrical and mechanical building at its Parkway Treatment Plant, source of the sludge used, as well as in other storm and sanitary drainage construction projects within its jurisdiction.

Industrial Wastewater Management

The large-scale, experimental use of cooling water for conduct of aquaculture, which was investigated at the Public Service Electric and Gas Company's Mercer Generating Station in Trenton, New Jersey was successfully concluded with assumption by the Company's research affiliate of further development of the concept. This project was conducted jointly by the Public Service Electric and Gas Company, Rutgers University, Trenton State College and the Long Island Oyster Farm, Inc. The Company has acquired a commercial fish hatchery and is continuing its work toward commercialization of their concept in which heat content of the cooling water is used to accelerate the growth of fish and to extend the growing season.

Research at the University of Houston on the feasibility of eliminating discharges of pollutants from cooling towers by softening a sidestream of the blow-down water (16) provided the basis for a demonstration project at a petrochemical plant in Houston (17). The concept was also used in design of several industrial cooling tower systems, and in modified form for the Regional Utilities Board's new fossil fuel power plant in Gainesville, Florida (18).

Diffuse, Non-point Sources of Pollutants

The Chesapeake Research Consortium's study of changes in quality of Chesapeake Bay as a consequence of pollution from point and non-point sources was transferred to the U.S. Environmental Protection Agency in 1977 by a final award to the Consortium through EPA's Regional Office in Philadelphia. EPA has just concluded its five-year assessment of the Bay as a further step toward resolving the many issues faced by the region in which that body of water plays a significant role in its economy.

RECENT CHANGES AT NSF

Almost coincident with the conduct of the 1977 AEEP Conference was the beginning of a process of implementation of changes in the National Science Foundation's approach to providing support for applied research prompted and guided by a report from the Science Applications Task Force headed by Dr. John Whinnery of the University of California's School of Engineering at Berkeley. In 1981, the Directorate for Engineering was established. One of its four Divisions is the Division of Civil and Environmental Engineering, within which a Program in Water Resources and Environmental Engineering was established. In November of 1982, this program was formally divided into two new programs, viz.

- Environmental and Water Quality Engineering, and
- Hydraulics, Hydrology and Water Resources Engineering.

Both programs have as their objective to obtain a better understanding of natural phenomena and fundamental principles that underlie the empirical practices and procedures used in the practice of environmental and water resources engineering. Current emphasis in the Environmental and Water Quality Engineering Program is on quality aspects of water supply, water and wastewater treatment, and on diffusion, dispersion and interactions of pollutants as affected by physical, chemical and biological factors in the environment. The Program in Hydraulics, Hydrology and Water Resources Engineering also provides support for research on groundwater, erosion and sediment transport, and for coastal and ocean engineering.

The goal of research is to improve our understanding of relationships between effects and causes. Engineering is a process by which the properties of matter and sources of energy are made useful to people. The search for knowledge and its application to manage the survival relationship of humans to other components of the earth ecosystem may be considered as basic to the practice of environmental engineering. This search may be considered as one in which correspondence is sought between principles of nature that precisely govern physical, chemical and biological relationships with those that form the basis for social and institutional interactions. We have many unanswered questions regarding what are and what are not essential goals in the management of environmental quality, how to achieve those goals and at what cost. A strong commitment to research and an equally strong commitment to the development and demonstration of those concepts that best meet those goals is essential minimally to our wellbeing and ultimately to our survival.

REFERENCES

1. *Fundamental Research Needs for Water and Wastewater Treatment Systems*, Proceedings of a Conference, Joseph H. Sherrard, Ed., Association of Environmental Engineering Professors (December 1977).
2. Ezra, Arthur A. "Basic Research in Environmental Enginering at the National Science Foundation," in Proc. of the AEEP Conference, Ibid., pp. 8-10 (1977).
3. Bryan, Edward H. "Research Directed Toward Appropriate Management of Wastewater Treatment Plant Sludges and Pollutants of Diffuse Origin," Proc. of the AEEP Conference, Ibid., pp. 11-16 (1977).
4. Irvine, Robert L. and Arthur W. Busch, "Sequencing Batch Reactors—An Overview," an introductory paper to six related papers, pp. 235-304, Journal of the Water Pollution Control Federation, 51:2 (1979).
5. Barth, Edwin F., III, "Sequencing Batch Reactors: Doing More with Less," Newsletter of the Federal Water Quality Association (October 1982).
6. Kadlec, Robert H. and David E. Hammer, "Wetlands Utilization for Management of a Community Wastewater, 1980 Operations Summary, Houghton Lake Wetlands Treatment Project," National Technical Information Service, PB 81-235924 (1981).
7. *Aquaculture Systems for Wastewater Treatment: An Engineering Assessment*, U.S. Environmental Protection Agency, 430/9-80-006/7, MCD 67/68 (June 1980).
8. Bryan, Edward H. "Research Supported by the National Science Foundation Relating to Treatment of Wastewater and Management of Residual Sludges," in Proc. of the 8th United States/Japan Conference on Sewage Treatement Technology, U.S. Environmental Protection Agency, pp. 54-64 (October 1981).
9. Trump, John G. "High Energy Electron Treatment of Wastewater Liquid Residuals," Final Report to the National Science Foundation, National Technical Information Service, PB 81-101577 (1981).
10. Hartenstein, Roy C. "Sludge Decomposition and Stabilization," *Science*, Vol. 212, No. 4496, pp. 743-749 (May 15, 1981).
11. Bryan, Edward H. "Future Technologies of Sludge Management," in Proc. of the 1980 Spring Seminar: *Sludge Management in the Washington, D.C. Area*, National Capital Section, American Society of Civil Engineers, pp. 52-62, (May 1980). Also published in *Dangerous Properties of Industrial Materials Report*, Vol. 2, No. 3, pp. 2-7, Van Nostrand Reinhold (May/June 1982).
12. Trump, John G. "Energized Electrons Tackle Municipal Sludge," *American Scientist*, Vol. 69, No. 3, pp. 276-284 (May-June 1981).
13. Bryan, Edward H. "Appropriate Technologies for Management of Municipal and Industrial Wastes," in Proc. of the Fifth Madison Conference of Applied Research and Practice on Municipal and Industrial Waste, pp. 393-403, The University of Wisconsin-Extension (September 1982).
14. Bryan, Edward H. "Research on Innovative Concepts in Treatment of Wastewater and Management of Residual Sludges," presented at the North Carolina Water Water Works Association/Water Pollution Control Association Joint Annual Meeting, Greensboro, North Carolina (November 1982) and to be published in the Proceedings.

15. "Maryland Suburbs Take Sludge-to-Brick Experiment Into Second Phase," in *Sludge Newsletter*, Vol. 7, No. 21, p. 166 (October 15, 1982).

16. Matson, Jack V. "Cooling Water Recycle by Softening," Final Report to the National Science Foundation, National Technical Information Service, PB 80-200959 (1979).

17. Gardiner, Wendy and Jack V. Matson, "Maximum Recycle Sidestream Softening at a Petrochemical Plant," Proc. of Water Reuse Symposium II, pp. 660-674, American Water Works Research Foundation (August 1981).

18. Matson, Jack V. Personal Communication (April 21, 1980).

Prepared for presentation at the AEEP/NSF Conference on Fundamental Research Needs for Water and Wastewater Systems, December 1-2, 1982 in Washington, D.C. Dr. Edward H. Bryan is Program Director, Environmental and Water Quality Engineering, Division of Civil and Environmental Engineering. Opinions expressed are those of the author and do not reflect an official position of the National Science Foundation or any other Federal agency.

TABLE 1
List of Awards—FY 1982
Water Resources & Environmental Engineering
—Diffusion, Dispersion and Interactions of Pollutants
—Water and Wastewater Treatment

Institution	Principal Investigator	Subject of Project	Amount FY 82
Arizona State University	Higgins	Acquisition of Instrument for Determination of Total Organic Carbon in Water and Wastewater	$ 7,435
Association of Environmental Engineering Professors	DiGiano	Conference on Fundamental Research Needs for Water and Wastewater Systems, Washington, D.C., December 1–2, 1982	39,910
Bend Research, Inc.	Babcock	Wastewater Treatment with Immobilized Liquid Ion Exchangers	105,195
University of California-Berkeley	Jenkins	Use of Selective Toxicants for Control of Bulking Activated Sludge	97,908
University of California-Davis	Schroeder	Biological Denitrification of Wastewater	40,442
University of California-Los Angeles	Stenstrom	Oxygen Limited Nitrification in Wastewater Treatment	25,550
Clemson University	Grady	Mechanisms and Rate of Organic Compound Removal from Wastewater by Use of Bacteria	114,438
Colorado State University	Ward	Workshop on Research Needs Relating to Soil Absorption of Wastewater	28,171
Cornell University	Brutsaert	Surface Gas Transfer in Environmental Waters	114,318
Cornell University	Dick	Rheological Characteristics of Biological Wastewater Suspensions	64,250
Cornell University	Loehr	Acquisition of Instrument for Determination of the Fate and Transformations of Elements in Soil and Waste Treatment Systems	6,240
Cornell University	Philpot	Experimental Verification of an Irradiance Transfer Model for Assessment of Water Quality	47,839
Drexel University	Gurol	Kinetics and Mechanisms of Ozonation of Substituted Phenols in Wastewater	48,000
Drexel University	Hasit	A Stochastic Approach to Processing Sludges Derived from Treatment of	48,000
Energy & Environmental Engineering, Inc.	Porter	Laser Induced Destruction of Toxic Chemicals Contained in Wastewaters	30,000
University of Illinois-Urbana	Ibbs	Effective Procedures for Engineering Specification of Water and Wastewater Treatment Processes	48,000
University of Illinois-Urbana	Valocchi	Validity of the Local Chemical Equilibrium Assumption for Modeling Solute Transport through Heterogeneous Aquifers	48,000
University of Illinois-Urbana	Snoeyink	Effects of Activated Carbon on Reactions of Chlorine-Containing Disinfectants with Organic Compounds	88,288
John Hopkins University	O'Melia	Coagulation of Suspended Particles in Lakes	95,696
Michigan Technological University	Crittenden	Mathematical Modeling of Fixed Bed Adsorption Systems for Water Treatment	38,810
University of Michigan	Weber	Determination of Mass Transport Parameters Used in Engineering Design of Adsorption Systems for Water and Wastewater Treatment	75,912
University of Minnesota	Gulliver	Experimental Investigation of Water-Surface Gas Exchange	48,000
NSF Office of Government and Public Programs	Bartlett	Film Distribution: Wetlands, Our Natural Partners in Wastewater Management	517
NSF Requisition	Born	Purchase of Conference Proceedings: Research Needs For: A Basic Science of the System of Humanity and Nature and Appropriate Technology for the Future	1,000
University of New Hampshire	Weber	Formation, Stability and Transport of Organometallic Compounds in Natural Waters	47,817
New York University Medical Center	Young	Biodegradation of Aromatic Hydrocarbons under Reductive Anaerobic Conditions	78,121
University of North Carolina	Johnson	Ultraviolet Radiation Dose Determination in Disinfection of Water and Wastewater	66,672

University of North Carolina	Wu	Modeling Methodology for Dual-Function Stormwater Detention Basins	42,289
University of Notre Dame	Irvine	Structured Models for Biological Waste Treatment Systems	143,849
Pennsylvania State University	Unz	The Isolation and Characterization of Filamentous Microorganisms from Bulking Activated Bulking Sludge	134,295
Rice University	Andrews	Computer Control of the Activated Sludge Process	78,087
Environmental Research and Technology, Inc., Concord, Massachusetts	Shanahan	Travel to Attend International Institute for Applied Systems Analysis Conference on Eutrophication of Shallow Lakes: Modeling, Monitoring and Management; Veszprem, Hungary; August 29–September 3, 1982	801
Stanford University	Leckie	Soil/Fly Ash Leachate Interactions: Release, Transport, and Retention of Trace Elements	104,650
Stanford University	McCarty	Formation and Significance of Brominated Polyethoxy Alkyl-phenyl Ethers Formed during Chlorination of Water	111,435
University of Texas-Austin	Holley	Transport of Effluents in Rivers, Phase II	61,895
Tracer Technologies, Inc.	Walsh	Electrochemical Oxidation of Polychlorinated Biphenyls	29,906
West Virginia University	Jenkins	Purchase of Total Carbon Analyzer	15,000
United Engineering Trustees, Inc.	Cole	Engineering Foundation Conference on Urban Stormwater Detention	5,000
University of Wisconsin-Milwaukee	Christensen	Algal Growth under Multiple Toxicant Limiting Conditions	77,269
Geological & Natural History Survey-Madison, Wisconsin	Zaporozee	Travel to Attend International Symposium Impact of Agricultural Activities on Ground Water at the 16th Congress of International Association of Hydrologists: Prague, Czechoslovakia; September 5–11, 1982	1,600
		Total of Awards	$2,260,195

TABLE 2
List of Awards—FY 1982
Water Resources & Environmental Engineering
—Groundwater —Erosion & Sediment Transport —Hydrology & Water Resources
—Coastal & Ocean Engineering —CE Systems

Institution	Principal Investigator	Subject of Project	Amount FY 82
1. Groundwater			
Univ. of Mass.	Hillel	Field Testing of Mathematical Models Describing Soil-Water-Solute Dynamics	$ 167,333
Univ. of Calif/Riverside	Sposito	Fundamental Studies on Subsurface Transport Theory	40,007
Univ. of Arizona	Bhattacharya	Fundamental Studies on Subsurface Transport Theory	18,500
Youngstown State	Khan	Inverse Problem in Groundwater	31,813
Princeton University	Gray	Investigation into the Physical-Chemical Aspects of Solution Mining	104,625
Univ. of Miss.	Gupta	Fundamental Studies on Subsurface Transport Theory	20,517
Cornell Univ.	Liggett	Boundary Integral Solutions to Groundwater Problems	79,239
2. Erosion & Sediment Transport			
Calif. Inst. of Tech.	Brooks	Sediment Transport Mechanics in Alluvial Streams	98,556
Univ. of Iowa	Kennedy	Characteristic Times & Rates of Non-Equilibrium River Processes	60,000
George Wash. Univ.	Mahmood	Prototype Research on Alluvial Channels in Irrigation Canals & Medium Sized Rivers	122,756
Univ. of Minnesota	Parker	Theoretical River Mechanics	47,942
U. of Calif.-Berkeley	Van Ingen	Mechanics of Sediment Bed Forms	47,135
Univ. of Pittsburgh	Chiu	Three Dimensional Flow in Alluvial	179,706
Mass. Inst. of Tech.	Hemond	Microbial Production of Nitrous Oxide in Nitrogen Rich River Sediments	116,457
San Diego State	Ponce	Outflow Hydrographs during Earth Dam Breaches	48,879
3. Hydrology and Water Resource			
Mass. Inst. of Tech.	Krzystofowicz	Methodological Foundation for Performance & Accountability Evaluations of Water Resource Systems	94,480
Colo. State Univ.	Salas	Stochastic Modeling of Geophysical Time Series	62,265
Princeton Univ.	Pinder	Simulation of Multi Phase Flow Phenomena via Catastrophe Theory	63,381
Cal Tech	List	Mechanics of Turbulent Buoyant Plumes	126,988
Utah State	Tullis	Evaluating Scaling Criteria for Vortex Modeling	46,544
Old Dominion Univ.	Chaudhry	Analysis & Stability Oscillatons in a Closed Surge Tank	29,215
UCLA	Yeh	Optimization of Regional Water Supplies for Optimal Planning and Management	50,000
MIT	Harleman	Turbulent Mixing & Entrainment in Doubly-Diffusive Systems	74,000
United Eng. Trustees	Haimes	Conference on Multi-Objective Analysis in Water Resources	5,000
Univ. of Michigan	Wylie	Two Problems in Modeling Transient Flows	44,998
Univ. of Arizona	Duckstein	Modern Stability & Numerical Concepts in Water Resource Management	85,345
Univ. of Texas	Maidment	Effect of Rainfall on Daily Municipal Water Use	47,999
Univ. of Illinois	Alavian	Three Dimensional Density Currents on a Sloping Surface	48,000
Stanford Univ.	Dantzig	Optimization of Large Scale Dynamic Systems with Special Reference to Energy & Water Resources	85,000
Stanford Univ.	Franzini	Mixing Processes in the Simulation Modeling of Irregularly Shaped Water Bodies	110,435

Princeton	Gray	A Two-Dimensional Finite Element Model for Surface Flow Simulation	34,383
Univ. of Delaware	Huang	Chemical Interactions between Complexed Heavy Metals & Hydrous Solids: The Effects of Complex Formation on Interfacial Reactions	103,744
Geo. Wash. Univ.	Yevjevich	Validity Range of New Free Surface Unsteady Flow Equations with Bottom Curvatures	25,005
Colo State	Shen	The Physical Basis of Surface Water Hydrology with Emphasis on Hillslope Hydrology and Drainage Network Developments	20,148
4. Coastal & Ocean Engineering			
Univ. of Hawaii	Lee	Shallow Water Ocean Engineering (Supplement) Workshop	3,100 34,000
Univ. of Wash.	Rattray	Baroclinic Response of Wide Deep Estuaries to Offshore Atmospheric Forcing	18,397
Univ. of Delaware	Garvine	Theoretical Dynamics of Buoyant Plumes in Coastal Waters	21,885
Univ. of Miami	LeMehaute	Establishment of Mathematical Modeling of Shoreline Evolution	44,577
Oregon State	Nath	Hydrodynamic Roughness of Marine Growths on Cylinders	57,961
Cal Tech	Raichlen	Kinematic Properties of Breaking Waves & Their Interaction with Bottom Pavements	103,070
MIT	Landahl	The Role of Small Scales in the Generation of Water Waves by Wind	72,908
U.S. Naval Postgraduate School	Sarpkaya	Yaw and Current Effects on the Hydrodynamic Resistance of Smooth and Rough Cylinders in Harmonic Flow	43,000
Univ. of Delaware	Dalrymple	Nearshore Circulation & Coastal Processes	85,291
Univ. of Florida	Partheniades	18th International Conference on Coastal Engineering, Capetown, So. Africa	2,397
5. Civil Engineering Systems (related to Environmental Engineering)			
Princeton	Wood	Global Stability Analyses for Environmental Systems	96,406
National Academy of Sciences	Goslin	Symposium on Adequacy and Maintenance of Urban Public Facilities	30,000
National Academy of Science	Parker	Barriers to Efficient Management of Urban Water Supplies	10,000
		Total of Awards	$2,963,387

ASSESSING THE IMPACT OF FUNDAMENTAL RESEARCH ON THE PRACTICE OF ENVIRONMENTAL ENGINEERING

Conference on Fundamental Research Needs for Water and Wastewater Systems

ASSESSING THE IMPACT OF FUNDAMENTAL RESEARCH ON THE PRACTICE OF ENVIRONMENTAL ENGINEERING: PRESENT AND FUTURE, WITH A FOCUS ON WATER TREATMENT

Walter J. Weber, Jr.
Department of Civil Engineering
The University of Michigan
Ann Arbor, MI

PERSPECTIVE

This panel has been charged with assessing the impact of fundamental research on environmental engineering practice; my specific charge relates to water treatment. Impacts are commonly manifest in change. I submit there have been major changes in environmental engineering practice over the past twenty-five years—a period which bounds my personal experience—and that we are likely to witness changes of equivalent order in the years immediately ahead. Indeed, I would begin with the premise that the practice of engineering is inherently transitional. Change may be signaled as new problems emerge and become more fully characterized, as methodologies for problem resolution are developed and refined, and as those practicing the art and science of engineering develop more sophisticated perceptions and philosophies of problem identification and solution. Accepting this, the issue at hand becomes one of determining the contribution of fundamental research to each of these three major elements of change.

The first two are subject to easy account, for they comprise the mantle of engineering practice. It is more difficult to detail the last, the magma from which practice ultimately derives. Despite its central import, this element is the most subtle for, not lending itself to ready quantification, it is seldom used as a gauge of change or even recognized as an essential feature thereof.

In addressing the charge before us I should like to consider each element of change in turn, beginning with the last because, as with fine wines, one should first address the more subtle, that which is least likely to fatigue the palate.

PERCEPTIONS AND PHILOSOPHIES

The Latin noun *ingenium*, root of the word engineer, connotes an inventive mind, a quality of genius. William James defined genius as "little more than the faculty of perceiving in an unhabitual way. "It is this quality that is most subtly yet fundamentally important to beneficial change in environmental engineering practice. New problems emerge whether or not we have the capacity to characterize and quantify them, and solutions, as temporal and empirical as they may be, are usually precipitated by practical necessity, whether or not we are capable of refining and generalizing them.

Thomas Edison neither discovered electricity nor first observed incandescence. Henry Ford neither invented the internal combustion engine nor first used it to power an automobile. Rather, in each case, and in many others which could serve as equally illustrative examples, an ingenious individual perceived a problem in an unhabitual way and conceived a unique and general solution.

Thomas Edison and Henry Ford may have been possessed of superior natural intellect, but each developed the faculty of genius only through iterative research and problem resolution. At least this is my hypothesis. Accident or curiosity may have brought them to the problems they addressed, and vision and perception to their solutions. I submit, however, that each had transformed and honed intellect into vision and perception through the labors—both successes and failures—of exploration and research. I have included failure as a part of the intellect honing process, for the Organizing Committee asked us to consider both successes and failures in evaluating the impacts of fundamental research. It is my philosophy and belief that well-designed research does not fail. An hypothesis may be supported or not supported by the evidence, but information, properly gained, that something is not so is as important to the construct of theory as affirmation of an innovative hypothesis. Failure, when it occurs, is vested in the unperceptive researcher.

Twenty years ago I was a copper-bright Ph.D. graduate, quick to profess and anxious to practice what I thought must certainly be the ultimate of knowledge, gleaned from assiduous study of everything that the best in our field could teach, and from an exhilarating first excursion into fundamental research. I recall well the kind but nonetheless firm admonition proffered at my dissertation defense by Professor Gordon M. Fair, a founder of contemporary environmental engineering precept and practice . . . "Your research" (which dealt with adsorption technologies for water reclamation and reuse . . .) "is of academic interest and acceptable quality, but you must keep in mind, Walter, that man was not meant to drink sullied water."

As Pliny the Elder believed of the nine aqueducts of Rome, *nihil magis mirandum fuisse in toto orbe terrarum*, so most environmental philosophers and practitioners of mid-century believed that public water supplies should be drawn from pristine upland catchments such as Boston's Wachusett and Quabbin reservoirs and the Croton and Catskill supplies of New York City, thus requiring minimal treatment. To the extent possible, this is sound and preferred practice in water supply engineering. The position quite understandably maintained by Professor Fair was that there was not enough known about contaminants in water, and the potential effects of these on public health, to risk short circuiting the hydrologic cycle by practicing water reuse.

As we moved into the 1960's and beyond, however, the philosophy and practice of water supply engineering underwent significant change as circumstantial water reuse became an accepted reality and dedicated reuse an increasing necessity. The ability of practicing engineers to respond as quickly and as well as they have to this emerging challenge does not derive from historical or empirical precedent; there is none. Principal credit is due fundamental research, not only because it generates methods and facts, but also because it engenders expanded perceptions and attitudes which ultimately translate into improved teaching and practice.

I maintain that fundamental research has intrinsic worth as a foundation upon which those who teach practice and those who practice the teachings can structure the ability to conceive and effect enduring solutions to the increasingly complex problems of environmental quality control. If it contributes no more than a quiet, persistent enrichment of the problem-solving philosophy and perceptive skill of environmental engineering educators, students, and practitioners, fundamental research provides a substantial return on investment to our society.

PROBLEM EMERGENCE AND CHARACTERIZATION

Having considered the last and most subtle of the three elements of change, perceptions and philosophies, I turn to the first and preaxial, the emergence and characterization of new problems. To assess how fundamental research has influenced this element of change in water treatment we must examine how our ability to characterize emergent problems has increased.

The most notable and significant contribution of fundamental research to problem recognition and characterization in water supply practice is the evolution of sophisticated means for detection of organic contaminants. It was just twenty years ago that the U.S. Public Health Service in its 1962 Revised Drinking Water Standards first advanced concern about the presence of organic chemicals in drinking water, and then only with respect to the lumped parameters of phenols, detergents, and carbon/chloroform extractables. The practical limits of recovery and analysis for these lumped parameters measured in the mg/l range.

Today we can identify and quantify hundreds of specific organic compounds at orders of magnitude lower concentration. The quality of our water sources and supplies is not likely much worse today than it was in 1962. The difference lies in the development, largely through fundamental research, of superior means for collecting, concentrating, separating, identifying, and quantifying organic contaminants.

That we have quantified organic constituents of water does not necessarily mean that we have quantified the problem of organic contamination. This remains a significant challenge in water supply engineering practice, and constitutes a problem that can be resolved only by continuing research on the behavior and fate of such chemicals in water treatment and distribution, and on their impacts on man and other consumers of water.

I am sure that each of us can suggest other examples of the manner in which research has affected our capabilities for problem identification and quantification in water treatment practice over the past two decades. I am confident, however, that in each such example we would conclude that problem evaluation is ultimately based on engineering perception; it is important that such perceptions be predicated on an understanding of problem dynamics that can be provided only through fundamental research.

METHODOLOGY DEVELOPMENT AND REFINEMENT

As problem identification is preaxial to change, so development of solution methodology is axial to engineering practice. A number of factors have converged to effect changes in methodologies in water treatment practice over the past twenty years. These include our enhanced ability to characterize contaminants of water, increased public awareness and concern as reflected in water quality legislation and regulation, increased demand for water in the face of an essentially fixed supply, and increased costs for the energy and materials used in treatment operations.

It is apparent that certain of these factors are in diametric opposition. Greater awareness and concern with levels of contamination of water would dictate a higher degree of regulation and treatment. But greater regulation and treatment, if structured on a fixed base of technology, means greater expenditures of energy and materials. Alternatively, it is possible to go further afield to secure fresher and cleaner sources, such as transporting water from the Great Lakes to regions supplied by such lesser quality sources as the lower Mississippi River, but this would involve high energy cost and place an unreasonable burden on a valuable limited resource. The role of research thus becomes one of facilitating the convergence of several divergent needs. This can be accomplished only by the development and refinement of technologies which provide higher levels of treatment at equal or reduced costs; that is, technology-efficient, cost-effective treatments. This is a difficult feat, one requiring a fundamental examination of all facets of the problem ranging from contaminant source identification and elimination—e.g., THM elimination by precursor removal or modification of disinfection practice—through the evolution of more effective and efficient removal processes—e.g., more rigorously designed and operated adsorption systems for organics removal.

One can identify few, if any, truly new water treatment technologies that have been developed over the past twenty years; rather, there have been many adaptations, modifications and refinements of existing technologies. A few examples illustrate this point: each of the variety of disinfectants we use today was available in 1962, but we now know more about the relative effectiveness of each as both bactericide and virucide; chemical coagulation is not new, but we are more aware of related mechanisms, control variables and the significance of this process in removing not only colloidal suspended impurities, but macromolecular substances such as THM precursors; the filtration process is almost as old as the water supply field, but we have learned more in the past two decades than in the preceding several hundred about mechanisms, pretreatment for optimization, and the advantages of direct filtration in certain cases; the significance of particulate and colloidal matter not only as a source of turbidity but as a potential carrier of associated organic contaminants has only begun to be recognized and quantified in the last five years; and, finally, the use of activated carbon for taste and odor removal was practiced before the turn of this century, but only in the last twenty years has there evolved a base of technology sufficient to carry this process toward optimum and routine use in water treatment for removal of a broad spectrum of hazardous trace organic compounds.

The list could go on, but the foregoing should sufficiently illustrate that the major changes which have occurred in water treatment practice in the past two decades relate to the development of a better understanding of process fundamentals; this through an active program of basic and exploratory research.

CONCLUSION

I have identified three elements of change relating to engineering practice in the water supply field, and have attempted to assess the significance of fundamental research to each of these. The needs and issues that we face throughout the arena of environmental quality control are more complex now than they have ever been. The associated complexities of problem resolution require increasingly sophisticated engineering approaches. It is my conviction that fundamental research provides the firm basis upon which the three elements, engineering perception, problem quantification, and methods development, must be structured.

Need and empiricism will ultimately precipitate solutions in the absence of research, as evidenced by the fact that water disinfection by boiling and clarity improvement by filtration were practiced in biblical times, and even charcoal addition for taste and odor control is a process of at least several-century vintage. It should be clear, however, that empirical brute force applications of such processes will not begin to approach current needs in water supply. Performance requirements are much greater than they have ever been, and consumable resources such as chemicals and energy are increasingly costly and scarce.

Virtually none of the advances in engineering practice in the water supply field over the past two decades has involved totally new processes. One might argue on this basis that fundamental research has produced no new technology over this period. I counter that research has enhanced and refined our level of sophistication regarding process mechanisms and dynamics and has provided insights which foster more imaginative and creative engineering in the development of advanced treatment systems. Were it not for fundamental research, environmental engineering would not have evolved to the necessarily more sophisticated practice it is today.

The focus of improvement in environmental engineering practice over the past several decades has been problem definition and methodology development. Future successes will depend upon our ability in the decades ahead to provide an innovative perceptual framework for problem resolution, one which stimulates development of new methods and practices. The time and space scales within which problem resolution must be achieved to sustain and advance society as we know it are rapidly compressing. If we are to respond adequately, we must continue to pursue fundamental research as a principal means for expanding our knowledge, horizons, and vision.

ASSESSING IMPACT OF FUNDAMENTAL RESEARCH ON PRACTICE OF ENVIRONMENTAL ENGINEERING—PRESENT AND FUTURE FATE OF POLLUTANTS

Donald J. O'Connor
Department of Civil Engineering
Manhattan College, NY, N.Y.

INTRODUCTION

The essential purpose of research in any field of engineering is to bridge the gap between fundamental knowledge and practical application. This element, which clearly distinguishes engineering from scientific research, requires both a thorough understanding of the relevant sciences and a sense of engineering orientation. It is particularly applicable to research in environmental engineering in view of the nature and complexity of our problems, the relative immaturity of our scientific heritage and the diversity of our professional community.

In assessing the impact of fundamental research on the practice of engineering, it is necessary to establish the criteria by which the research is evaluated. It is, therefore, appropriate to consider first the goals of fundamental research, in general, and then to define those specific attributes which characterize engineering research. Fundamental research ideally provides a deeper and more comprehensive understanding of the phenomenon under investigation. In realizing this ideal, continuity is maintained by building on previous developments and by providing avenues for further exploration The quality of such research is measured by the simplicity and generality of the theoretical concepts describing the phenomenon. In engineering, it is further characterized by the utility and applicability of these concepts in addressing real problems. The impact of research is, therefore, most effectively evaluated in time—by its fruits in theory and practice. This perspective, however, does not relieve this or any discipline of the responsibility of self analysis, which is the theme of this conference.

This discussion concerning the fate of pollutants in the environment is therefore based on the following criteria: 1) quality and orientation of the research and 2) the effectiveness with which it is transferred to practice. With respect to the first, exceptional research, either good or bad, is readily recognized. However, since the vast majority of research efforts fall in an intermediate "gray" area, formal multi-peer evaluations should be effected, and transmitted to the investigators, and perhaps to the field as a whole. One means of accomplishing this may be through publication of state of the art summaries of a more comprehensive, critical and succinct nature than as has been done in the past. The second criterion is equally important and, in view of contemporary tone of the field, more significant. The following assessment of fundamental research on the fate of pollutants is based on both criteria, with greater emphasis on the second.

A brief description of the nature of this specialty is first presented with a general assessment of research activities. An evaluation of "successes" and "failures" follows. Specific areas in which continued or additional effort is required is next presented, concluding with some general observations on the overall effectiveness of the national research programs in addressing present and future problems.

BACKGROUND AND GENERAL ASSESSMENT

A wide variety of engineering and scientific disciplines participate in the study of the fate of pollutants in the natural environment - air, water and land. Each field of specialty involves experimental investigations in both the field and the laboratory, the findings of which test existing concepts or formulate new hypotheses. While scientists and engineers in the individual specialties have contributed greatly to the understanding of natural phenomena, the greatest contributions of the environmental researcher has been in the synthesis of these concepts into an overall framework of analysis. Environmental engineers have played a major role in the formulation and development of models and have, thereby, provided the professional practitioner with invaluable tools for assessing environmental quality.

Most problems are characterized by time-variable, three-dimensional and non-linear elements, some of which have significant random components. Assuming our knowledge of the real world is complete, at least in principle, an abstraction or a "model" of the prototype is effected. The significant factors are included, some are modified and the irrelevant ones are omitted, invariably resulting in simplifications in the mathematical expression of phenomena. Many water quality models are thereby expressed in steady-state, one-dimensional linear forms of a deterministic nature. The difference between the actual conditions and the equations which are used to analyze it is an inherent feature of the modeling process.

While much progress has been made in modeling each phase of the environment, the most notable advances have been accompoished in analyses of natural water systems, to which the following discussion is primarily directed. However, many of the observations may be equally applicable to the research on the fate of pollutants in the air and the land.

1. One function of mathematical models is to increase the scientific insight and understanding of the processes occuring in natural systems. Fundamental concepts are expressed in mathematical form, which leads to the correlation and synthesis of scientific observations and data. A second function, assuming a validated model is available, is to provide a realistic, yet practical, means of analysis and projection for planning, design and management purposes.

Fundamental research has been generally successful in fulfilling the first function, but relatively unsuccessful in the answering of the second.

2. Water quality models, in most cases, must be calibrated in order to insure that the relevant factors are included and the approximations are reasonable. This process usually requires prototype data to define the various transport and reaction coefficients of the system. The calibrated model must then be checked against observations collected under different sets of system conditions, in which the coefficients, assigned in the calibrated state, are extrapolated to the validation period.

While fundamental research has provided the knowledge to formulate the appropriate mathematical equations describing natural phenomena, more remains to be done in establishing sound procedures for calibration, validation and projection. The transference of the basic research to sound practice is lacking in this regard, one in which fundamental research must contribute to a limited, yet critical, degree.

3. Water quality models have been used extensively for management and planning purposes. One of the most important applications is the determination of waste load allocations. Treatment requirements are generally predicated on a low river flow of a drought frequency. Thus, the transport and reaction coefficients must be extrapolated to conditions generally far removed from those under which prototype data were collected for calibration-validation.

That these coefficients change is a theoretically sound and empirically established fact, one which fundamental research has inadequately addressed. This is particularly relevant to the reduction in the kinetic coefficients of effluents with increasing degrees of treatment, and to changes in transport and transfer coefficients under low or drought flow conditions, when the roughness elements may be of the same order as the depth of flow. This lack of understanding is one of the most glaring deficiencies in the present state of the art.

4. There are presently available a number of water quality models, ranging from the relatively simple to extremely complex. Each is structured to address a certain class of problem. In planning applications extreme accuracy is not necessarily required, but rather trends, ranges or relative differences may be sufficient to address the water quality problem. In other cases, a greater degree of accurary is necessary, requiring a more complex model structure. The significance and nature of the problem dictate the relative simplicity or complexity of the analysis.

What is lacking is a perspective on the range of applicability and restriction of available models in addressing a specific water quality problem. The practitioner is faced with a specific problem and a variety of models to address that problem. Which does he choose? What is the basis for the selection? While answering such questions may be primarily a function of applied research, it requires a significant input from fundamental research, which has not yet been provided.

5. In many of the complex models, the number of state variables, relating to the concentration of various constituents, are outweighed by the number of the rate and transfer coefficients. Thus, there may be several combinations of values, all of which provide fits to observations. The practitioner is faced with the obvious question of which set to use for projection and planning. Based on the present scientific knowledge of aquatic systems, there are limited ranges within which these coefficients lie. The validity of the model as a realistic representation of a complex system rests on the internal consistency of the coefficients defining biological or chemical reactions, in concert with the variety of environmental conditions reflecting the hydrodynamic and hydrological features of the system.

In applying the model, the practitioner requires some perspective on the interplay and balance among the kinetic, transfer and transport routes, a perspective that fundamental research has only partially addressed.

EVALUATION OF PAST SUCCESS AND FAILURES

The preceding discussion presented some general observations about the impact of fundamental research on the practice of environmental engineering. The following focuses on some specific areas of water quality analysis. The research may be regarded as relatively successful if it provided some input into a management or planning decision. A failure of fundamental research to do so, does not necessarily imply the research is poor, but rather that the research effort has not progressed to the degree which could affect a decision. More to the point, the research has not been structured to effect a ready transference to practice. In the writer's opinion, it is the latter, coupled with the lack of overall perspective discussed in the previous section, which constitutes the majority of failures.

1. Dissolved Oxygen

Many research efforts have contributed to the success of dissolved oxygen analysis in streams and lakes—e.g., direct measurement of reaeration coefficients, the enumeration of the nitrifying bacteria, distinction between nitrogeneous and carbonaceous components, and better definition of the photosynthetic and respiratory effects of phytoplankton. These specific contributions have been incorporated in realistic and rigorous modeling frameworks. Computer programs and codes are readily available and have been routinely used for water quality planning by engineers in both governmental agencies and private practice, for water quality planning. Many of these plans have been implemented, resulting in evident improvement of water bodies throughout the country. Fundamental research, therefore, has made significant contributions in this area.

Certain inadequacies and gaps in our present state of knowledge have been identified viz. the effect of benthal conditions, the impact of distributed sources such as combined sewer overflows and urban runoff, and as noted above, the effect of treatment on reaction rates. Future effort of a fundamental nature must be directed to these areas in order to answer the next question, which is already being posed.

2. Eutrophication

The understanding of eutrophication, particularly in lakes and reservoirs, has also advanced to the point where rational analysis can be made and reasonable decisions effected. Fundamental research has contributed significantly in this area, notably with respect to the effect of nutrients, light and predation on the growth and decay of phytoplankton.

Due to the increased complexity of this problem, more uncertainty exists concerning these models. While a reasonable consensus on the analysis of dissolved oxygen exists, there does not appear to be a comparable agreement on eutrophication models, which range from the relatively simple to extremely complex. The factors mentioned above concerning dissolved oxygen also apply in this case - particularly with respect to nutrient fluxes to and from the benthal zone and particulate exchange between the water column and the bed.

3. Toxic Substances

Notable efforts have been made in evaluating the fate and effects of toxic substances. The initial results of laboratory and field work are promising. Excellent progress has been made in understanding and formulating many of the reactions and transfer routes in the water column, which have been incorporated in preliminary modeling frameworks.

Similar to the dissolved oxygen and eutrophication problem, insufficient attention has been directed to conditions in the bed, with respect to reaction and exchange mechanisms, as well as transport phenomena. While there has been notable progress in the bed transport of discrete particles in fresh water streams, little progress has been made in the transport of the cohesive particles such as clays and organic matter, which have a greater impact on the distribution of heavy metals and organic chemicals. The lack of understanding of these materials in estuarine environments and of the accumulation of materials in the bed is evident. Fundamental research involving field, laboratory and modeling is mandatory.

4. Acid Precipitation

Advances have been made in the understanding of this phenomenon in the atmosphere, to which theoretical studies, laboratory experiment and extensive field data programs have contributed. The various kinetic pathways have been identified and appropriate reaction equations have been formulated. Comparable progress has been made in the development of aerodynamic transport models, based largely on the advances made in research on related atmospheric processes. Models of the spatial and temporal distribution of acid precursors and the formation of acid products appear to be sufficiently developed for realistic, albeit preliminary, analysis.

Comparable progress has not been effected with respect to the water quality impacts. Given the nature of the problem, essentially involving the acidity, from both dry and wet deposition, and alkalinity, a natural constituent of ground and surface waters, there should be no conceptual difficulty in structuring a valid analysis of the basic kinetics. Equally and perhaps more important is an understanding of the secondary effects with respect to the mobilization of heavy metals and organic chemicals resulting from pH changes. The study of the surface, interflow and ground water components of runoff, each interacting with its characteristic media, requires the fundamental contributions of the respective specialists. The two general areas of kinetic interactions and hydrodynamic transport must then be synthesized by the cooperative efforts of all the involved disciplines. In view of the significance of the problem and the congressional and public concern, this is an area which requires and demands fundamental research. (These comments are equally applicable to hazardous waste evaluations).

5. Hydrodynamics

Of the various elements of the mass balance equation of the pollutants in natural systems, the transport component is the most fundamentally known. The ability to analyze the complex hydrodynamics of natural systems has developed markedly over the past one or two decades. Significant progress has and continues to be made, particularly in the case of two and three dimensional flow in estuarine and groundwater systems. Although there also have been notable developments in the hydrodynamic analysis of lakes and coastal zones, formidable difficulties remain.

From the water quality viewpoint, these models provide the transport element of the continuity equation for the pollutant. A commonly encountered problem is the reduction of the complexity of the hydrodynamic model for incorporation in the water quality model. The appropriate blending of the output of the hydrodynamic model with the kinetic and transfer components of the water quality model has not been adequately addressed. The problem arises primarily because of the different time and space scales of the respective models, and must be answered by a cooperative effort of the fundamental disciplines.

FUTURE NEEDS

The most productive and useful areas toward which future research efforts could be directed, are outlined briefly below. Two categories are identified to distinguish between basic and more general aspects of environmental engineering research.

BASIC NEEDS

1. *Sediment/Water and Air/Water Interfaces:* These areas are relevant to toxic, dissolved oxygen and nutrient issues, and applies to all major classifications of water bodies. Mechanisms of transfer across these interfaces and specific transfer coefficients require additional understanding and characterization. The distinction between smooth and rough surfaces provides the initial basis for the understanding of interfacial transfer at air-water interfaces. Much remains to be done, particularly with respect to the transitional zone, which occurs at commonly encountered wind velocity, and in the extrapolation of laboratory results to field conditions. Questions to be addressed at the bed interface include the "availability" of the dissolved and particulate components of sediment materials for interactions with the water column, aerobic and anaerobic reactions in the bed, diffusion and kinetics in the interstitial water, and hence long term fate. Further aspects of this issue are burial, compaction and immobilization of bed solids and associated pollutants, and resuspension and transport due to the scour of the bed load.
2. *Groundwater:* The substantial progress made in the hydrodynamic aspects of the problem is not matched by that made in the fate of pollutants n groundwater aquifiers. The need for an improved ability to address quality aspects is emphasized by current concerns with land disposal of sludges and solid wastes, particularly those classified as hazardous, and to a lesser extent the land application of wastewaters and recharge of urban stormwaters.
3. *Toxic Substances:* Although a good start has been made, continued effort is clearly necessary to improve our understanding of decay routes (chemical, biological, and via air/water and sediment/water interfaces). A long term fundamental need includes the structuring of an appropriate connection between fate of specific pollutants and their impact on the overall food chain, from benthos through fish to man. A closely related issue is the source and fate of potentially toxic substances which have significant natural sources, as opposed to wastewater discharges, such as trihalomethanes and their precursors. Calibration and validation with field data is a most important and immediate need.
4. *Acid Rain:* The importance this has acquired as an environmental issue suggests the need to provide the additional knowledge which will be required to address this problem effectively. Contributions from lake inputs via surface and subsurface inflows

need to be characterized to augment present information on inputs directly to the lake surface. Better information on loading and sources is required, as well as information on mobilization of toxic metals resulting from pH changes.

5. *Ocean Disposal of Sludges:* Data and analysis procedures to determine the fate of objectionable substances present in sludges released at ocean disposal sites, and their effect on the long-term water quality and ecology of the area, is an issue of significant importance. Efforts to protect the ocean by a total ban on ocean disposal create serious difficulties, in terms of our ability to find alternate disposal routes which are technically, environmentally and economically practical. All disposal approaches have some environmental impact; assuming ocean disposal is retained as an option, then it is important that we develop the assessment technology which will permit it to be accomplished in an environmentally sound manner.
6. *Non Point Source Impacts:* The intermittent and highly variable nature of pollutant discharges from these sources present a significant challenge to the determination of fate and environmental effects. Progress has been made both in the acquisition of appropriate data bases and in probabilistic analysis methods, which are consistent with the nature of these discharges. Additional research on probabilistic methods for characterizing fate of pollutants, and extending this into the area of biological effects, is a productive avenue to pursue.

GENERAL NEEDS

1. *Post-Audits:* Field research deals largely with experiments under controlled conditions in a highly variable environment. Adequate data is more difficult and costly to obtain than in controlled laboratory experiments. One result of decisions based on otherwise successful fate research efforts, is that environmental conditions are changed with treatment, e.g., modification of the reaction rates and elimination of solids contributing to sediment deposits. If research is to continue to contribute to sound environmental decisions, it is important that the changes effected are understood and quantified. Post-audits, seriously neglected in the past should receive much more attention.
2. *Synthesis:* If the overall success of research is related to an ability to contribute to sound decisions, then it is necessary that the important research contributions be effectively synthesized in a form that can be applied to the issue at hand. Many water quality problems require a synthesis of submodels, which address the specific components of the overall framework - viz., transport (hydrodynamics and hydrology), reactions (chemical and biological), transfer (air-water and water-bed) and inputs puts (point and distributed). Each of these is structured in accordance with the time and space scale, appropriate for the specific phenomenon under analysis. Some of the most outstanding "successes" of fundamental research relate to the analysis of these specific components. The majority of research activity has been directed to analyzing specific, specialized problems, and insufficient effort has been directed to the synthesis of these individual contributions. The engineering practitioner has neither the time nor the resources (and frequently neither the inclination nor the ability) to incorporate the necessary submodels into an internally consistent unit. The exercise of model synthesis is in large measure answered only by fundamental research.
3. *Model Conversion:* In an earlier section, it was emphasized that fate models can be utilized either to advance our understanding of a system, or for planning and decision-making. Fate models developed to advance our understanding must include all of the potentially important factors. They will tend to be more complex, and have substantial data needs for both state variables and rate coefficients. Planning applications on the other hand, must often operate on limited data (particularly in relation to basis research requirements) and may well require consideration of significantly different time and space scales to support the required decision process. Research oriented fate models (even excellent ones), will very often not be able to be transferred directly to use in the planning/decision area, with a reasonable expectation that they will be able to be applied successfully. There is a need to convert successful research models, to more simplified, less data intensive forms which can address the time and space scales which are relevant for planning/decision activities.

CONCLUSIONS

The basic premise on which the foregoing assessment was predicated, is reiterated - the goal of fundamental engineering research is to bridge the gap between theory and practice. In environmental engineering, the essential first step is the translation of the fundamental sciences to the natural environment, but equally important, is the transfer of the environmental science to engineering practice. While acknowledging the significant contributions in the fundamental aspects, this discussion has attempted to strike a balance between the evident importance of the fundamental contributions and equivalent significance of engineering applications to planning and management.

The fields of science and engineering which deal with the fate of pollutants and their effects on water quality are highly segmented - both academically and professionally - the fundamental sciences (chemistry and biology), the earth sciences (limnology, oceanography, meteorology and soil sciences), the engineering sciences (fluid mechanics and hydrodynamics) and the fields of engineering (environmental, civil and chemical). What is needed is a coordinated study and research plan of the interrelationships between and among these disciplines. One of the necessary elements for an integrated program of this nature is the participation of engineers and scientists, who have a recognized specialized area of competence and more important, are willing and able to relate that competence to other disciplines to form the broader, more complete framework of analysis.

IMPACT OF FUNDAMENTAL RESEARCH ON ANAEROBIC WASTEWATER TREATMENT

by
Perry L. McCarty
Department of Civil Engineering
Stanford University
Stanford, CA

INTRODUCTION

Fundamental research has had an enormous impact on environmental engineering. Great strides have been made over the past 30 years that are readily recognized when one compares the state of knowledge in 1952 with respect to pollutants, their impacts, and methods for their control with the state of knowledge today. Significant advances have been made from knowledge developed in biology, chemistry, physics, and the various engineering disciplines, as well as from the basic studies conducted within the environmental engineering field itself. In 1952 the existence of anthropogenic compounds that persist in the environment was not even recognized, while today a large portion of effort in the environmental field is being placed in learning how to measure them, understanding their fate and effects, and bringing about their control. It is equally obvious today that our understanding of these materials is still far too limited to make satisfactory judgments about their impacts. In addition, control of more traditional pollutants such as pathogens, biochemical oxygen demand, nitrogen, and phosphorus has become more intense, and better and more economical methods have evolved from a better basic understanding of traditional processes as well as from the development of entirely new processes.

The time available here is much too limited to present a general discussion of the impacts of fundamental research on the practice of environmental engineering as a whole. Therefore, I have chosen to take a fairly limited topic to illustrate how a process of importance in environmental engineering has developed, largely as a result of the evolution of basic knowledge generated in environmental engineering and in other disciplines. The anaerobic treatment process was chosen as this example because it is one of the oldest wastewater treatment processes used by environmental engineers, was about abandoned by the profession during the 1960s due to optimism over what might be accomplished with space-age technology, but is now seeing a resurgence, partly as a result of a recognition that resources are indeed limited. While renewed interest has been shown largely because anaerobic treatment produces rather than consumes energy, it is equally the result of a better fundamental understanding of the process that is permitting efficient treatment of relative low strength wastewaters and at exceptionally high loading rates. The development of fundamental knowledge about the chemistry, microbial ecology, nutritional needs, inhibitors, and kinetics of this process were crucial to the newer applications that are currently being made, and are likely to be made in the future on an even broader scale.

EARLY YEARS WITH ANAEROBIC TREATMENT

The year 1981 marked the 100th anniversary of the first recorded application of the anaerobic process for the treatment of wastewaters. At that time the invention by M. Louis Mouras of the "Automatic Scavenger," a forerunner of the septic tank, was hailed as "a complete solution of the problem which for centuries had been an insolent menace hurled in the face of all humanity" (1). While few today would call the septic tank a complete solution to the wastewater problem, it was an important beginning. By 1897 methane gas from anaerobic treatment was being used for heating, lighting, and power (2), thus marking the beginning of byproduct recovery that is reawakening the interest in this process today. Modifications followed with the development of the Imhoff tank in 1905 (3,4), and separate heated digestion in 1927.

While the applications of anaerobic treatment expanded, so did the understanding of fundamental microbiology. In fact, knowledge of the microbiology preceded process application. Volta in 1775 (5) showed that "combustible air" was formed from sediments in natural waters and concluded it was derived from the plant material in the sediment. Popoff in 1875 (2) reported that methane could be produced from a variety of organic materials and that 40°C was the optimum temperature for gas production. Van Senus in 1890 (2) attributed the anaerobic decomposition of a material like cellulose to the joint activities of several microorganisms. By 1900, Sohngen (6), Hoppe-Seyler (7), and Omelianski (2,5) had concluded from basic microbiological studies that methane resulted from reduction of carbon dioxide by hydrogen produced in the process and from acetate decarboxylation. These fundamental concepts are of considerable importance today in the operation and control of anaerobic processes.

APPLICATIONS TO INDUSTRIAL WASTEWATER TREATMENT

The understanding of anaerobic treatment was extended considerably during the 1920s and 1930s by A. M. Buswell and his colleagues at the Illinois Division of the State Water Survey (2,4,8). Through both pilot and laboratory studies, they demonstrated the anaerobic treatability of a wide range of industrial and agricultural residues in addition to municipal sludge, and showed the importance of the volatile organic acids as intermediates in the process. Their work led to better understanding of process control, and gave insights into the mass balance between substrate composition and methane production.

Buswell's studies are highly important in providing a foundation to the application of this process to treatment of industrial wastes, but significant application did not occur until recently. This is because of several factors. One was the lack of sufficient understanding of the nutrition of the microorganisms carrying out the process. Another was the lack of understanding of microbial kinetics as needed to develop processes that could handle dilute industrial wastes economically. The third was the availability of relatively cheap energy which, combined with other factors, made aerobic more attractive than anaerobic treatment. Fortunately, in recent years when energy prices began to rise significantly, the needed fundamental groundwork had been laid so that anaerobic treatment could be successfully applied to the treatment of many industrial wastewaters.

NUTRITIONAL NEEDS

For many years a major stumbling block to the general application of anaerobic treatment to industrial wastewaters was the lack of knowledge of nutritional needs by the bacteria involved. The need for nitrogen and phosphorus in relationship to bacterial growth was recognized in he 1950s by Sawyer (9) for aerobic processes. Bacterial yields were found by Speece and McCarty (10) in the early 1960s to be much less in anaerobic processes because most of the energy available in substrates is lost in the methane gas. This became recognized as an important advantage of anaerobic processes as it meant lower production of waste bacterial solids for disposal as well as a greater fraction of organic substrate converted to usable methane gas. Formulations for bacterial yield were developed by Heukelekian (11) and Eckenfelder (12) in the 1950s, and these concepts have been found useful for anaerobic treatment as well.

There were, however, important unrecognized nutritional needs for methanogenic microbial consortia beyond that for nitrogen and phosphorus. Speece and McCarty (10) found that the presence of relatively high concentrations of iron and some cobalt were necessary for the methane process to proceed over extended time periods. These findings provided a sufficient breakthrough to permit long-term kinetic studies of pure substrate fermentation as needed to understand the process more thoroughly. However, this by itself was not sufficient as the high volumetric rates of methane production found possible with municipal sludges could not be sustained with less complex substrates. Studies with pure cultures of methane bacteria and the relatively recent understanding of their unique characteristics is what helped to provide the final clue to nutritional requirements. Balch et al. (13) reported in 1979 from an analysis of rRNA nucleotide sequencing for methanogens that they represent a unique class, quite distinct from other typical bacteria. Many of the enzymes associated with methanogenic bacteria are also unique. Among these are Factor 420 (14), which has a high sulfur content, and Factor 430 (15), which contains nickel as an enzyme activator. These facts indicate a relatively high sulfur concentration is desirable and nickel is an essential trace nutrient in anaerobic treatment. Taking advantage of these observations has permitted the anaerobic treatment of industrial wastes at rates on the order of 10 to 50 kg COD/m^3/day (0.6 to 2.4 pounds COD/ft^3/day). The ability to anaerobically ferment at such high loadings was recognized in 1962 (16), but continuous application on a full scale could be achieved only in the last four years.

PROCESS KINETICS

Other fundamental studies on bacterial kinetics encouraged the development of reactors to take advantage of the high possible rates of digestion. The general principles upon which the kinetics of biological treatment processes in general are based can be attributed partially to Michaelis and Menten (17), who in 1913 provided the relationship between rates of enzyme reactions and substrate concentration. A similar formulation widely used today was suggested from his doctoral work by the Nobel laureate Jacques Monod (18) to relate bacterial growth rate to substrate concentration. This concept was developed further in 1956 by Herbert, Elsworth, and Telling (19) to provide useful tools for design and operation of continuous-stirred tank reactors. Before then in 1952, in the environmental engineering field, Monod's concepts were applied by Garrett and Sawyer (20) to activated sludge systems with organisms settling and recycle. Garrett can be credited with providing the fundamental understanding of the relationship between solids retention time (SRT) and process efficiency that is so widely used today for suspended growth biological treatment systems in general. This concept is particularly useful for anaerobic treatment because measurement of the methanogenic bacterial population is so difficult.

Lawrence and McCarty (21) found that the minimum generation time for the critical methane fermentation step in anaerobic treatment was about four days, which means that the SRT for a suspended growth system cannot drop below this value without washout of methanogenic bacteria and failure of the process. This is much longer than the minimum generation time for aerobic bacteria of about an hour, and represents a significant difference between the aerobic and anaerobic processes which must be dealt with in system design and operation. The slow generation time for methanogenic cultures means that start-up times of several months may be required if starting with a small bacterial seed. Loss of the anaerobic culture by chemical toxicity or through wash-out of organisms, may result in long down times for a system while a new culture is developed. Thus, building in of reliability features for anaerobic systems is much more critical than for aerobic systems.

REACTOR DESIGN

Various reactor designs began to evolve beginning in the 1950s in order to economically treat industrial wastewaters that were more dilute than municipal wastewater sludges. That first reported by Schroepfer and his associates in 1955 (22) simulated the activated sludge system and allowed hydraulic detention times of less than a day. One of the first upflow sludge blanket reactors was developed by Stander (23) in South Africa and this process has been used successfully to treat winery and fermentation wastewaters. A newer version with a unique method for separation of liquid and gas evolved from work in The Netherlands by Lettinga (24) and has now been fairly widely applied.

Other attempts to maintain bacteria within the reactor have led to the development of the anaerobic filter by Young and McCarty (25). After a long delay since first developed, this process is seeing increasing full-scale application in recent years, especially in the United States. Other reactors that have recently evolved are the anaerobic expanded-bed reactor (26), the anaerobic fluidized bed reactor (27), and the anaerobic baffled reactor (28). These newer systems permit treatment at hydraulic detention times measured in hours rather than in days. In addition, phenomenal loadings in the range of 10 to 40 kg/m^3/day and even higher are common with BOD removal efficiencies of 70 to 95 percent. The anaerobic process is now capable of treating wastewaters with BOD concentrations down to 1000 mg/1, and efforts are being made to extend this down to concentrations representative of municipal wastewater. Time does not allow a comparison of the advantages and disadvantages of the various reactors that have emerged, and indeed this is difficult to judge well at this point because the processes are just evolving and full-scale experience is not yet sufficient.

AMENABLE SUBSTRATES

Several other developments have brought us to where we are today. These involved an understanding of process chemistry and stoichiometry, a knowledge of inhibitory materials and their control, and a better knowledge of the broad range of substrates that can be treated anaerobically, given sufficient time for organism acclimation. Aromatic compounds (29) such as phenol, catechol, benzoic acid, and a range of others that only a few years ago were believed not to be amenable to anaerobic degradation are now known to be successfully treated by this process. Of significance are recent findings (30,31) that a range of halogenated aliphatic compounds such as chloroform, 1,1,1-trichloroethane, trichloroethylene, and tetrachloroethylene are mineralized under methanogenic conditions. These compounds are generally considered to be biologically refractory under aerobic conditions. While further studies are required to determine suitable conditions, the potential applications for groundwaters contaminated with these chemicals as well as to industrial wastewater treatment are apparent.

MICROBIAL ECOLOGY

A debate that has ensued over the past 15 years has been over the significance of hydrogen in the methanogenic conversion of complex organic substrates. It is now fairly well documented (32,33,34) from various lines of evidence, including co-culture research with consortia that convert simple fatty acids such as propionate and butyrate to methane, that the oxidation of complex substrates generally results in the evolution of hydrogen gas. The hydrogen partial pressure must be kept low within the process for oxidation to occur (35,36), and this is accomplished through the action of methane bacteria. This unexpected finding has great significance for the energetics of methane fermentation, and is likely to result in impacts on the overall process, especially under non-steady-state conditions. Recent commercializaton of hydrogen monitoring instruments will be valuable in helping to understand these effects. However, further fundamental research is required to evaluate the significance of hydrogen in the overall dynamics of the process, and is likely to lead to better control procedures for methane fermentation. This is a particularly exciting area for research which is likely to enhance our understanding of the process considerably.

FUTURE IMPACTS OF FUNDAMENTAL RESEARCH

Several areas were outlined in this paper indicating how fundamental research in microbiology and process engineering have led to a much better understanding of just one of the many environmental engineering processes, broadening its application to a range of industrial wastewaters as well as to the traditional treatment of municipal sludge. The pace of basic research on the ecology and biochemistry of the consortium of bacteria involved in methanogenesis has been rapid in the last few years. It can be expected to grow considerably in the near future because of the major breakthroughs in knowledge that have recently been made. The microorganisms that we have been taking for granted to treat wastewater sludges for so many years have turned out to be unique in the biological world, leading to much excitement among microbiologists. The environmental engineering profession needs to be aware of these developments so that basic knowledge obtained can be converted into better tools for design and control of the anaerobic process.

Further development of some of the promising processes that have evolved recently should lead to economical treatment of dilute wastes, perhaps to the point where domestic wastewaters will be treated first by the anaerobic process, thus reducing energy requirements and the generation of biological solids for disposal. Further understanding of process ecology and thermodynamics should allow better process control, perhaps by monitoring the production and consumption of hydrogen gas. It is possible that control of hydrogen partial pressure will permit the formation of intermediate compounds in the process that may be more valuable than the methane gas now being produced. Thus, the potential exists for creating even more valuable byproducts from organic containing wastewaters.

The discovery that the consortia of bacteria producing methane can also mineralize many halogenated compounds offers promise for use of methane fermentation to biologically destroy hazardous wastewaters. This requires increased emphasis on studies of biochemical pathways so that the relationship between molecular structure and biodegradability under aerobic and anaerobic conditions can be better understood. Some observations indicate that for certain compounds, a combination of aerobic and anaerobic treatment can result in the mineralization of a compound when this cannot be achieved by either process alone (37). Certainly, this is an area requiring more basic knowledge. The complexity of the process and its potential for producing valuable chemicals from wastes leads to speculation over the potential that genetic engineering might have for the field. We are no doubt a long way from useful application in environmental engineering, but we need to be aware of developments here since the potential is great.

One area of importance that has been touched upon quite lightly is the effect of inhibitors to the process. Recent work (38) has indicated that the methanogenic bacteria are generally hardier than once believed, and certain process modifications such as the anaerobic filter seem to lend greater protection to the microorganisms involved. Nevertheless, more needs to be learned of inhibitory substances and their control. The construction of reliable systems is essential because of the longer startup time for the anaerobic process. On the other hand, once developed the process does offer good stability, especially during periods of shut-down. It can be restarted quite readily after months with no organic additions.

While anaerobic treatment is an old environmental engineering process, fundamental research over the last few decades has extended its usefulness considerably. The potential is just being realized, and a good case for considerably more fundamental research in this area can readily be made.

This paper emphasized only one example out of many where fundamental esearch has and is having a significant impact. An equal or greater case can be made for many other areas, especially with respect to the movement, fate, and control of hazardous chemicals. In order to advance, the environmental engineering profession needs to draw on other disciplines as well as its own to gain fundamental knowledge, and to apply this knowledge to the emerging environmental problems as they present themselves. Opportunities exist for interdisciplinary teams to help solve fundamental problems, but they also exist for the single individual who is willing to seek knowledge from others or by himself and to apply it to his own field. The emerging problems are every bit or more complex than those of the past. Research is required to help ensure that development of basic knowledge precedes the need for that knowledge. In this way, the waste of resources to solve poorly defined problems that has been evidenced in recent years can be minimized. A good case for continued development and expansion of fundamental knowledge within the environmental engineering profession certainly exists.

REFERENCES

1. Moigno, A. F. (1882) "Mouras' Automatic Scavenger," Cosmos, 622 (1881) and 97 (1882), as reviewed in Minutes of Proceedings of the Institution of Civil Engineers, XLVIII, 350, London.
2. Buswell, A. M. and Hatfield, W. D. (1938?) Anaerobic Fermentations, State Water Survey, Bulletin No. 32, State of Illinois, Urbana, Illinois.
3. Metcalf, L. and Eddy, H. P. (1915) American Sewerage Practice, III. Disposal of Sewage, 1st ed., McGraw-Hill Book Company, Inc., New York.
4. Buswell, A. M. and Neave, S. L. (1930) Laboratory Studies of Sludge Digestion, State Water Survey, Bulletin No. 30, State of Illinois, Urbana, Illinois.
5. Barker, H. A. (1956) "Biological Formation of Methane" in: Bacterial Fermentations, John Wiley & Sons, Inc., New York, p. 1.
6. Sohngen, N. L. (1910) Rec. trav. chim., 29, 238.
7. Hoppe-Seyler, F. (1887) "Die Methangahrung der Essigsaure," Z. Physiol. Chem., 11, 561.
8. Buswell, A. M. and Hatfield, W. D. (1930) Studies on Two-Stage Sludge Digestion, 1928-1929, State Water Survey, Bulletin No. 29, State of Illinois, Urbana, Illinois.
9. Sawyer, C. N. (1956) "Bacterial Nutrition and Synthesis," Biological Treatment of Sewage and Industrial Wastes, I, 3, Reinhold Pub. Corp., New York.
10. Speece, R. E. and McCarty, P. L. (1964) "Nutrient Requirements and Biological Solids Accumulation in Anaerobic Digestion," Advances in Water Pollution Research, 2, Pergamon Press, London, p. 305.
11. Heukelekian, H., Orford, H. E., and Manganelli, R. (1951) Factors Affecting the Quantity of Sludge Production in the Activated Sludge Process, Sew. Ind. Wastes, 23, 945.
12. Eckenfelder, W. W., and Weston, R. F. (1956) "Kinetics of Biological Oxidation," in: J. McCabe and W. W. Eckenfelder (ed.), Biological Treatment of Sewage and Industrial Wastes, Vol. 1, Reinhold, New York, p. 18.
13. Balch, W. E. et al. (1979) "Methanogens: Reevaluation of a Unique Biological Group," Microbiological Reviews, 43, 260.
14. Eirich, L. D., Vogels, G. D., and Wolfe, R. S. (1979) "Distribution of Coenzyme F_{420} and Properties of Its Hydrolytic Fragments," Jour. of Bacter., 140, 20.
15. Diekert, G., Konheiser, V., Piechulla, K., and Thauer, R. K. (1981) "Nickel Requirement and Factor F_{430} Content of Methanogenic Bacteria," Jour. of Bacter., 148, 459.
16. McCarty, P. L. and Vath, C. A. (1962) "Volatile Acid Digestion at High Loading Rates," International Journal of Air and Water Pollution, 6, 65.
17. Michaelis and Menton (1913) Biochem. Zeit., 49, 333.
18. Monod, J. (1950) "La Technique de Culture Continue; Theorie et Applications," Ann. Inst. Pasteur, 79, 390.
19. Herbert, D., Elsworth, R., and Telling, R. C. (1956) "The Continuous Culture of Bacteria; A Theoretical and Experimental Study," J. Gen. Microbiol., 14, 601.
20. Garrett, M. T. and Sawyer, C. N. (1952) "Kinetics of Soluble BOD Removal by Activated Sludge," Proc. 7th Industrial Waste Conf., Purdue Univ., Ext. Ser., Lafayette, Indiana, 79, 51.
21. Lawrence, A. W. and McCarty, P. L. (1969) "Kinetics of Methane Fermentation in Anaerobic Treatment," Jour. Water Pollution Control Federation, 41, R1.
22. Schroepfer, G. J., Fullen, W. J., Johnson, A. S., Ziemke, N. R. and Anderson, J. J. (1955) "The Anaerobic Contact Process as Applied to Packinghouse Wastes," Sewage and Industrial Wastes, 27, 460.
23. Stander, G. J. (1966) "Water Pollution Research—A Key to Wastewater Management," Jour. Water Pollution Control Federation, 38, 774.
24. Lettinga, G., van Velsen, A. F. M., de Zeeuw, W. and Hobma, S. W. (1979) "Feasibility of the Upflow Anaerobic Sludge Blanket (UASB)-Process," Proceedings, National Conference on Environmental Engineering, American Society of Civil Engineers, New York, p. 35.
25. Young, J. C. and McCarty, P. L. (1969) "The Anaerobic Filter for Waste Treatment," Jour. Water Pollution Control Federation, 41, R160.
26. Switzenbaum, M. S. and Jewell, W. J. (1980) "Anaerobic Attached-Film Expanded-Bed Reactor Treatment," Jour. Water Pollution Control Federation, 52, 1953.
27. Jeris, J. S. (1982) "Industrial Wastewater Treatment Using Anaerobic Fluidized Bed Reactors," Proceedings, IAWPR Seminar on Anaerobic Treatment in Fixed-Film Reactors, Denmark.
28. Bachmann, A., Beard, V.. and McCarty, P. L. (1982) "Comparison of Fixed-Film Reactors with a Modified Sludge Blanket Reactor," Proceedings, First International Conf. on Fixed-Film Biological Processes, Kings Island, Ohio.
29. Healy, J. B., Jr. and Young, L. Y. (1979) "Anaerobic Biodegradation of Eleven Aromatic Compounds to Methane," Appl. Environ. Microbiol., 38, 84.
30. Bouwer, E. J., Rittmann, B. E. and McCarty, P. L. (1981) "Anaerobic Degradation of Halogenated 1- and 2-Carbon Organic Compounds," Env. Sci. & Tech., 15, 596.
31. Bouwer, E. J. and McCarty, P. L. (1983) "Transformations of 1- and 2-Carbon Halogenated Aliphatic Organic Compounds under Methanogenic Conditions," Appl. Environ. Microbial., 45, 1286.
32. Bryant, M. P., Wolin, E. A., Wolin, M. J. and Wolfe, R. S. (1967) "*Methanobacillus omelianskii*, a Symbiotic Association of Two Species of Bacteria," Archiv fur Mikrobiologie, 59, 20.
33. McInerney, M. J., Bryant, M. P. and Pfennig, N. (1979) "Anaerobic Bacterium that Degrades Fatty Acids in Syntrophic Association with Methanogens," Arch. Microbiol., 122, 129.

34. Boone, D. R. and Bryant, M. P. (1980) "Propionate-Degrading Bacterium, *Syntrophobacter wolinii* sp. nov. gen. nov., from Methanogenic Ecosystems," Applied and Environmental Microbiology, 40, 626.

35. Zehnder, A. J. E. (1978) "Ecology of Methane Formation" in: Water Pollution Microbiology, Vol. 2, John Wiley & Sons, Inc., New York, p. 349.

36. McInerney, M. J. and Bryant, M. P. (1981) "Review of Methane Fermentation Fundamentals,]" in: D. W. Wise (ed.), Fuel Gas Production from Biomass, 1, CRC Press, Inc., Boca Raton, Florida, 19.

37. Hakulinen, R., Salkinoja-Salonen, M. and Saxelin, M. L. (1981) "Purification of Kraft Bleach Effluent by an Anaerobic Fluidized Bed Reactor and an Aerobic Trickling Filter at Semitechnical Scale," Proc. TAPPI Envir. Conf., New Orleans, 197.

38. Parkin, G. F. and Speece, R. E. (1982) "Attached vs. Suspended Growth Reactors: Response to Toxic Substances," IAWPR-Seminar on Anaerobic Treatment in Fixed-Film Reactors, Denmark.

DISCUSSION

W. Katz
(William J. Katz Assoc.)

For O'Connor:

How do we build the bridge between fundamental research and the practitioner?

It seems to me that in some cases we lack technology from a practical standpoint. For example if you miss your mark by a factor of 10 you are not doing much as an engineer for your client or agency. Would you care to comment on that?

O'Connor's reply:

Bridging the gap between fundamental research and practice is one of the most important issues which we as teachers and researchers in engineering should address. Two brief recommendations: 1. Further development of short term refresher and up-dating courses for the practitioner offered by many academic institutions. 2. A policy of educating the professors in the application and practice of environmental engineering. It presently appears that the recently awarded Ph.D. moves directly to academia, without any contact with professional practice. Efforts should be made to correct this self-defeating custom.

Weber's reply:

One step in building a bridge between research and the practitioner is to ensure that each is realistically exposed to the other. In this manner the needs of the practitioner can be more fully addressed by research, while the former will eventually feel more comfortable with the latter and thus better position himself to benefit from its findings and revelations. There are a number of vehicles (journals, conferences, workshops, short-courses, etc.) which can facilitate this interaction, but one is more fundamental and effective than all the others; namely, teaching the concepts, demonstrating the values, and instilling the enthusiasm for innovative and creative thinking when the practitioner is most willing and able to absorb and is most likely to be influenced. I refer to the educational programs which produce the practitioners of our field. It is important that the mutual benefits of interaction between research and practice be appreciated, and the interaction itself begun, early in the careers of both the researcher and the practitioners.

Prof. A. Busch
(Univ. of Texas, Dallas)

Comment:

With respect to the bridge between research and practice, I would like to make one point: Let's acknowledge that clarifiers remove settleable solids, not suspended solids.

Dr. S. Bhagat
(Washington State Univ.)

For all:

I think it is clear that fundamental research is important. However, is there a place for empiricism in situations where there is a lack of fundamental understanding?

O'Connor's reply:

Yes. Furthermore, one of the goals of our research should be an examination of empirical approaches which, of necessity are many, with a view to demonstrating their validity on each. This examination should be done with the understanding that, if we as researchers can offer no more fundamental solutions, we analyze and evaluate these empiricisms and support them wherever appropriate.

Weber's reply:

There is no question that empirical approaches are valuable, even necessary, in the absence of fundamental understanding. In fact a fundamental understanding of a process or phenomenon in our field is usually predicated on repeated examination and re-evaluation of experimental observations. Stated another way, the nature of problems with which we deal so often dictates that we take deductive—as opposed to inductive-approaches to research. A first-cut deduction is usually an empiric which must eventually be refined through further observations and deductive logic to a fundamental understanding.

Dr. F. DiGiano
(Univ. of North Carolina)

For O'Connor:

My question relates to another bridge which is perhaps similar to the one between fundamental research and practice. I am referring to the relationship between fundamental research and the regulatory process which determines what the country as a whole will do about a particular problem. I would like to ask Dr. O'Connor about the current interest in a switch from a technology based standard to a water quality standard. In the past 10 years since we were there, has fundamental research, particularly on non-point sources, advanced knowledge to the state that those responsible for the regulatory process have enough confidence to suggest the change to a water quality based standard?

O'Connor's reply:

I recognize the trend in the direction of water quality based standards, one which I support. However, since the establishment of standards stems from sociological and technological expertise, those of us who are engineers, skilled more in the hydrodynamic physiochemical and mathematical areas, need the appropriate background to evaluate them. Such standards are generally independent of the particular sources, either point or distributed.

Dr. R. DeLucia
(Meta Systems, Inc.)

Comment:

I would like to elaborate on this dialogue. As I reasd the legislative issue, an argument for a technology-based standard was administrative efficiency; however, there was also a significant amount of argument as to whether or not an administrative process could be based on the kind of water quality analysis that would be necessary to support legally the water quality-based standard. That was a very significant issue in the decision. We may argue about the fact that our laws are written by lawyers and that they view things in a certain way, and they make certain sets of decisions. Although a technology-based standard is clearly not cost-effective, one cannot deny that a factor in its acceptance was the degree to which the profession had not been able to convince the people who make these other decisions that they could handle the problem in a way that would be satisfactory to the judiciary system, that would ultimately have to make some of those final decisions.

Dr. P. Roberts
(Stanford University)

For all:

We have used the words "fundamental" and "applied" a lot today. Do we have a concensus on what those words mean? Lack of a concensus may be part of the difficulty in communication between theorists and practitioners.

Weber's reply:

The terms fundamental and applied must be taken in the context of use and compared in the perspective of the user. What is defined by one as applied may be considered quite fundamental by another, depending upon problem perceptions and solution requirements. In a more general sense, however, what is fundamental by way of a problem requirement in practice defines, at least in the context of the practice, what constitutes fundamental research on that problem.

O'Connor's reply:

I agree strongly with the point raised by this question. A lack of clarity, which characterizes much of our communication in the profession as well as society in general, is a weakness in structure of transmitting what we do and in what we should do in research. I believe all our research should be applied in the sense that we address current and foreseeable questions, which necessitates in most cases fundamental approaches. With respect to the fate of pollutants in natural water systems, for which fundamental information and understanding is lacking or incomplete, in many cases, empirical approaches are not only necessary, but desirable. The fundamental researcher in engineering should not be hesitant to apply such techniques in such cases, with the evident stipulation the the limitations of the analysis be clearly stated.

THE IMPACT OF FUNDAMENTAL RESEARCH AS VIEWED BY THE USERS

THE IMPACT OF FUNDAMENTAL RESEARCH AS VIEWED BY A REGIONAL WATER UTILITY ENGINEER

Michael J. McGuire
Water Quality Engineer
The Metropolitan Water District of Southern California
La Verne, California

INTRODUCTION

In the past, the impetus for fundamental research in the water and wastewater fields was the need to solve pressing problems in the areas of public health and consumer acceptance. However, recent history suggests that the current impetus for fundamental research is actual or anticipated regulatory activity on the federal level. The recent discovery of trace contaminants in almost every major drinking-water supply in the country has focused the attention of the water utility industry on the need for additional fundamental research. While the federal research budget for drinking water has decreased in recent years, many questions raised as a result of pioneering research in the early 1970s remain to be answered. Also, federal research funds have traditionally been directed toward answering regulation-oriented questions; studies oriented toward helping water utilities solve problems have not received as high a priority. The water utility industry is just now awakening, recognizing its own needs, and developing its own set of guidelines for funding fundamental research.

The purpose of this paper is to identify the current and future impact of fundamental research on the water utility industry in general and on a large regional water utility in particular. As one of the largest water utilities in the U.S., The Metropolitan Water District of Southern California has most of the problems that beset water utilities throughout this country. Our perspective is wide in that we serve nearly 13 million people in Southern California, but our concerns are sensitive to the needs of our smallest retail agency and its customers.

TRADITIONAL WATER QUALITY PROBLEMS

Fundamental research over the past 100 years has made dramatic contributions to the understanding of traditional water quality problems. Table 1 shows, in general, that the state of the art for understanding what can be considered "macro" problems is generally good to excellent, particularly from a treatment perspective. The ratings on the matrices shown in Tables 1 and 2 are explained below:

Poor	Little or no understanding of basic concepts; some general information available from an operational viewpoint.
Fair	A beginning has been made in under standing from a mechanistic viewpoint.
Good	Good understanding of mechanisms; more information and fine tuning needed to complete the picture.
Excellent	Sufficiently complete understanding to handle known problems; this rating for a specific contaminant can drop with additional discoveries.

Common Pathogens

Many of the basic discoveries in sanitary or environmental engineering were made in or before the nineteenth century. Conventional water treatment plants for removal of common pathogenic bacteria and gross particulates (turbidity) were in place in many parts of the U.S. by 1910 (1). A key aspect of the current good-to-excellent mechanistic understanding of treatment has been the refinement of analytical methods over many decades. Fundamental understanding of the identification of common pathogenic bacteria such as *Shigella*, *Salmonella*, and *Vibrio cholerae* has evolved to the point where a fairly well accepted surrogate parameter (total coliforms) is used instead of individual analytical techniques for each pathogen.

Recent fundamental research on the total-coliform method has shed new light on such phenomena as the occurrence of false positives and false negatives and the suppression of coliform growth by noncoliforms (2). Despite the need for this useful research, scourges of waterborne diseases have been virtually eliminated by conventional sanitary engineering practices. While methods for the recovery of traditional pathogenic and indicator bacteria are relatively well established, our understanding of their removal during water treatment and disinfection is not as well developed. The mechanism(s) behind the loss of cell viability upon exposure to disinfectants is being studied in a number of laboratories. Further, much of the data on pathogen and indicator disinfection kinetics has been derived from pure cultures of "laboratory strains," which are often poor representatives of environmental isolates. Extrapolating these data to the "real" world may result in serious misjudgment of actual disinfection kinetics.

Aesthetic Problems

The other contaminants listed in Table 1 are primarily associated with aesthetic problems; however, there are some poorly understood health aspects of these substances, such as the impact of water hardness on heart disease and the possible contribution of elevated sodium levels to hypertension. An exception to the excellent rating in the treatment category for "other inorganics" is nitrate treatment. Significant ground-water resources are unavailable in Southern California because a cost-effective nitrate treatment technique is not currently available.

A good example of how recent fundamental research has changed ratings in the health-effects area concerns color: a traditional treatment technique (bleaching with chlorine) has resulted in excessively high levels of trihalomethanes and high-molecular-weight, polar, chlorinated organic compounds. Ten years ago, concerns about color removal were strictly aesthetic, but now the chief concern is with their possible and largely unknown health effects.

Metropolitan is contributing to the fundamental understanding of gross particulate removal through support of Ph.D.-level research by T.S. Tanaka, a Metropolitan employee, at the University of Southern California. Interest in this type of research grew out of the decision to build the next 100-mgd module of Robert A. Skinner Filtration Plant as a direct-filtration design. Direct filtration is a well-known example of how fundamental research has been translated into optimization of water treatment process design, with dramatic savings in capital and operating costs.

The new filtration plant under design for the Los Angeles Department of Water and Power (LADWP) is another example of the ways in which fundamental research can lead to new designs and cost savings. The LADWP plant will employ preozonation, activating a microcoagulation process followed by coagulant addition and filtration through deep, monosized media filters.

While saving money by design optimization is certainly desirable, there are many areas in the water utility field where dramatic gaps in our knowledge exist. Table 1 shows that our understanding of the overall causes of and solutions to taste and odor problems is not as good as our comprehension of other aesthetic or traditional water quality problems. Indeed, consumer complaints about off-tastes and odors in drinking water have always dominated the responses to surveys on consumer acceptance of potable water.

A fundamental understanding of sensory methods is being pursued by Metropolitan through the adaptation of techniques traditionally used in the food and fragrance industries. While treatment, particularly with activated carbon, is useful in controlling off-tastes and odors, more understanding is needed of the reasons why taste- and odor-causing substances are produced and ways in which they might be controlled in the environment before the problem reaches the treatment facility. A discussion of new trace-organics work related to tastes and odors in drinking water follows.

TRACE-CONTAMINANT WATER QUALITY PROBLEMS

Table 2 lists the areas where, in the author's estimation, the biggest gaps exist in our knowledge of the water treatment field (3). Moving out of the macro or mg/l world and into the submicro or ng/1 and μg/1 world changes the rules learned by most of the environmental engineers currently running water utilities in this country.

Microbiological Problems

The acceleration of our knowledge in the microbiological area has opened up new concerns regarding the safety of drinking water, particularly in distribution systems. New pathogens that have been identified in drinking water and linked to gastroenteritis outbreaks include *Giardia*, *Campylobacter*, and *Yersinia*. Of more serious concern is the recently confirmed link between an often fatal pneumonia-like disease and the bacterium *Legionella*. While a good start has been made in identifying these new pathogens, an understanding of their ecology and their removal by treatment is lacking. In the case of *Giardia* cysts, the recovery of the organism is poor, and fundamental research is sorely needed to improve the analytical method.

Opportunistic pathogens including *Klebsiella*, *Acinetobacter*, *Flavobacterium*, and *Pseudomonas* have been identified as the cause of a wide range of infections and related problems in people who are weakened by other medical afflictions such as severe burns. It is of particular importance that the four genera of bacteria listed above are very common in source- and treated-water supplies. These opportunistic pathogens can constitute a major fraction of the total aerobic bacterial population (as measured on M-SPC and R-2A media) (4). The health significance of these organisms is the least understood part of the overall picture. The question is, what risk does the general population face because of the presence of these organisms?

Since Metropolitan is planning to switch from free chlorine to chloramines to control its trihalomethane problem, it is greatly concerned with how the opportunistic pathogens will respond in the distribution system. Metropolitan is working with the University of California, Irvine (UCI), to establish the effect of chloramines on the significant natural bacterial population coming from a large, uncovered finished-water reservoir (5). Metropolitan's in-house research and work with UCI have demonstrated that our traditional understanding of disinfection does not necessarily apply when environmental bacteria are examined.

Viruses have not received widespread attention in drinking water, primarily because of a lack of good analytical methods for their enumeration. Until a method or methods can be developed to recover the viruses of interest, this field will remain stagnant. Water transmission of viral diseases is poorly understood.

Trace Particulates

Our fundamental understanding of particulates below 2 microns in diameter is poor, but an area of great concern is that of asbestos fibers in drinking water. While an elevation of lung and gastro-intestinal cancer due to inhalation of asbestos fibers has been demonstrated, the health effects associated with the ingestion of asbestos in drinking water are only now beginning to be understood. A conference was held by the U.S. EPA on October 13 and 14, 1982, to summarize all known information on the occurrence, fate, treatment, and health effects of asbestos ingestion. Based on preliminary information from that meeting, the health-effects data do not clearly point to a link between cancer and asbestos in drinking water; however, the U.S. EPA may recommend reducing fiber levels in water where this is feasible.

Metropolitan is funding a study of the fundamental physical and chemical characteristics of asbestos fibers at the California Institute of Technology (6). The results of this effort will help Metropolitan deal with the asbestos problem whether or not a regulation is adopted by the U.S. EPA.

Trace Organics

By far the greatest trace-contaminant concern over the past decade has been with trace organics. As a result of intensive U.S. EPA, university, and water utility studies, a great deal is known about the occurrence, sources, fate, and treatment of the relatively nonpolar fraction of organics in drinking water. The nonpolar fraction, for these purposes, is defined as those organics amenable to detection by gas chromatography/mass spectrometry (GC/MS). However, the health effects of the hundreds of compounds identified in water are either unknown or hotly debated. The debate centers almost exclusively on the possible carcinogenicity of these compounds at μg/1 levels or less. Relying on epidemiological studies and high-dosage animal feeding experiments to demonstrate possible carcinogenicity in humans is not optimal, even though these are the only techniques available at the present time.

Nonpolar trace organics provide an excellent example of how the analytical technology for a trace substance has completely outstripped the ability of toxicologists to determine actual health effects with a high degree of certainty. Since millions of dollars are being expended across the U.S. to achieve compliance with the existing trihalomethane regulation, we are obligated to assure the people of this country that these expenditures are supported by scientific data. Table 2 illustrates the weak link in the current understanding of all trace contaminants: health effects—particularly with regard to carcinogenicity.

In some cases, utilities are not waiting for definitive health-effects data or regulatory pressure to implement advanced technology for removing trace contaminants. The City of Cincinnati, Ohio, is proceeding with the design of full-scale granular activated carbon filters for trace-organic-compound reduction in Ohio River water. In the judgment of the political leadership of that city, the time has arrived to provide a barrier against the trace organics in their water supply.

Another conflict arises where the trihalomethane/cancer risk is reduced by introducing alternative disinfectants. Unknown risks from the widespread use of alternative disinfectants have received little research attention. For example, a recent journal article confirmed the poor state of our knowledge regarding the inorganic fraction of chloramines and chlorine dioxide (7). Until the health effects of these alternative disinfectants and their by-products are better understood, we continue to incur certain risks.

In a recent NAS report (8), suggested-no-adverse-response levels were established for chlorine dioxide and chloramines using a procedure which, in this author's opinion, was highly questionable. The report appeared to confuse an aesthetic limit with a health-effects limit. This type of confusion makes the utility's job of applying acceptable technology extremely difficult.

A fundamental understanding of high-molecular-weight, polar organic compounds is lacking. Besides measurements of surrogate parameters such as total organic carbon (TOC) and total organic halogen (TOX), little is known about the polar fraction except treatability. Application of broad-spectrum treatment techniques such as activated carbon and reverse osmosis can dramatically reduce the polar fraction, but only at a very high cost. The problem is that the health effects of this fraction are largely unknown, even though it accounts for 80-90 percent of the organic carbon in most drinking water.

Taste and odor organics are included in this section because the analytical techniques used to isolate and quantify nonpolar trace organics have been adapted to the measurement of organic compounds causing off-flavors in drinking water (9,10). Metropolitan has adapted the closed-loop stripping analysis (CLSA) technique, in conjunction with GC/MS analysis, to analyze earthy-musty odorants at low ng/1 levels which are below the detection limits of most human noses. This technique has enabled Metropolitan to open up an entirely new field of fundamental research on a purely aesthetic but very troublesom problem (11-14). The CLSA-GC/MS tool will also be used in a University of Southern California research project funded by Metropolitan to evaluate for the first time the removal mechanisms of specific taste and odor compounds at ng/1 levels in treatment plants (15).

Trace Metals

Recently, controversy has developed over the chronic health effects of certain trace metals. Are certain of the currently regulated trace metals—such as arsenic, selenium, and cadmium—carcinogenic? Interpretive problems with the health-effects data for these trace metals are the same as those involved with the trace-organics data discussed previously. The environmental fate of these metals is fairly well understood from an equilibrium point of view, but an understanding of the kinetics of the significant reactions for trace metals in aquatic systems has only recently been approached.

Journal Survey

To get an idea of how current research efforts fit into the two matrices shown in Tables 1 and 2, a survey of five journals that cover the drinking-water field was undertaken. The survey was not intended to be a definitive study, as resources were limited, but it was felt that the types of papers published by these five journals would be representative of the direction of current research efforts.

Table 3 shows that the emphasis of current research is on trace contaminants. Also, significant efforts are still being expended in treatment research on the traditional water quality problems. Studies of coagulation/sedimentation/filtration unit processes are still being done at a fundamental level, which is useful. However, the preponderance of studies using synthetically produced particulate suspensions should diminish in the near future so that mechanistic studies on environmental particulates can proceed.

Not surprisingly, trace organics accounted for the lion's share of published articles dealing with trace contamination. Nonpolar organics were the subject of 90 percent of the articles dealing with trace organics. Articles on drinking-water microbiology were the next most frequently found, probably because *Applied and Environmental Microbiology* was one of the five journals surveyed. Trace metals also accounted for a significant number of articles, while asbestos investigations are apparently not currently receiving widespread attention.

Of considerable interest is the fact that only four articles addressed health-effects issues. Of course, there are other journals specifically devoted to this topic, but the general lack of health-effects articles in journals relied on by the water treatment industry suggests the existence of problems in communicating the specialized nature of toxicological investigations to a nonspecialized audience. (A recent exception to this observation was the December 1982 issue of the *Journal AWWA*, the theme of which was health effects.)

SOURCE, TREATMENT, AND DISTRIBUTION SYSTEMS

In regard to a number of aspects of a water utility's source, treatment, and distribution systems, our fundamental understanding is inadequate. Source of supply is generally a well-characterized area until one considers the impact of using reclaimed wastewater as a source (16). All the trace-contaminant problems in Table 2 then become critically important.

Except for the deficiencies noted in Table 2, the treatment process is fairly well understood. Much work needs to be done on cost optimization, which is beyond the scope of this discussion. Recently, the National Academy of Sciences and the U.S. EPA have been developing criteria for chemicals used in treatment to avoid contamination by trace substances. Metropolitan has been engaged in evaluating the fate of organic polymers used in the coagulation process. We are particularly interested in the interaction between the polymers and chlorine, but difficulty in analyzing the polymers has held up advances in this area.

Microbiological, trace-particulate, trace-organic, and trace-metal problems are poorly understood in distribution systems. For example, the impact of attached growths on pipeline walls and of coliform regrowth in the distribution system has only recently been investigated (17,18).

The causes and effects of corrosion have not yet been mentioned in this discussion. There are health concerns with the products of corrosion, primarily in relation to trace metals. However, corrosion is essentially a poorly understood economic problem. The primitive state of our knowledge of corrosion mechanisms is surprising, considering the number of years it has been studied.

FUTURE RESEARCH NEEDS AND THE ROLE OF THE WATER UTILITY INDUSTRY

The overwhelmingly deficient area in our fundamental understanding of the drinking-water field is the true significance of the health effects of trace contaminants, and the largest gap in our knowledge of trace-contaminant health effects is the issue of carcinogenicity. In the author's view, there are many important research projects that deserve funding, but, given the scarcity of available resources, health-effects research should have priority fund allocation. However, it is prudent to be conservative from a regulatory point of view in regard to the potential of these trace contaminants to cause cancer as long as the costs are not totally out of line with the perceived reduction in risk to the consumer. On the basis of funding priorities, health-effects research should receive the highest level of attention.

Tables 1 and 2 show areas where additional fundamental research is currently needed. In general, those areas in the poor-to-fair categories should have higher priorities for funding than the areas showing good-to-excellent understanding. Upgrading our understanding of water treatment should take precedence over other areas, since the treatment plant is the final barrier between a potentially contaminated water supply and the consumer. Work in the distribution system is necessary to avoid a degradation in high-quality water produced by optimized treatment plants.

While these future research needs can be addressed on a national level by NSF and U.S. EPA funding, the water utility industry has a collective and individual obligation to expand fundamental understanding of critical trace-contaminant areas. Over the past two years, the American Water Works Association Research Foundation (AWWARF) has developed a funding mechanism for what that group described as applied research (19). In many areas there is no difference between AWWARF's definition of applied research and NSF's definition of fundamental research; this appears to be merely a matter of semantics. In fact, AWWARF has committed $10,000 to partially support the asbestos research initiated by Metropolitan at the California Institute of Technology. Recent estimates place AWWARF's 1983 funding for extramural research at $450,000. A larger role for AWWARF in funding necessary research is a welcome development, as it may replace some of the federal research funds which have been cut.

Metropolitan has realized the necessity for individual utilities to fund research efforts. As stated previously, Metropolitan is currently funding three outside research projects with Southern California universities at an annual cost of over $140,000. In these three cases, the direct benefits that Metropolitan derives from these projects will be shared with the rest of the water utility industry. In addition to the outside funding of research efforts, Metropolitan is actively engaged in in-house research projects directed toward microbiological, asbestos, and trace-organics problems (20-25). Metropolitan realizes that the only way to be sure that research on its problems will be accomplished is to fund the research itself. The days of depending solely on federal research funds are certainly over.

In this author's opinion, a joint effort in areas of fundamental research by water utility personnel and outside experts with research backgrounds has the greatest chance of producing useful information that can be translated into action by the utilities. A team approach by scientiests and engineers working in these difficult trace-contaminant problem areas appears most promising, since pragmatism and new ideas can cross-fertilize one another.

SUMMARY AND CONCLUSIONS

1. The state of our understanding of traditional water quality problems is very good, although more work is needed in certain areas. Except for the microbiological category, this is not a high priority for funding.
2. The higher priority for funding is in the area of health effects of trace contaminants. Trace contaminants provide the greatest challenge to the water utility industry in the 1980s.
3. After health effects, funding of treatment research for controlling trace contaminants should be given a high priority so that barriers can be set up to protect the consumer while the poorly understood health-effects issues are being resolved.
4. Water utilities sorely need priority funding to enable us to understand the microbiology of, and the trace contaminants in, our water distribution systems.

5. AWWARF is beginning to participate in the funding of drinking-water research. However, there is a need for individual utilities to work with and directly fund researchers to solve the water industry's high-priority problems.

ACKNOWLEDGMENTS

The author gratefully acknowledges the suggestions and comments made by the following people, who reviewed this paper prior to publication: A. Eugene Bowers, Dale D. Newkirk, and Edward G. Means, The Metropolitan Water District of Southern California; Jerome B. Gilbert, East Bay Municipal Utilities District; Michael C. Kavanaugh, James M. Montgomery Consulting Engineers, Inc.; Jack B. Mannion, American Water Works Association Research Foundation; and Irwin H. Suffet, Drexel University.

REFERENCES

1. Baker, M. N., *The Quest for Pure Water*, Vol. 1, American Water Works Association, Denver, Colorado, 1981.
2. Means, E. G., and Olson, B. H., "Coliform Inhibition by Bacteriocin-Like Substances in Drinking Water Distribution Systems," *Applied and Environmental Microbiology, 42*, No. 3, pp. 506-512, 1981.
3. McGuire, M. J., "The Age of Trace Contaminants," *Water Engineering & Management, 128*, p. 12, February 1981.
4. Geldreich, E. E., Nash, H. D., Reasoner, D. J., and Taylor, R. H., "The Necessity of Controlling Bacterial Populations in Potable Waters: Community Water Supply," *Journal of the American Water Works Association, 64*, pp. 596-602, September 1972.
5. Ward, N. R., Means, E. G., Olson, B. H., and Wolfe, R. L., "The Inactivation of Total Count and Selected Gram-Negative Bacteria by Inorganic Monochloramines and Dichloramines," presented at the AWWA Water Quality Technology Conference, Nashville, Tennessee, December 6, 1982.
6. Morgan, J. J., and Bales, R. C., "Chemical and Physical Behavior of Chrysotile Asbestos Particles in Water Supplies and Water Treatment Processes," research proposal to The Metropolitan Water District of Southern California, California Institute of Technology, April 1982.
7. Bull, R. J., "Health Effects of Drinking Water Disinfectants and Disinfection By-Products," *Environmental Science & Technology, 16*, p. 554A, 1982.
8. *Drinking Water and Health*, Vol. 4, National Academy of Sciences, Washington, D.C., pp. 174-178, 1982.
9. Krasner, S. W., Hwang, C. J., and McGuire, M. J., "Development of a Closed-Loop Stripping Technique for the Analysis of Taste- and Odor-Causing Substances in Drinking Water," in *Advances in the Identification and Analysis of Organic Pollutants in Water*, Vol. 2, L. H. Keith (Ed.), Ann Arbor Science Publishers, Inc., Michigan, 1981.
10. Krasner, S. W., Hwang, C. J., and McGuire, M. J., "A Standard Method for Quantification of Earthy-Musty Odorants in Water, Sediments, and Algal Cultures," presented at the First International Symposium on Off-Flavours in the Aquatic Environment, Espoo, Finland, June 15, 1982.
11. McGuire, M. J., Krasner, S. W., Hwang, C. J., and Izaguirre, G., "Closed-Loop Stripping Analysis as a Tool for Solving Taste and Odor Problems," *Journal of the American Water Works Association, 73*, No. 10, pp. 530-537, October 1981.
12. Izaguirre, G., Hwang, C. J., Krasner, S. W., and McGuire, M. J., "Geosmin and 2-Methylisoborneol from Cyanobacteria in Three Water Supply Systems," *Applied and Environmental Microbiology, 43*, No. 3, pp. 708-714, March 1982.
13. Izaguirre, G., Hwang, C. J., Krasner, S. W., and McGuire, M. J., "Production of 2-Methylisoborneol by Two Benthic Cyanophyta," presented at the First International Symposium on Off-Flavours in the Aquatic Environment, Espoo, Finland, June 16, 1982.
14. McGuire, M. J., Krasner, S. W., Hwang, C. J., Izaguirre, G., "An Early Warning System for Detecting Earthy-Musty Odors in Reservoirs," presented at the First International Symposium on Off-Flavours in the Aquatic Environment, Espoo, Finland, June 17, 1982.
15. Pirbazari, M., "Removal of Problem Taste/Odor Causing Compounds from Water," research proposal to The Metropolitan Water District of Southern California, University of Southern California, June 1982.
16. *Quality Criteria for Water Reuse*, National Academy of Sciences, Washington, D.C., 1982.
17. Ridgway, H. F., and Olson, B. H., "Scanning Electron Microscope Evidence for Bacterial Colonization of a Drinking Water Distribution System," *Applied and Environmental Microbiology, 41*, No. 1, pp. 274-287, 1981.
18. Martin, R. S., Gates, W. H., Tobin, R. S., Grantham, D., Sumarah, R., Wolfe, P., and Forestall, P., "Factors Affecting Coliform Growth in Distribution Systems," *Journal of the American Water Works Association, 74*, pp. 34-37, January 1982.
19. *Research Priorities 1981*, AWWA Research Foundation, Denver, Colorado, 1981.
20. Means, E. G., "Bacteriological Quality of Metropolitan's Distribution System," in-house report, The Metropolitan Water District of Southern California, March 1982.
21. McGuire, M. J., Bowers, A. E., and Bowers, D. A., "Optimization of Large-Scale Water Treatment Plants for Asbestos Fiber Removals," presented at the National AWWA Meeting, Miami Beach, Florida, May 20, 1982.
22. McGuire, M. J., Bowers, A. E., and Bowers, D. A., "Asbestos Analysis Case History: Surface Water Supplies in Southern California," *Journal of the American Water Works Association, 74*, pp. 471-178, September 1982.
23. Davis, M. K., and McGuire, M. J., "Surface Water Supply Trace Organics Survey, Phases I and II, Synthetic Organic Chemicals," in-house report, The Metropolitan Water District of Southern California, July 1980.
24. Shepherd, B. M., Davis, M. K., and McGuire, M. J., "Surface Water Supply Trace Organics Survey, Phase II, Maximum Trihalomethanes Potential," in-house report, The Metropolitan Water District of Southern California, February 1981.
25. McGuire, M. J., Tanaka, T. S., and Davis, M. K., "Experimental Error Estimates Associated with Pilot-Scale Investigations of Trace Organic Removals," in *Treatment of Water by Granular Activated Carbon*, M. J. McGuire and I. H. Suffet (Eds.), American Chemical Society, 1983.

TABLE 1

STATE-OF-THE-ART MATRIX FOR UNDERSTANDING TRADITIONAL WATER QUALITY PROBLEMS

Contaminant	Identification	Sources	Environmental Fate	Treatment	Health Effects
Common pathogens	Good	Excellent	Good	Good	Excellent
Gross particulates	Excellent	Excellent	Excellent	Good	Aesthetics
Color	Excellent	Excellent	Fair	Good	Aesthetics
Hardness and TDS	Excellent	Excellent	Excellent	Excellent	Aesthetics
Other inorganics	Excellent	Excellent	Good	Excellent	Aesthetics
Taste and odor	Good	Fair	Poor	Good	Aesthetics

TABLE 2

STATE-OF-THE-ART MATRIX FOR UNDERSTANDING TRACE-CONTAMINANT WATER QUALITY PROBLEMS

Contaminant	Identification	Sources	Environmental Fate	Treatment	Health Effects
Microbiological					
New pathogens	Fair/good	Fair	Fair	Good	Fair/good
Opportunistic pathogens	Good	Fair	Fair	Good	Poor
Viruses	Fair	Fair	Poor	Fair	Poor
Trace Particulates					
Asbestos	Good	Good	Poor	Fair/good	Fair
Trace Organics					
Nonpolar	Good	Good	Good	Good	Poor
Polar	Poor	Fair	Poor	Good	Poor
Taste and odor	Fair	Fair	Poor	Good	Aesthetics
Trace Metals	Excellent	Fair	Fair	Good	Poor-good

TABLE 3

JOURNAL SURVEY SUMMARY, OCTOBER 1981 TO SEPTEMBER 1982

Contaminant	Identification	Sources	Environmental Fate	Treatment	Health Effects
Traditional Problems					
Common pathogens	6	5	3	2	—
Gross particulates	—	—	—	12	—
Color	—	—	—	1	—
Hardness and TDS	—	—	—	3	—
Other inorganics	—	1	—	2	—
Taste and odor	—	2	—	—	—
Trace Problems					
Microbiological	12	8	9	8	1
Trace particulates	—	1	—	1	—
Trace organics	16	13	31	36	1
Trace metals	1	3	19	5	2

Note: Journals surveyed included: *Journal of the American Water Works Association, Applied and Environmental Microbiology, Environmental Science & Technology, ASCE Journal of the Environmental Engineering Division,* and *Water Research.*

THE IMPACT OF FUNDAMENTAL RESEARCH AS VIEWED BY THE USER

by
Cecil Lue-Hing, D.Sc., P.E.
Research and Development Department
Metropolitan Sanitary District of Greater Chicago
Chicago, IL

INTRODUCTION

In this paper, it has been asked that the questions of how fundamental research has improved the practice of environmental engineering in the past, and what may be projected for the future, from the user's standpoint, be addressed. It seems clear that any discussion of these issues must first consider what is meant by the term fundamental research. At this conference, fundamental research is defined in terms of goals. The conference brochure states that:

> "The goals of fundamental research are to improve the degree and efficiency with which pollutants are removed from water and wastewater, to find better ways to manage residuals from treatment, to improve our understanding of the fate of pollutants and to develop better decision-making tools for management of water resources."

Clearly, fundamental research for the purposes of this conference cannot be strictly basic research, nor can it be site-specific applied research. For purposes of this paper, fundamental research is defined as "research designed to gather information of universal applicability directed toward solving problems in water and wastewater treatment."

From a user's point of view, fundamental research should be directed toward the concept that the information being gathered has at least the possibility of practical application. It is possible, of course, that some fundamental research will ultimately have no practical value, but researchers should have considered whether the research could ultimately be used for solving some kind of environmental problem. Although the research should be directed toward problem solving, it is nevertheless important that the focus be on basic principles. It is very important that the environmental engineering profession continue to conduct research which explores basic principles. In this way, new concepts can be brought to bear in solving pollution related problems. We cannot rely on our past practices in environmental engineering simply because they have been workable and reliable. It should be obvious to all of us that there is constant room for improvement in our environmental control processes and we must continue to seek better concepts and principles to resolve environmental problems.

In this paper, some past fundamental research in environmental engineering will be reviewed and the values of this research to a large user, namely the Metropolitan Sanitary District of Greater Chicago (District), will be discussed. In addition, recommendations on areas for future fundamental research will be presented.

HISTORIC REVIEW—MAJOR ADVANCES IN ENVIRONMENTAL ENGINEERING

During the period of 1800 to 1850, there was a general awakening to the need for improved sanitation. The period of 1850 to 1910 can be called the era of great advancements in sanitary science because of the realization that many enteric diseases are caused by the consumption of water contaminated by pathogenic microorganisms contained in human wastes, the advances made in bacteriology to isolate and cultivate these organisms, and the formulation of codes of municipal administration in Europe. It was during this period that wastewater treatment processes such as chemical precipitation, biological contact beds, intermittent sand filtration, Imhoff tanks, trickling filters, and disinfection by chlorine were developed.

However, the period from 1911 to present marks the period that encompasses intense efforts to advance the state-of-art of technology of wastewater treatment. During this period the universally accepted activated sludge process and its various modifications were developed. The activated sludge process has a colorful history and the magnitude of its initial discovery has still not been surpassed in the wastewater treatment field (1,2). In addition to the refinements that are constantly made to the existing technologies by incorporating the knowledge acquired on a rational engineering and scientific basis, a great deal of legislative effort was also made during this period to control and regulate water pollution not only locally, but also globally.

A chronology of some of the significant achievements in the environmental engineering field, in general, and when they were applied to meet the needs of the District is given in Table 1. It can be seen from this table that the District has been expeditious in implementing new technologies such as Imhoff tanks, activated sludge, and sludge dewatering and drying facilitities as they were found to be appropriate. The upgrading of these facilities and implementation of other new or modified unit processes have been constantly made as the needs of the District's operations change.

RECENT FUNDAMENTAL RESEARCH IN ENVIRONMENTAL ENGINEERING AND ITS IMPACT UPON DISTRICT OPERATIONS

Due to its large size, the District has been involved in almost every conceivable aspect of wastewater treatment technology. Its manpower and financial resources have allowed it to be a contributor to fundamental research as well as a user of fundamental research. This would not necessarily be the case for smaller users. In recent years the District has experimented with, and implemented, a variety of wastewater and sludge treatment processes. In most cases the same chronology of events has led to the final decision, this is:

1) Identify the problem.

2) Research the problem through the use of the available scientific literature.

3) Discuss the problem with experts in the field to supplement information gained from the literature.

4) Formulate possible solutions to the problem.

5) Conduct pilot plant studies to test promising solutions.

6) Conduct large-scale field studies of the optimum process chosen in the pilot studies.

7) Implement the new concept or process, full scale, at the District.

Five to ten years can often pass in getting from point 1 to point 7.

In the following sections we will examine the basic principles and concepts which have been applied to wastewater treatment and sludge disposal, and discuss how they have been used in the context of the District's decision-making processes.

A. Nitrogen Removal

The principal chemical species containing nitrogen which are of importance in wastewater treatment are ammonia, organic, nitrite, and nitrate nitrogen. Municipal wastewaters usually have nitrogen contents ranging from 15-25 mg/L in untreated and primary settling wastes. These nitrogen compounds have become increasingly important in wastewater management programs due to the many effects that they may have in the environment. Due to these adverse effects, some regulatory agencies have promulgated limits on effluent discharges of NH_4-N.

The applicable NH_4-N discharge standards for the District were imposed by the Illinois Pollution Control Board (IPCB) in 1972. These limits restrict effluent NH_4-N concentrations to 2.5 mg/L from April through October, and 4.0 mg/L at all other times (Chapter 3. Water Pollution, Rule 406, IPCB. 1972).

In order to comply with the restriction on NH_4-N discharge imposed by the IPCB in 1972, the District conducted a series of investigations to evaluate alternative methods of nitrogen removal. The District's investigations into nitrogen removal alternatives were confined to those involving variations of conventional biological processes which could be adapted to nitrogen removal. The basic principles for these processes were developed from the observations of Sawyer (4) on the relationships of BOD_5 to NH_4-N and the ability of activated sludge to nitrify. More specifically, the work of Knowles et al. (5) on kinetic constants for nitrifiers, and Jenkins and Garrison (6) on the use of mean cell residence time for control of activated sludge laid the foundation for the experiments performed. Descriptions of processes for the control of nitrogen provided by Ehreth and Barth (7) provided process details for these investigations.

Table 2 is a listing of the several nitrogen removal experiments conducted by the District from 1972-80. Based upon the results of these studies, the District constructed two new wastewater treatment plants utilizing the two-stage activated sludge process for nitrification. The first of these, the 30 MGD Egan plant, was completed in 1975; and the 70 MGD O'Hare plant was completed in 1980. However, even during this period of new plant construction, the District was conducting single-stage nitrification pilot studies at each of the three major plants in order to investigate the potential economic and operational advantages of single-stage nitrification. The results obtained from these studies demonstrated the feasibility of the single-stage nitrification process for both the North Side and the West-

Southwest plants. Conversely, the two-stage process was determined to be more practical for the Calumet plant, which has a higher industrial input and higher influent sewage BOD, suspended solids and ammonia nitrogen concentrations.

B. Effluent Polishing

With the passage of the Federal Water Pollution Control Act Amendments of 1972 and 1977, an effluent standard of 30 mg/L suspended solids (SS) and BOD was established for secondary treatment plants in the United States. In the following years, many states adopted even more stringent effluent standards to protect water quality limited streams and lakes. In order to meet these standards many treatment plants have turned to effluent polishing in the form of SS removal techniques.

At present, granular media filtration has become the predominant process used for this effluent polishing, with microscreening and chemical coagulation also being used.

Sand filtration actually got its start in England in the mid-nineteenth century as a drinking water treatment process. It quickly moved to the United States where it became popular due to the public health need to improve drinking water quality. A classic paper by Fuller (15) described its use in Kentucky.

The mechanisms involved in the removal of suspended solids by a filter are complex and involve the physical and chemical characteristics of the wastewater and the filtration media, as well as the operating characteristics of the filter. Basic work on the flow through filters was done by Camp (16) who applied Darcy's law to the process. O'Melia and Stumm (17) summarized many researchers' work dealing with the mechanisms of filtration. Much operational testing work was done by Tchobanoglous and Eliassen (18), Baumann and Cleasby (19), and Ives (20). Early testing work on microscreens was done by Diaper (21).

The District recognized in the 1960s that secondary treatment could not be considered the ultimate treatment scheme in terms of meeting future environmental goals. This was confirmed in 1972 when the IPCB adopted effluent standards of 12 mg/L SS and 10 mg/L BOD to be met by District plants as of December 1977.

The initial District testing of tertiary treatment system for SS removal actually began in 1965 and continued through 1978. During this period, a variety of granular media filters and microscreens were evaluated on both a pilot and full scale. Table 3 presents a chronology of this testing program. The testing program was aimed at studying the filter and microscreen operating variables which affected process performance, studying various types of media to use in the filters, and verifying whether the different types of equipment can consistently meet anticipated effluent standards. As a result of this work, the District has determined that dual media sand filters are the systems of choice for upgrading its major treatment plants.

In general, an attempt was made to utilize the fundamental work available in the literature to assist in developing the on-site testing programs which were essential in the District's decision-making process.

C. Sludge Dewatering

Sludge disposal is a difficult and expensive problem for wastewater treatment plants. As the level of wastewater treatment increases, sludge quantities invariably increase also. The key to efficient and economical sludge disposal is volume reduction through the use of sludge dewatering processes.

In the early years of centralized treatment plants, sludge was simply stored in lagoons for disposal. As storage space disappeared, sludge drying beds gained in popularity. As larger treatment plants were built, especially in colder climates, land availability for drying beds became a problem. This gave impetus to the development and use of mechanical sludge dewatering devices.

The first of these to gain widespread use was the vacuum filter. Vacuum filters were installed in the United States, beginning in the 1920s, and their popularity continued into the 1970s. The District installed vacuum filters at its Calumet Plant in 1935 and at its West-Southwest Plant in 1939. The filter cake was heat dried to form a fertilizer product.

Early work on predicting vacuum filter performance led to the development of the filter leaf test (27,28) and the development of the specific resistance concept (29) for evaluating the dewaterability of sludges.

In the 1960s centrifuges began to be successfully used for sludge dewatering with the advent of the continuous flow horizontal bowl design (30). In the 1970s belt filter presses received attention (31).

In 1973 the District began to explore the possibility of mechanically dewatering its digested sludge in order to allow it to be handled as a solid for land application uses. Desk top studies of the available information indicated that vacuum filters, centrifuges, and belt filter presses had the greatest potential. Based upon this, extensive pilot scale field studies of these three types of dewatering devices were conducted at District plants from 1974-76 (32,33). The performance of each unit was evaluated with respect to cake solids, solids capture, and chemical conditioning costs. The results of these field tests indicated that horizontal bowl centrifuges would best fit the District's needs, and three of our treatment plants now have centrifuges in operation.

D. Agricultural Use of Sludge

The Greeks realized the value of manure in 900 B.C. The gardens and olive groves around Athens were treated with sewage from the city. It is believed that the farmers paid for the sewage (34). As the urban areas started to build sewerage systems in the late 1800s, sewage farming became popular.

As sewage treatment advanced, the Imhoff treatment and later the activated sludge process became accepted. These processes isolated the sewage solids which were then air dried or heat dried. The air-dried sludge was given to farmers and the heat-dried sludge was sold as a fertilizer. The District used these two methods to dispose of most of their sludge from 1920 to 1950. The demand for air-dried sludge rapidly declined partly because of the availability of high analysis nitrogen fertilizer since the 1960s. The oil shortage of the late 1970s caused the price of nitrogen fertilizer to increase to the extent that farmers returned to the use of animal manures and, to a lesser extent, sewage sludge.

In 1966, the District began to study the possibility of using liquid digested sewage sludge on cropland. Dr. T. D. Hinesly of the University of Illinois and Harza Engineering of Chicago, Illinois were retained to evaluate the details of this concept. These studies (35) resulted in a series of field demonstrations with sludge fertilization of corn and grass in Cook County, Illinois. In 1971, the District purchased land which had been strip-mined for coal and by 1973 the District owned 15,500 acres of mine spoil in Illinois.

This operation has provided the District with experience in handling liquid sludge, dewatering liquid sludge, odor management, soil and water conservation, and crop production. The experience of the District has shown that the use of municipal sludge in agriculture is a viable and environmentally acceptable sludge management alternative (36). The challenge in the future will be to develop management schemes for this alternative which can be used at an acceptable cost by municipal sludge management agencies (37).

A history of the District's agricultural uses of sludge is presented in Table 4.

IMPLEMENTATION OF FUNDAMENTAL RESEARCH

The preceding section has discussed how the District has utilized fundamental research, in the past, in improving its wastewater treatment processes. Even with the technology available, these improvements would not have been possible without federal funds for construction, and the support of the public for improving the environment through the use of tax dollars. As we move into the 1980s and beyond, the availability of tax dollars is waning, and public awareness of environmental problems is giving way to other concerns.

For these reasons, it is necessary that today's researchers consider how to get their ideas implemented. It must be recognized that especially in environmental matters, outside forces may often determine how and in what manner fundamental research resources will be used. Economics and political forces may determine whether a particular research finding is used toward solving a specific problem. Public pressure is often so strong a force in environmental matters that it may cause a particular environmental principle to be used before it is really ready for practical application, while another principle may be dropped entirely because of a negative public perception.

The researcher must be made aware that any work he does cannot be of value unless it is communicated properly to the environmental engineering profession and the public in general. It is becoming increasingly important in this age of the information explosion for researchers to communicate their information not only in professional journals but other media as well. Ideas will never be used unless they are communicated in a positive way to the user community and to the general public.

Researchers should recognize the importance of outside forces in their research efforts even before their work begins. It must be recognized that research which has a high probability of being useful to the environmental engineering profession, and will have positive economic, aesthetic, and public benefits, is much more likely to be adopted than one that does not. In environmental matters, researchers do not work in a vacuum, and it is necessary that planning of fundamental research consider the areas where there is a high probability that the results of such research will be used.

RECOMMENDATIONS FOR FUTURE FUNDAMENTAL RESEARCH FROM THE PERSPECTIVE OF THE USER

With the above thoughts concerning the implementation of fundamental research in mind, we have prepared a listing of future research needs in the wastewater treatment field from the perspective of the municipal user.

A. Energy Conservation

Due to many factors, the cost of energy has skyrocketed in the last decade and can be as much as 30 to 50 percent of the total operating budget of a wastewater treatment plant. The greatest energy use is for the operation of blowers to provide air for the activated sludge process, in which a large amount of air is wasted or inefficiently used because of manual operation, oversized blowers, or poor oxygen transfer efficiency.

Research needs to be conducted to determine:
1. Optimum DO values for nitrification.
2. Better methods of determining oxygen transfer efficiency.
3. Optimum placement of DO probes in the aeration tank.

B. By-product Energy Conversion

Research needs on this topic include:
1. Methods to improve further degradation of organic matter and gas yield by lessening the dead space in digesters.
2. Fundamental research to recover proteins and other plant nutrients with a follow-up on pilot scale research to study the viability of process schemes for by-product recovery.
3. Fundamental research to study nonmethanogenic fermentation, to recover energy in the form of hydrogen and other simple organic intermediates by the selective enrichment of hydrogen-producing bacteria.
4. Further development work on the use of fuel cells for the direct conversion of digester gas to electrical energy.

C. Improved Automation of Wastewater and Sludge Treatment Processes

Improved automation of wastewater and sludge treatment processes can lead to significant operating costs savings as well as improvements in treatment reliability. Areas for further research are as follows:

1. Development of better on-line specific ion electrodes to monitor and control plant processes. Electrodes for toxic substances such as cyanide, placed in the plant wet well could help prevent plant upsets.
2. Development of more reliable suspended solids sensors to control return sludge rates and sludge wastage.
3. Development of on-line equipment for assessing sludge dewaterability after chemical conditioning, to be coupled with the chemical feed system.

D. Improvements in Clarifier Performance and the Determination of the True Factors Affecting the Settleability of Activated Sludge

The efficiency of solids to liquid separation and sludge thickening (i.e., sedimentation) determines the overall performance of the activated sludge process. It is, therefore, important that the factors affecting the settleability of activated sludge be better understood and that clarifier performance be optimized. The following areas need further research:

1. The effect of particle size distribution on the settleability of sludges from the perspective of assessing the impact of the possible long-term accumulation of "fines" from in-line dewatering processes.
2. The mechanisms involved whereby biopolymers enhance or inhibit sedimentation.
3. Additional work on studying the hydraulic regime within clarifiers in order to reduce velocity and density gradients.

E. Developing Specific Strains of Bacteria Through Genetic Engineering to Improve Wastewater Treatment

Scientists are now capable of directly manipulating the genetic material (DNA) of living cells, thus, making it possible to transfer specific genes from the cells of one organism to those of another, and perhaps endowing them with characteristics that were previously unavailable to them. The application of applied genetic technology to the pollution control field should encourage research along the following lines:

1. Cell fusion techniques as an alternative to recombinant DNA manipulation.
2. Genetically engineered microorganisms coupled with immobilized enzyme or organism bioprocesses for the removal or biodegradation of toxic organics.
3. Genetically engineered microorganisms having the ability to oxidize NH_4 to NO_2 or NO_2 to NO_3, but having significantly high growth rates, so that nitrification facilities can be made with very low SRTs.
4. Removal of toxic heavy metals by bacteria.
5. Biological denitrification and desulfurization.

F. Better Parameters to Measure Dewatering Characteristics of Sewage Sludges

Sludge treatment and disposal remains a major problem and cost factor in the wastewater treatment field. Additional research is needed to:

1. Develop parameters indicative of dewatering characteristics for different kinds of sludges.
2. Develop better standards for the parameters that measure floc strength and settling rate characteristics of sludges in order to provide for direct

comparisons of sludges with respect to these characteristics.

3. Develop a parameter that measures the floc particle size per unit of shear stress applied, a major dewatering characteristic that is related to floc strength, settling rate, and free water binding force.

G. Better and More Efficient Methods of Sludge Dewatering

Fundamental research is needed to:

1. Develop dewatering methods capable of producing cake solids greater than 20 percent and solids capture greater than 90 percent.
2. Improve sludge conditioning methods for dewatering.
3. Improve passive systems such as drying beds, translucent roofs, and thin-film drying.

H. Efficient and Practical Procedures for Removing Metals and Toxic Organics from Sewage Sludge

Municipal and industrial sludges can contain the full range of the waste products of our society and, thus, their disposal or utilization can have environmental and/or regulatory impediments. Although industrial waste control ordinances are normally used to control inputs to a municipal system, there have been some suggestions that some substances be removed directly from the municipal sludge. Research is needed to:

1. Develop economically feasible methods for removing heavy metals from sludge.

2. Develop methods for removing toxic organics from sludge or converting them to nontoxic compounds.

3. Develop methods to immobilize toxic substances within the sludge to prevent their undesirable redistribution.

I. Better Information on the Effects of Effluents on Aquatic Biota

More and better information on the effects of effluents on aquatic biota are urgently needed so that the degree of treatment required to protect the environment can be specified. Research suggestion:

Develop a standard acute toxicity assay or in-vitro alternative whose results are repeatable, within acceptable limits, from one laboratory to the next. These tests should employ organisms from more than one taxonomic group.

J. Information on the Factors that Affect Leaching from Landfill Sites

Sludge placement in landfills has been practiced for many years. The sludge solid content has varied from 3 to 60 percent solids. The advantages of using drier sludge include: less land is needed; less odors; more options as to future land use; and less chance of groundwater contamination. Research is needed on the following topics:

1. Odor control.
2. Gas collection for reuse.
3. Treatment of drainage water.

K. Aerosols

The downwind dispersion of microbial aerosols from wastewater treatment plants has become a public health issue recently in some cities. A number of studies on this topic have been conducted, but further work is needed in the following areas:

1. Determination of the source strength of microbial aerosol emissions from aeration tanks.
2. Methods to differentiate between bacterial die-off vs. dispersion in the downwind attenuation of bacterial aerosol concentrations.
3. Improved sampling equipment for collecting microbial aerosol samples.

L. Odors

Odors emitted from sewage treatment plants can give rise to complaints and adverse public criticism. Research is needed in the following areas:

1. Identification of odor-causing compounds in ambient air samples.
2. Determination of threshold odor levels of selected compounds taken individually and as mixtures.

REFERENCES

1. Martin, A. J., *Activated Sludge Process*, McDonald and Evans, London, G. B., 1927.
2. Alleman, J. E., and T. B. S. Prakasam, "Reflections on Seven Decades of Activated Sludge History," Paper presented at the Fifty-Fifth Annual Conference Water Pollution Control Federation, St. Louis, Missouri, October 2-8, 1982.
3. Pearse, L., *Modern Sewage Disposal*, Lancaster Press, Inc., Lancaster, PA., 1938.
4. Sawyer, C. N., "Activated Sludge Oxidation, V: The Influence of Nutrition in Determining Activated Sludge Characteristics," Sewage Works Journal, *12*, No. 1, 1940.

5. Knowles, G., A. L. Downing, and M. J. Barrett, "Determination of Kinetic Constants for Nitrifying Bacteria in Mixed Culture with the Aid of an Electronic Computer," Journal of General Microbiology, *38*, 263, 1965.

6. Jenkins, D., and W. E. Garrison, "Control of Activated Sludge by Mean Cell Residence Time," Journal Water Pollution Control Federation, *40*, 11, 1904, 1968.

7. Ehreth, D. J., and E. Barth, "Control of Nitrogen in Wastewater Effluents," prepared for the United States Environmental Protection Agency (USEPA) Technology Transfer Program, March 1974.

8. Lue-Hing, C., A. W. Obayashi, D. R. Zenz, B. Washington, and B. M. Sawyer, "Nitrification of a High Ammonia Content Sludge Supernatant by Use of Rotating Discs," Metropolitan Sanitary District of Greater Chicago, Research and Development Department Report, April 1974.

9. Washington, B. and A. Obayashi, "Final Report: Calumet Nitrification Pilot Plant," Research and Development Department Report, Metropolitan Sanitary District of Greater Chicago, April 1974.

10. Washington, B., A. Obayashi, C. Lue-Hing, and D. R. Zenz, "Single Stage Nitrification Study at the West-Southwest Treatment Plant," Report No. 76-2, Research and Development Department, Metropolitan Sanitary District of Greater Chicago, November 1975.

11. Sawyer, B., A. Obayashi, and C. Lue-Hing, "Full-Scale Single Stage Nitrification Study at the North Side Sewage Treatment Plant," Report No. 75-26, Research and Development Department, Metropolitan Sanitary District of Greater Chicago, October 1975.

12. Bogusch, E., T. B. S. Prakasam, C. Lue-Hing, and D. R. Zenz, "Report on the Single Stage Biological Nitrification of Calumet Sewage," Report No. 76-8, Research and Development Department, Metropolitan Sanitary District of Greater Chicago, March 1976.

13. Lue-Hing, C., G. Richardson, B. Sawyer, and D. R. Zenz, "Determination of the Design Parameters for a 30 MGD Two-Stage Nitrification Plant," Report No. 76-18, Research and Development Department, Metropolitan Sanitary District of Greater Chicago, July 1976.

14. Lue-Hing, C., B. Washington, D. R. Zenz, A. W. Obayashi, and T. B. S. Prakasam, "Full-Scale Combined Carbon Oxidation-Nitrification at the Metropolitan Sanitary District of Greater Chicago," Report No. 80-12, Research and Development Department, Metropolitan Sanitary District of Greater Chicago, July 1980.

15. Fuller, G. W., *The Purification of the Ohio River Water at Louisville, Kentucky*, D. Van Nostrand Co., New York, 1898.

16. Camp, T. R., "Theory of Water Filtration," Journal Sanitary Engineering Div. Amer. Society Civil Engineers, *90*, SA4, 1, 1964.

17. O'Melia, C. R. and W. Stumm, "Theory of Water Filtration," Journal Amer. Water Works Assoc., *59*, 1393, 1967.

18. Tchobanoglous, G. and R. Aliassen, "Filtration of Treated Sewage Effluent," Journal Sanitary Eng. Div. Amer. Society of Civil Engineers, *96*, 243, 1970.

19. Baumann, R. E. and J. C. Cleasby, "Design of Filters for Advanced Wastewater Treatment," Engineering Research Institute, Iowa State University, Ames, Iowa, 1973.

20. Ives, K., "Filtration—The Significance of Theory," Journal of the Institution of Water Engineers, *25*, 13, 1971.

21. Diaper, E. W. J., "Tertiary Treatment by Microstraining," Water and Sewage Works, *116*, 202, 1969.

22. Lynam, B., G. Ettelt, and T. McAloon, "Tertiary Treatment at Metro Chicago by Means of Rapid Sand Filtration and Microstrainers," Journal Water Pollution Control Federation, *41*, 2, 247, 1969.

23. Zenz, D. R., B. Lynam, C. Lue-Hing, and G. Richardson, "Suspended Solids Removal Processes at Metro Chicago," paper presented at the Third U.S./Japan Conference on Sewage Treatment Technology, Tokyo, Japan, February 3-16, 1974.

24. Sawyer, B. and A. Obayashi, "North Side Pilot Sand Filtration Study," Report No. 75-23, Research and Development Department, Metropolitan Sanitary District of Greater Chicago, September 1975.

25. Sawyer, B., A. Obayashi, and C. Lue-Hing, "Dual Media Filtration of a Nitrified Secondary Effluent, " paper presented at the Forty-Ninth Annual Conference Central States Water Pollution Control Association, Arlington Heights, Illinois, May 19-21, 1976.

26. Sawyer, B., A. Obayashi, D. R. Zenz, and C. Lue-Hing, "Performance Comparison of Three Dual Media Tertiary Filters at the John E. Egan Water Reclamation Plant, " paper presented at the Fifty-Second Annual Conference Central States Water Pollution Control Association, St. Charles, Illinois, May 16-18, 1979.

27. Halff, A., "An Investigation of the Rotary Vacuum Filter Cycle as Applied to Sewage Sludges," Sewage and Industrial Wastes, *24*, 8, 962, 1952.

28. Bargman, R. D., "Discussion: A Method for Evaluating the Variables in Vacuum Filtration of Sludge," Sewage and Industrial Wastes, *27*, 6, 701, 1955.

29. Coackley, P. and B. R. S. Jones, "Vacuum Sludge Filtration," Sewage and Industrial Wastes, *28*, 8, 963, 1956.

30. Albertson, O. E. and E. J. Guidi, Jr., "Advances in the Centrifugal Dewatering of Sludges," Water and Sewage Works, *114*, R. N., R-133, 1967.

31. Haworth, P. J., "The Filterbelt Press," Effluent and Water Treatment Journal, 10, 617, 1973.

32. Sawyer, B., R. Watkins, and C. Lue-Hing, "Evaluation of Unit Processes for Mechanical Dewatering of Anaerobically Digested Sludge at Metro Chicago's West-Southwest Sewage Treatment Plant," Paper presented at the Thirty-First Annual Purdue Industrial Waste Conference, West Lafayette, Indiana, May 4-6, 1976.

33. Zenz, D. R., B. Sawyer, R. Watkins, C. Lue-Hing, and G. Richardson, "Evaluation of Dewatering Equipment for Anaerobically Digested Sludge," Journal Water Pollution Control Federation, *50*, 8, 1965, 1978.

34. Tisdale, S. L. and W. L. Nelson, *Soil Fertility and Fertilizers*, The MacMillan Co., New York, NY, 1956.

35. Harza Engineering Co., *Land Reclamation Project.* Prepared for the Metropolitan Sanitary District of Greater Chicago, October 1967.

36. Hinesly, T. D., L. G. Hanson, and D. J. Bray, *Utilization of Sewage Sludge on Agricultural and Disturbed Lands*, USEPA Grant No. R805629, U.S. Environmental Protection Agency, Cincinnati, Ohio (In Press).

37. Rimkus, R. R., *The Metropolitan Sanitary District of Greater Chicago Maintenance & Operations Department 1981 Annual Report*, Chicago, IL.

38. Pearse, L., *Utilization of Sewage Sludge as Fertilizer*, Federation of Sewage Works Assoc., Manual of Practice No., 1946.

THE METROPOLITAN SANITARY DISTRICT OF GREATER CHICAGO

TABLE 1

CHRONOLOGY OF SIGNIFICANT ACHIEVEMENTS IN ENVIRONMENTAL ENGINEERING AND THEIR APPLICATION TO THE NEEDS OF THE METROPOLITAN SANITARY DISTRICT OF GREATER CHICAGO

Period	Significant Achievements in Environmental Engineering	Year Applied in the District
Pre-1900	Sewage disposal by dilution and genesis of scientific aspects of natural purification — 1850	City started to build sewers to discharge sewage to Chicago River, which discharges to Lake Michigan — 1856
		Reversal of Chicago River and dilution of sewage by diverting Lake Michigan water — 1900
		Intercepter sewers completed and Lake Michigan water was protected. Striking decline in waterborne disease — 1907
		Studies showed pollution of Illinois River by Chicago sewage disappeared before Peoria, Illinois — 1899-1900
1900-1910		
	Imhoff tanks patented in Germany — 1904	Imhoff tanks built at Calumet STW — 1922
	First Imhoff tank built in USA, Madison WI — Chatham, NJ — 1911	Imhoff tanks built at West-Southwest (WSW) STW — 1930
	First full-scale trickling filter built in USA, Madison, WI — 1901	Trickling filter plant at Morton Grove — 1914
	Beginning of disinfection of sewage and effluents by direct chlorine application — 1906-1908	Chlorination of water by City of Chicago* — 1908
		Chlorination of sewage to reduce its BOD — 1926-1928
		Chlorination of North Side STW — 1967
		Chlorination of Calumet STW — 1969
		Chlorination of WSW STW — 1972
	First sludge centrifuge used in Germany — 1908	Centrifuges at WSW STW — 1981
	First continuous sludge centrifuge used in USA, Milwaukee, WI — 1921	Centrifuges at Calumet STW — 1981
		Centrifuges at J. Egan WRP — 1982
1910-1920		
	First separate sludge digestion at Baltimore, MD — 1912	Sludge digestion in unheated Imhoff tanks at Calumet STW — 1922
	Heat was applied to a digester in Plainfield, NJ — 1926	Sewage digestion in unheated Imhoff tanks at WSW STW — 1930
	First heated digester installed at Antigo, SI — 1928	Design criteria for high-rate mesophilic digestion — 1961
		High-rate mesophilic digestion at WSW STW — 1963
		High-rate mesophilic digestion at Calument STW — 1965
	Invention of activated sludge — 1913-1914	Des Plaines River Sewage Treatment Plant — 1922
	First plant in USA, San Marcos, TX. Porous diffuser plates used — 1916	Calumet STW — 1922
		Tests conducted with porous diffuser plates at Des Plaines Sewage Treatment Plant (1922-1924) for prescribing them to North Side plant.
		North Side STW — 1928
		Additional diffuser plate studies conducted in 1934 for prescribing them to Calumet and Southwest Sewage Treatment Works

	FUNDAMENTAL RESEARCH NEEDS	RESEARCH USER PERSPECTIVE
		WSW STW — 1935
	Nature of nitrification — 1877	Full-scale nitrification studies and development of design criteria — Hazelcrest STW, WSW STW, Calumet STW, NSSTW, and J. Egan WRP — 1969 to date
	Occurrence of nitrification in activated sludge — 1913-1914	
1920-1930	Vacuum filters first tried in Milwaukee, WI — 1921	Vacuum filters at WSW STW — 1935
	$FeCl_3$ and chlorinated copperas were discovered to be good conditioning agents for sludge filtration at the District — 1928	Conditioning of sludge at Chicago stock yards with $FeCl_3$ and chlorinated copperas — 1928
		Mechanical sludge removal rakes installed at Calumet STW — 1928 — followed by WSW STW, and known for their unique design
	Sludge incineration developed — 1920	
1930-1940	First large-scale dewatering and incineration plant for sludge (20 dt/d) at WSW STW — 1932	30 dt/d sludge dewatering and drying plant at Calumet STW — 1936
		Heat-drying plant for waste activated sludge at WSW STW — 1939
		New sludge drying equipment installed at WSW STW — 1935
1940-1950		Studies on clarification of activated sludge and optimization of the settling of activated sludge mixed liquor in secondary clarifiers — 1944-1945
1950-1960	First dissolved air flotation (DAF) installation in USA, — Bay Park Sewage Treatment Plant, Nassau County, NY — 1957	DAF studies at the District — 1964
	Zimmerman Wet Air Oxidation Process for sludge disposal — 1958	A Zimpro plant was installed at WSW STW — 1960
		Zimpro plant abandoned due to safety and operational problems — 1972
1960-1970	First microstrainer installation in USA, Hanover Park District — 1965-1966	Studies on effluent polishing suspended solids removal by microstraining, sand, and dual media filtration — 1965
	Concept of sewage effluent and solids application on land quite old (Greece-B.C.) — applied in Bunzlav, Germany — 1531	Concept of utilization of sludge solids on land adopted — 1967
		The above concept was implemented — 1970
1970-1980	First commercial biodisc installation in USA — 1969	Pilot-scale studies on nitrification of primary and secondary effluents, and sludge lagoon supernatant with biodisc — 1973
		Pilot-scale studies on nitrification and denitrification of sludge lagoon supernatant with biodisc — 1975
1970-1980		Tunnel and Reservoir Plan adopted to solve combined sewer overflow — 1972

° Drinking water purification and distribution are under the jurisdiction of the City of Chicago which is in the District's service area.

Note: Some information contained in this table is extracted from Reference (3).

THE METROPOLITAN SANITARY DISTRICT OF GREATER CHICAGO

TABLE 2

NITROGEN REMOVAL STUDIES BY THE METROPOLITAN SANITARY DISTRICT OF GREATER CHICAGO, 1972-1980

Process	Size	Objective	Results	Reference
Biological Rotating Disc	pilot plant (2.0gpd/ft^2)	study B.R.D. as means of removing high NH_4-N concentrations from sludge lagoon supernatant	NH_4-N removal 99%, pH control required, need added Na_2CO_3	8
Two-stage Nitrification	pilot plant (150 gpd)	study nitrification in high industrial waste sewage by two-stage process	NH_4-N removal 88%, NH_4-N oxid. 145-420 mg/L-d, Avg. SRT=19.3 days	9
Single-stage Nitrification	full scale (21 mgd)	test of design criteria for single-stage nitrification at West-Southwest STW	HRT=8.9 hrs, SRT=9.2 days, NH_4-N removal 95%, high NOD in effluent	10
Single-stage Nitrification	full scale (80 mgd)	test design criteria for single-stage nitrification at North Side STW	SRT $\geqq$ 7 days for $\geqq$ 90% NH_4-N removal at 10°C; DO <2 mg/L inhib. nitrif.	11
Single-stage Nitrification	pilot plant (100 gpd) & full scale (30 mgd)	study nitrification in high industrial waste sewage by single-stage process	HRT=10 hrs, SRT=10 days for year-round nitrification, NH_4-N removal 90%	12
Two-stage Nitrification	pilot plant (0.6 mgd)	develop design parameters for two-stage nitrification at John E. Egan WRP	optimum 2nd stage nitrification, HRT= 4.4 hrs, SRT $\geq$ 10 days	13
Single-stage Nitrification	full scale (300 mgd)	test design criteria for plant expansion at West-Southwest STW	SRT controlling factor. 10-day SRT for year-round operation HRT-7.9 hrs	14

THE METROPOLITAN SANITARY DISTRICT OF GREATER CHICAGO

TABLE 3

TESTING OF TERTIARY TREATMENT SYSTEMS FOR EFFLUENT POLISHING AT THE DISTRICT

Year	Plant	Type of Effluent	Systems Tested	Scale	Reference
1965-68	Hanover Park	Secondary	Hardinge continuous backwashing sand filter, Glenfield and Kennedy micro-strainer	Full	22
1969-70	Hanover Park	Secondary	Delaval upflow filter, Neptune Microfloc mixed media filter, Graver dual media pressure filter	Pilot	23
1971-73	North Side	Secondary	Crane-Cochrane micro-strainer	Full	23
1973-74	West-Southwest	Two-stage Nitrified	Neptune Microfloc mixed media filter	Pilot	Unpublished
1974-75	North Side	Nitrified	Roberts dual media gravity filter	Pilot	24
1975	Calumet	Nitrified	Roberts dual media gravity filter	Pilot	25
1977-78	John Egan	Two-stage Nitrified	Dual media gravity sand filters	Full	26

THE METROPOLITAN SANITARY DISTRICT OF GREATER CHICAGO

TABLE 4

AGRICULTURAL USES OF SLUDGE BY THE METROPOLITAN SANITARY DISTRICT OF GREATER CHICAGO, 1923-1981

Date	Activity
1923	Heat-dried activated sludge was bagged and sold from the Maywood and Calumet STW's (38).
1936	Air-dried Imhoff sludge was given to farmers (38).
1939	Vacuum filtered and flash-dried sludge was sold from the West-Southwest STW (38).
1968	A liquid sludge on land program was started. Demonstration fields were established in Cook County.
1969	Research with the University of Illinois started at Elwood Agronomy Research Station. A seven-acre research site was established at the Hanover Park WRP. An alkali sand spoil was reclaimed with liquid sludge at Ottawa, Illinois.
1970	First unit trains of liquid digested sludge went to Arcola, Illinois. First District land purchase in Fulton County, Illinois, for liquid sludge application.
1972	Commenced application of liquid sludge in Fulton County, Illinois.
1973	Fulton County land purchase totals 15,500 acres.
1978	Liquid sludge application expanded to 100 acres at Hanover Park WRP.
1981	Air-dried digested sludge used to reclaim Chicago dump site.

PERCEPTIONS OF RESEARCH REQUIREMENTS FOR WATER AND WASTEWATER MANAGEMENT

by
William J. Katz, Ph.D.
President, William J. Katz Associates, Ltd.
Milwaukee, WI

My job today is to identify some of the needs for technology development to protect our water resources. The issues of maintaining and improving water quality deal with the very essence of the quality of our lives. The high costs involved demand more precise, usable technical information than is now available. The demand for dollars for other programs of federal and local governments and the present recession requires that expenditures for water quality improvement be made on the basis of the impact on water quality and more accurate estimates of cost. Today, affordability is a foremost concern. What is the increment of water quality improvement for each dollar spent is a question being asked today. The answer is not available with an accuracy required for decision-making, because the present status of knowledge is lacking. The premise for setting forth research programs and projects has changed and the need for technical information has also changed. To be candid, this task requires an immense ego and a lot of gall. In moments of humility, I recognize the complex inter-relationships that exist between the water quality, water discharge, economic conditions, politics and religious (philosophical) perceptions. The perception of technology needs as seen by the practitioners, those that design, finance and operate facilities, varies with time and is affected by a complex set of factors including the biases of the individual.

The circular intellectual process by which we set our priorities for research is well described by Gary Zukav in his book the "Dancing Wu Li Masters. "Zukav discusses in his book the changes in how the world is viewed, by physicists. Similarly, the changing views of the water and waste water research needs seem to follow a similar pattern.

Zukav states, "Reality is what we take to be true. What we take to be true is what we believe. What we believe is based on our perceptions. What we perceive depends upon what we look for. What we look for depends upon what we think. What we think depends upon what we perceive. What we perceive determines what we believe. What we believe determines what we take to be true. What we take to be true is our reality."

If this circular intellectual path is so, research requirements change with our perceptions (our perceptions change with time) and need to be re-examined in light of new information and changing circumstances. Let us take a look at some of the significant changes in perception of problems in our environment that have occurred since the 1972 Clean Water Act was being written and passed.

All of you are aware of the Needs Categories that was the basis of the regulations and funding of the Municipal Grants Program. Some of these are Sewage Treatment, Combined Sewage Overflows, Storm Water, Rural Surface Run-off. In many urban areas, water pollution has occurred from each of these discharges. Frequently, pollution loads from storm water and combined sewage during wet weather-related conditions exceeded dry weather flow loads by more than ten times. From a funding and regulatory standpoint, the dry weather sewage flow treatment was given priority while ignoring the impact on the decision making process of the other sources of pollution. It followed then, that the technical criteria for the design and operation of treatment plants were a result of an intellectual model based on engineering neatness and institutional convenience. Also, implicit in the view of those who were involved in drafting the 1972 Act was that there would be sufficient resources (dollars) provided to meet the then perceived objectives of cleaning up our nation's waters.

The idea held in 1972 and still operative today is that by setting standards for treatment plant effluents based on existing treatment technology that water quality improvement objectives will be met. It was known then as we know today that more information was needed and new methodology was required to provide the ability to make decisions for the design of sewerage facilities based on water quality impacts. It was unrealistic to expect that engineering technology for the design of sewerage facilities to achieve water quality goals when sufficient knowledge of the relationship between treatment results and water quality impacts are often not available. Our ability to make sound decisions for building and operating treatment facilities is illustrated by the shortfall in achieving the water quality goals of the 1972 Act. Our ability to estimate costs of this program is not much better than one order of magnitude. Unfortunately, the actual costs were invariably underestimated.

New developments in technology have also changed our percepton of water and waste water treatment needs. Over the past fifteen years we have learned to identify the existence of hundreds of exotic organic compounds in concentrations on the level of parts per billion (or less). The explosion of methodology and equipment (computer, etc.) to generate numbers inexpensively and profusely has a powerful influence on our perception needs. Some of these compounds have been identified as being carcinogenic, mutogenic or toxic as determined in the laboratory on test animals. Persistence of some of these compounds in the water ecological system has raised questions of human health and concern about the damage to other living species. These issues of human health and the quality of the environment are a part of the core of the existence of each of us, therefore, are legitimate concerns for society and government. Scientific knowledge, when reduced to numbers, becomes a powerful influence in the decision-making process of the practitioners. It makes little difference that some of the information, even though it is numerically expressed, is little more than a qualitative statement of our knowledge. Affordability as a criterion in setting policy for expenditures of limited resources (limited dollars) to treat waste waters cries out for more intensive and continuing research. There is an ever greater need today to develop usable, quantitative information about ocean waters, streams, lakes and ground water. The connection between water quality standards and risks to health and between water quality and protection of the ecosystems of the environment needs to be understood with greater accuracy. The zero risk (zero discharge) posture of many who are concerned with environmental issues is a logical consequence of the lack of sufficient knowledge held by the experts.

How real is the expectation that an activated sludge plant as currently designed can achieve the promulgated effluent standards? These standards were arrived at by viewing the past performance of existing plants which were operated with practices that are forbidden today, or at the very least, not accepted.

Prior to and up through the early part of the 1960s, waste water treatment was used to remove organic material (BOD) and suspended solids. The criteria for a well operated activated sludge plant were the achievement of 90% removal of BOD and suspended solids. Generally, if the 90% removal was achieved on an annual average basis this was accepted satisfactory performance. If a process problem arose such as too high a flow (usual during wet weather) for the plant to remain in reasonable operating conditions, by-passing of untreated sewage was also accepted as a necessary part of operating. It was not unusual for a plant operator who experienced problems in the removal, treatment or disposal of solids to allow the solids to pass through the treatment plant into the plant effluent and flow to the receiving water. Restrictions on discharges to the sewers were related to protecting the facility and/or the process rather than to impacts on effluent quality and impacts on land and groundwater by disposal of treatment plant residuals.

The research in the 1950s and early 1960s was related to better understanding of the unit operations used for sewage treatment. New materials were introduced for construction of sewers, equipment, and treatment plants. The new knowledge gained through research by universities, equipment manufacturers and pollution control agencies gave the designers and operators a new level of quantitative understanding. Perceptions of the functioning of an aeration basin in an activated sludge process changed to that of a bio-reactor. Mass transfer of oxygen from the air to liquid to bio-mass became a part of the applied technology. Similarly, technology transferred from the chemical and mining fields added to the change in perceptions of the functioning of the final clarifier in the activated sludge process. The perception of the final clarifer changed to a two phase liquid-solids separation basin with time required for coalescence of the bacterial flocs and a zone for thickening the settled activated sludge to be returned to the bio-reactor. Overflow rate and mass loading became the new quantitative design and operating perceptions replaced the "Ten State Standards." Many of the audience never heard of, or may have forgotten, the overflow rate criteria in "Ten State Standards." These two examples of changing perceptions of function of the activated sludge unit operations improved understanding that made design and operation more rational. Even though a more rational basis is used for the design of an activated sludge plant, the expected reliability of performance has not been verified in the field. Historical performance of treatment plants, because of past operating practices, does not include the variability of flow which a sewerage system must collect and treatment under present day operating criteria, e.g. no by-passing.

In 1972, standards for treatment plant effluent were established and promulgated. Logic dictated that water quality is affected by effluent content (BOD, solids and other constituents) not by the percentage removal of these constituents. Standards of discharge were based on perceived treatment technology. (Unfortunately, not related to water quality impacts, because quantitative knowledge of the relationship of discharge standards to water quality was not then and is not now sufficient.) The "30/30/1" (BOD, suspended solids, phosphorus, in mg/l) effluent standard was established for the performance at design flow and at design concentrations of sewage entering an activated sludge treatment plant. Added to the promulgated standard was the idea that it should be achieved on a monthly average basis (rather than a year) or on a running 30 day average. In some instances a maximum daily limit was set at 45/45/1. I suspect strongly that the effluent standards were established much like the "Ten State Standards" for the design of settling basins. A group of experts reviewed the past performance of existing activated sludge plants and concluded that the data showed that some "well operated" plants do or could meet the promulgated 1972 effluent standards. Indeed, with appropriate selectivity, one can find 30 day periods in which the standards are met by any reasonably operated plants; however, rarely at design conditions. In addition, the technology as applied to the real world of the activated sludge plants could not meet the standards with the reliability of performance expressed by the promulgated regulations. Design of treatment plants does not take into account the reliability of performance, because we do not have the necessary information and methodology to achieve a desired level of reliability. It follows that the attendant investment and operating cost upon which to base engineering decisions are also lacking.

The research and development efforts until now have been mainly to develop new technologies that make the treatment processes, equipment and hardware more sophisticated and more complex. This was done without due consideration of reliability and without consideration of redundancy for critical equipment and processes. The greater sophistication requires more attention of skilled technicians which smaller pollution control plants are unwilling to provide. The new technology arising from process research in the 1950s and 1960s has led us to reduce the size of treatment plants to reduce capital costs. This more intensive use of facility capacity comes at the price of reduced reliability. However, even the answer to make facilities bigger is not easily ascertained. Which units of operation are critical and by how much should these be increased in size? We need more quantitative technology to make more accurate cost-effective decisions.

Another area of technology requirements is a quantitative understanding of the variability of the quantity and quality of treatment plant influent standards. Diurnal variations during dry weather in the strength and flow of sewage have an effect on the effluent quality. However, the greater effects by an order of magnitude is the variation in flows due to wet weather related phenomena. Generally, the failure to meet discharge standards occurs during times of rainfall and snow melts. It has become apparent that many sanitary sewer systems receive large quantities of clear water flow during wet weather conditions. While some reduction in clear water flow in sanitary sewers is feasible, as a practical matter, the flow in sanitary sewage will continue to be affected by wet weather conditions. The strength of the sewage, combined sewage and the storm water varies with the history of the storm and the history of preceding wet weather occurrences. In the past, using combined sewer overflows and bypassing sewage around the sewage treatment plant were acceptable procedures. They are not today. If we are serious about meeting water quality goals, the need for treatment options other than activated sludge process must be available, because of the unaffordable cost to capture and treat all sewage to secondary treatment standards.

To deal with the treatment of flows in an urban area during wet weather, it will be required to view the water discharges in an urban area as a system of variable and multiple flows and discharges. It may be more affordable and more effective to capture and treat by other than secondary treatment at certain times and certain flows than to increase capacity of the conventional sewage plant. More may be achieved in protecting the water quality and public health. The design and operating methodology to deal with pollution control on a system-wide basis for an urban area needs further development. While a significant amount of information was developed by USEPA on the treatment of storm water and combined sewage overflows, more needs to be developed. The role of flow equalization on modulating variability and mitigating attendant problems of solids handling and disposal is another area of needed technology. The relationship of water quality to discharge standards is a key factor in making these design decisions. A functional quantitative relationship needs to be developed.

When discussing the needs for technology development for waste water treatment, the need for improvements in the handling and disposal of waste water treatment residuals is apparent. Over the years, a great deal has been accomplished in improving thickening and dewater technology. Improvements in conditioning chemicals and hardware have been significant. In recent years, it has been apparent that one of the areas for opportunities to develop technology is in the reduction of energy requirements in the disposal of sludge. The increased energy costs have changed the perception of an anaerobic digestor from a sludge stabilizing and pathogen destroyer process to an energy recovery process. Changes in operation and design may afford new advantages to reduce overall energy requirements for treatment. There is a critical need for technology to develop beneficial uses of residuals such as for use as a compost, fertilizer or for direct application as a soil conditioner in farming. Technology is needed that will provide more definitive, quantitative answers to concerns for health, aesthetic criteria, and impact on surface and ground water to overcome public resistance to the beneficial use of sewage sludges.

There is a growing need for research and development which will provide information to achieve improvements in water quality that are affordable. Research is required to develop relevant technical information to deal with the new issues of affordability. These issues require the marriage of relevant technology and financial management. Knowledge arising from research must give a better understanding of our practical needs. Which came first, the chicken or the egg? Which came first, the practical need or the technology? If there is an answer to each of these riddles, it might be that both came first.

SOME OBSERVATIONS ON RESEARCH ISSUES

Russell J. deLucia*
MetaSystems Inc.
Cambridge, MA

One of the problems that arises when discussing with a captive audience such important issues as research is the temptation to think that one can say something profound and perhaps even unique. Such illusions can, however, be easily crushed. Making a list of some of the issues one wishes to address along with a few of the thoughts on these issues, thinking a bit about people with whose work one is familiar, reading or rereading some of the more "philosophical" and/or state-of-the-art papers, and talking to colleagues, and, if you are as lucky as I am to have a mentor* still easily available, talking to them about some earlier general "pieces" can be quite disillusioning. Most if not all of what one might think to say has been said earlier, although often in a different context, but more crushing, it has often been stated more eloquently.** So one is reduced, as in my case, to accepting the lack of uniqueness and merely holding the hope that some of the particulars which are part of my observations may prove useful to both those who undertake research and those who sponsor it.

Some twenty-three years ago, the ASCE Sanitory Engineering Journal published the following definition.[1]

> "Environmental Engineering is the application of engineering principles and practices to one or more elements of man's physical environment for the purpose of controlling or improving the total environment.
>
> In this concept of environmental engineering, man is surrounded by his total physical environment and lives within the constraints of his social institutions. The Environmental Engineer, with the support of a broad spectrum of physical sciences and some of the social sciences works within the constraints of these institutions to control and improve man's total physical environment."

The same article goes on to suggest specific activities that constitute professional practices in environmental engineering. The definition is broad enough to cover much of what I associate with environmental engineering and scientific practices and specifically mentions the broad dimensions of this application area and its reliance on both physical and social sciences. I have found it useful in thinking about some of the research issues that I want to discuss to focus on two of the characteristics of this broad area of endeavor, characteristics which emphasize the enormity of what we have to deal with. The first of these is the range of scale (time and space) that encompasses the various "problem sheds" with which we have to deal. The second is the vast eclecticness of this area as manifested in the numerous disciplines upon which we draw. Table 1 presents the range of scales and disciplines which are associated with the problems analyzed. Against these char-

TABLE 1. SCALE AND DISCIPLINE BASIS OF ENVIRONMENTAL PROBLEM ANALYSIS

Scale in space	Scale in Time	Disciplines
Global	Centuries	ecology hydrology physical chemistry aquatic biology
National	Decades	geomorphology meteorology
State/Regional	Years	economics applied mathematics
Physical (e.g., river basin)		statistics
Economic/ Institutional	Hours/Days	epidemiology physiology demography
Site/Project		political science
Process/ Technology	Seconds/Minutes	forestry

*The author is fortunate to have as a colleague and fellow principal in MetaSystems Inc., his former mentor, Professor Harold A. Thomas of Harvard University who graciously lent him some files which were drawn upon in preparation of this paper.

**This is evident from some of the citations below.

acteristics there are a number of broad observations regarding research issues which I wish to make.

1. *Our increasing understanding of the environmental effects of anthropogenic change argues for increasing the scale limits of our analysis. But at a larger scale (time or space), our research basis for adequately addressing the problem is often weaker.*

Much professional practice is focused at smaller (space) and shorter (time) scales. But increasingly, we are either called upon to examine problems on a larger spatial scale (e.g., regional/coastal zone, environmental management plans, state, region, air quality control, etc.) or to redefine the nature of the problem in larger temporal dimensions (e.g., nuclear waste disposal, the cumulative nature of some organic and inorganic residuals). The increasing use of the air, land and water systems for the storage, assimilation or detoxification of wastes from the sum of man's concentrated activities such as urbanization and intensive agriculture or the effects of macro scale engineering projects argue for larger scale limits to our analysis.

The dimensions of time and space to which our analysis (predictions, inferences and recommendations) must hold are often not matched with the causal and/or analytical methods necessary to support the analysis. There are obvious areas, such as acid rain and carbon dioxide issues, in which visible and significant research expenditures are being made to alleviate these shortcomings. In other areas, research is both less visible and less generously supported. For example, there is a host of situations, such as large-scale irrigation, extensive deforestation and intensive urbanization and other activities, under which the convenient assumption of ignoring feedback between man's activities and hydrology and climate may not hold. As Eagleson[2] has recently summarized, there is an evolving understanding of man's role in the atmospheric-earth coupling. But we clearly do not know enough for analysis in most situations. Other analogous scale-related issues can be mentioned. For example, there is our poor understanding at a larger regional scale of "background" or weathering as opposed to anthropogenic loads of trace elements, some of which may be highly toxic. Or consider such regional problems, particularly important in parts of Asia, as the downstream impact of coral reef destruction and concomitant fisheries losses seemingly related to increased upstream erosion as a result of poor farming practices or the coastal erosion as a result of mangrove forest destruction from the use of defoliants in Vietnam.

2. *At small scales, there are still many weaknesses in our basis for analysis, some of which have been identified for decades but for which much remains to be done.*

An obvious example that comes to mind here are the problems of the vadose zone. Having worked as a graduate student on the issues of crop root zones, and remembering interest at that time among fellow students and professors on the questions of subsurface waste disposal, I realize now that this was one of my first exposures to the markedly different way in which various disciplines examine problems. I recall suggestions made by certain investigators to a national research group examining waste treatment processes, suggestions that more research be focused on subsoil waste disposal issues, and I understand that not a great deal has been done in this area until recently. A recent focus on the storage, discharge and transport (through the soil column) of toxic and/or hazardous materials and the flux of materials from large suburban septic systems to groundwater systems has increased the need for additional work in this area. Based on discussions with colleagues, there seems to be a considerable way to go. The understanding and analysis base for water movement is still less than what it might be and an understanding of solute movement and change is even more woeful. This lack of progress may be due to both different disciplinary emphasis and a lack of interest in and support for non-laboratory field investigations.

3. *At various scales, parts of our systems are better understood than others. The research agenda should reflect system perspectives.*

Many environmental issues require dealing with sequentially dependent processes (rainfall, runoff, streamflow, residue emission, transport, dilution/ assimilation, exposure). Analysis requires the linking of models or constructs of the individual processes, and there are questions regarding the predictive power of the causal chain given the uncertainties with which these processes can be described. Research might well be focused on that part of the chain that would most reduce the uncertainty in the overall analysis, since it is the overall predictive power that is most important for making relevant societal decisions.

The two very different examples of such linked processes mentioned above both have areas of uncertainty. In the rainfall runoff hydrology example, the more important overland and subsurface flow, the more limited the overall usefulness of the various urban hydrology models. The success of these models is to a large degree proportional to how much of the catchment area consists of components such as impervious surfaces, sewers and other hydraulic components for each of which the models are well developed and calibrated. All these subsystems are within the purview of hydrology, one of the disciplines upon which environmental engineering draws. The dominant need is for better understanding of the particular subsystem, but this may imply research of a type "less favored" than others, particularly by academics active in this area.

The pollutant discharge to health effect causal chain encompasses subsystems normally the focus of many disciplines—physiology, epidemiology and statistics as well as various sciences involved with transport media. Hence, it is easier to understand the failure to deal with these causal chains as a system. The questions of what contribution (overall system uncertainty) individual subsystems make and whether existing data basis can be the bases for overall models are ones with which some of my colleagues have recently been grappling.[3]

4. *Too much environmental research reflects the constraints of the current academic system enamored with mathematical system analysis and adverse to long haul and field work efforts and a reasonable blend of mission and discipline-oriented research.*

Observations analogous to this have been made before. The balance between empirical and causal investigations, "important" versus "soluble" problem research agendas, and the difference between prediction and understanding-driven research are common in the history of environmental science and engineering and the various disciplines upon which they build. Others have discussed these issues at length in both the general scientific context (e.g., Weinberg[4]) and the most specific discipline categories. In the latter case, the NAS Panel on Scientific Basis of Water-Resource Management most recently discussed the recent history of computer model domination of water resource research without the complementary commitment of furthering basic scientific understanding.[5] I shall not belabor points made at length by others but rather will add some personal comments related to both unfortunate byproducts of this phenomenon and the need for influencing the research agenda.

One of the byproducts of the emphasis on mathematical techniques and computer-based research (often at the expense of both work on causal theory and field work) is the production of students and future employees who possess an overabundance of computing skills and ideas and a relative scarcity of theoretical grounding and/or appreciation for data limitations, measurement problems and other issues. The result is an analytical bias in engineers and analysts who can produce empirical relationships with facileness but have inadequate perspectives regarding the errors of measurement that may be inherent in the numbers they are using and the weaknesses in the formulation of the empirical relationships that reflect at least some modicum of possible causal relationships.

Of course the problems are not just the publish or perish pressure, the attractiveness of the variations on a mathematical theme approach or the inconvenience of field research. There is also a scarcity of research monies for both long-term work and that research which requires considerable and expensive field work. In some cases these barriers have been overcome by fruitful symbiotic relationships between government laboratories and/or data collection agencies and particular academic groups and contract research organizations. But there is a need for an increase in the research resources and mechanisms necessary to support long-term types of work, particularly those that may be risky.

Can we shift the emphasis, agenda, incentive and reward structure of both the sponsors and implementers of the research agenda? Isn't that some of what this meeting is all about? My guess is yes, we can influence the emphasis, but given the current political environment there are real limits. Without significant increases (or decreases) in research budgets, structural change to research organizations and processes will not come easily or quickly. Nonetheless, there is a role for the practitioner to influence this process.

5. *There has been and will be a role for the practitioners in filling the research voids but more importantly in influencing the research agenda. We need to be active in defining the mission versus discipline research duality.*

Those of you who share with me the experience of having done course work under the late Professor Gordon Fair probably also appreciate the degree to which the early basis of environmental engineering and science was built by practitioners—many of them non-engineers. From the early days of British (Chadwick) and Bostonian (Shattuck) concerned citizens, through to the leading turn-of-the-century practitioners such as Hazen, the area of professional endeavor we now label environmental engineering was defined and influenced by non-engineers. Much later it was "academics turned practitioners," such as O'Connor and Orlob and their colleagues, whose work impacted particular aspects of professional practice—in this case the analysis of stream and estuarial systems. Today it can be the work of various practitioners, including increasingly those who are part of public or special environmental interest institutions that will influence both problem identification and solution.

In the early part of this century, when important research was done to better understand water and wastewater processes, the mission versus disciplinary dichotomy either did not exist or was not as important. As Harvey Brooks[6] pointed out a generation ago, this discrepancy also did not exist in the early concept of engineering. An engineering school consisted of practitioners teaching young people from the benefit of their experience, but this approach gave way to the importance of integrating new scientific findings into engineering education. Brooks[6] suggests that what was lost in this shift in emphasis was the practitioner's "willingness to deal with problems whole rather than in terms of the individual contributing discipline. "The resulting overspecialized attitude led to support by Brooks[7] and many others to broader approaches and the emergence of the interdisciplinary programs. But even a generation ago Brooks noted the limits to interdisciplinary approach—one cannot know everything. The need is to communicate with scientific specialists and extract from them what is needed for an analysis.

From my perspective, the early interdisciplinary programs, particularly the one which produced me—that at Harvard—moderated its emphasis on the use of operations research and system analysis technologies, teaching it as a tool for integrating various disciplinary inputs to problem solving. But somehow programs emphasizing interdisciplinary research and teaching and heavy use of mathematical techniques went too far. Perhaps it was the increasing availability of mathematical computational capacity and the interdisciplinary focus on linkages. In any case, emphasis on the subdiscipline, theoretical development, waned. Perhaps it is time the wheel turned again. The practitioner can and should influence the turn of the wheel.

Consider some of the observations made above which suggest various problems arising from anthropogenic activities which require solutions. The problems arise from societal conflicts and pressures, and their solutions define the need of mission-oriented research. But in many cases, problems cannot be solved without a better understanding of different particular phenomena—hence the potential for mission orientation to drive discipline-oriented research. But the history of science and engineering has told us that the important problems are often the most difficult, hence much disciplinary research focuses on soluble problems with the goal of developing a knowledge base that will allow the solution of the important but difficult. It is important that the practitioner prod, cajole, explain and argue to stimulate the necessary research, preferably before the problem is of crisis proportions. This responsibility is unfortunately not easily met, although meetings such as this facilitate its discharge.

REFERENCES

1. Sanitary Engineering Division, *Journal of the American Society of Civil Engineering*, submitted by Roy F. Weston of Roy F. Weston, Inc., Newton Squnare, Pennsylvania, October, 1965.
2. Peter S. Eagleson, "Hydrology and Climate" (Chapter 2), *Scientific Basis of Water-Resource Management*, Studies in Geophysics series.
3. Meta Systems Inc., "Statistical Aspects of Relating Environmental and Health Data" prepared for Environmental Protection Agency, September, 1981.
4. Weinberg, Alvin M., "But Is the Teacher Also a Citizen?", *Science*, Volume 149, August 6, 1965.
5. *Scientific Basis of Water-Resource Management*, Studies in Geophysics series, panel on Scientific Basis of Water-Resource Management, Myron B. Fiering, Chairman. Chapter 3, "Implications of the Vadose Zone to Water-Resource Management," Donald R. Nielsen and J. W. Biggar; Chapter 8, "Empirical and Causal Models in Hydrology," V. Klemes; Chapter 9, "Field Data: The Interface Between Hydrology and Geomorphology," Luna B. Leopold. National Academy Press, Washington, DC, 1982.
6. Brooks, Harvey, "Challenge to Civil Engineering," *Journal of Professional Practice, Proceedings of the American Society of Civil Engineers*, January, 1964.
7. Brooks, Harvey, "The Dean's Report, 1963-1964," *Harvard Engineers and Scientists Bulletin* No. 45, page 6, 1964.

THE ANALYTICAL/REGULATORY INTERFACE

R. O. Kagel
Director, Environmental Quality
Dow Chemical U.S.A.
Midland, MI

A few weeks ago, the Technical Director of the Dow Michigan Division Analytical Laboratories, was hurrying through the Tokoyo Airport when he came upon a young gentleman tearing pages from a bible and handing them out. He took one. When he began to read it on the airplane, he found at the top of the page, Proverbs 11:1.

> "A false balance is abomination to the Lord:
> But a just weight is his delight."

The research aspects of my remarks are probably as antiquated as this biblical reference. You've all learned about statistics and error analysis of measurements in your undergraduate engineering and chemistry courses. The problem is that too few people in the world have any appreciation whatsoever for statistics and error analysis. This course should be required in all law and journalism curricula. And, it is an area that deserves the attention of all researchers in the regulatory arena today.

Milton Wessel, a well-known environmental attorney, notes in his book, *Science and CONscience*[1]

> "Analytical methodology tends to be one of the sciences on the very forefront of many environmental disputes."

He goes on to say that the difficulties arise because there are differences of opinion among chemists when it comes to the interpretation of data. This is certainly true.

But, if analytic chemists have a problem interpreting each others' data, imagine the magnitude of the problem when lawyers use these data to develop or enforce an environmental regulation. Now, add to this the news media's often inadequate reporting of environmental matters, and it is no mystery why the general public doesn't have a chance to understand the real issues in many environmental incidents.

Setting and enforcing most environmental regulations involves the concentrations of chemicals—numbers—and the validity of the analytical methodologies used to generate those numbers.

Validation of an analytical procedure involves the statistical treatment of data to determine precision, accuracy, sensitivity and reproducibility of the procedure from laboratory to laboratory (inter-laboratory) or even from analyst to analyst within a laboratory (intra-laboratory). Analytical methods do not yield numbers, but rather, ranges of numbers as determined from the validation procedure or by inter- or intra-laboratory round-robins. The American Chemical Society has published Guidelines for validating analytical procedures.[2] The American Society for Testing Materials has routine procedures for conducting round-robins.[3]

Unfortunately, conflict over regulations ultimately is arbitrated in the court system. It is probably safe to say that the judiciary does not understand the problem either. This only adds to the confusion and mystique surrounding the use of analytical data in the regulatory rulemaking process.

Let me explain—Chief Judge Coffin of the 1st Circuit Court of Appeals ruled on matters of analytical data in the Pesticide Best Practical Technology Case[4] in 1979:

> "In this case, we think it was far more important for EPA to solicit comments on how it intended to collect and use the data than on the data itself."

This ruling effectively puts aside all arguments about the validity of the data base in this rulemaking process. Chief Judge Coffin also ruled:

> "We cannot play the role of the superchemist. Petitioners bear an extremely heavy burden if they desire to demonstrate that the results of the rulemaking procedure are infected with fatal flaws of laboratory practice."

This ruling set a precedent that the court will not rule on the validity of analytical data. The Agency's data are assumed to be correct. This ruling could have a major impact in future enforcement cases where analytical data are in question. And, this is likely to be the case.

So the judiciary is another segment of society that clouds the understanding of the analytical/ regulatory interface. And, it is a very important segment. The analytical/regulatory interface is indeed very serious business.

A violation of the provisions of the Clean Water Act (CWA) can result in civil sanctions up to $10,000/day and criminal sanctions for a known or intentional violation of up to $25,000/day, *plus* prison sentences. Some states have passed legislation that would require up to ten years at hard labor, plus the $25,000/day fine for a knowing violation.

It is a matter of record that there have been both civil and criminal convictions under the provisions of the CWA. In at least one case, the interpretation of analytical data was the central issue in a lawsuit brought by the Agency against a major chemical company.

The Clean Air Act, The Resource Conservation and Recovery Act and the Toxic Substances Control Act also have equally stringent provisions for civil and criminal sanctions for violation of the provisions of these Acts.

Let me explore the analytical "anatomy" of one rulemaking process in the CWA and the enforcement of the rule if it is promulgated under today's standards. The rulemaking is the development of effluent guidelines for the organic chemicals industry under Section 307 of the CWA.

The Agency's effort to develop these effluent guidelines began in the mid-1970's under the Carter Administration. The policy-makers in the Agency devised a strategy which involved sampling and analysis of industrial effluents. The strategy involved a two-phase approach—the screening phase and verification phase.

In 1976, the Agency collected samples from about 175 industrial sites and these samples were analyzed by gas chromatography/mass spectroscopy (GC/MS) for the 129 priority pollutants by Agency contractors. The GC/MS procedures were not validated. Most companies split samples with the Agency and independently analyzed these samples by the same GC/MS methods used by the Agency contractors.

Since many companies had split samples with Agency during the 1976 screening phase sampling, and analyzed these samples, the Chemical Manufacturers Association (CMA) contracted with Radian Corp. to statistically analyze the inter-laboratory variability in this data base. Radian Corp. concluded[5]:

1) Qualitatively, approximately 50% of the time the company and the Agency contractor could not agree about the presence or absence of a priority pollutant, and

2) Quantitatively, when the two laboratories agreed a priority pollutant was present, the absolute values differed by an order of magnitude—differences of two orders of magnitude were not uncommon.

CMA also engaged Professor D. F. S. Natusch[6], Colorado State University, to evaluate the Agency's screening phase sampling procedures and data. He concluded:

> *"Under absolutely no circumstances whatsoever should the results obtained be used for setting standards."*

Thus, the screening phase data were laid to rest.

Next, the Agency's verification phase zeroed in on those compounds detected in the screening phase. GC was the prescribed methodology with some specified Quality Assurance/Quality Control (QA/QC) procedures. Again, industry split samples with Agency contractors. Dow evaluated the variability in the data generated in its laboratories compared to the Agency's contractor.[7] We concluded:

1) Qualitatively, the data were comparable to the screening phase data.

2) Quantitatively, about 50% of the time, the Agency will report a result greater than or equal to the values reported by Dow—10% of the time, the Agency's value will be 14 times greater than Dow's.

Early in 1979, the Agency came to the realization that its data bases left something to be desired. In August 1979, the Agency published a notification of a proposed self-monitoring program. The Agency proposed that long-term (30-60 day) effluent monitoring be performed and raw waste load data be provided by individual companies, at each company's expense.[8] The self-monitoring program died because of legal, economic and technical reasons. In its place was conceived a long-term treatability study, which has become known as the "Five-Plant" Study.[9] This joint study between industry, CMA and the Agency, in addition to treatability data, was designed to yield process and analytical variability data. The study began in June 1980 and ended in June 1982.

The Five-Plant Study did yield useful treatability data. Well-operated biological waste treatment plants remove toxic pollutants. The House of Representatives Oversight Committee[10] must have anticipated the Five-Plant Study results in their 1981 report:

> ". . . to a degree not anticipated, the conventional secondary treatment technologies employed by municipalities and many industries have succeeded in removing most of the priority pollutants in a highly efficient manner."

A more important finding of the Five-Plant Study is the variability of the analytical data. Both intra- and inter-laboratory variability data were generated within and between the industrial laboratories, the CMA contractor laboratory and the Agency contractor laboratories.

The intra-laboratory variability shows large errors at the 95% confidence level (Base/Neutral Group). The range of numbers measured for 100 ppb was 23-428 ppb. The range in concentration for a second single value doubled. The inter-laboratory variability at the 95% confidence level for 100 ppb sample ranged from 17 ppb to 599 ppb for a single analysis and the range doubled for a second single value.

These are among the first hard data showing the uncertainty in measurements done on real industrial effluents. This is the type of variability one finds in the real world measurements. And, the study got high marks from the Agency's Science Advisory Board.

Finally, in the overlapping effort, the Agency proposed analytical methods for the priority pollutants as required by Section 304(h) of the CWA, on December 3, 1979.[11] The Environmental Monitoring Support Laboratories (EMSL) in Cincinnati, Ohio is conducting 20 lab round-robins (contractor labs) to determine the variability of data generated from these Methods. There are 13 GC methods for the priority pollutants or 2 GC/MS methods. All of the round-robins have not yet been completed.

An extensive statistical analysis of the data for 2-chloronaphthalene determined by Method 608 has been performed by Mr. William Prescott, American Cyanamid Corp.[12] In real industrial affluent matricies, for example, at 100 ppb the range of values is between 30 ppb and 170 ppb. This represents an error of about 70% at the 95% confidence interval.

Now, let me turn legalistic again. The consolidated permit regulations, which carry the full force and weight of the law, require that analytical measurements be made with 99% confidence.[13] For 2-chloronaphthalene, measured by Method 608, the error at the 99% confidence level is 105%. Therefore, the range of expected values for 100 ppb standard is 0-205 ppb based on the Agency's 20 lab round-robin data.

Now, apply this to an actual compliance measurement. Suppose your National Discharge Elimination System (NPDES) permit allows you to discharge 100 ppb 2-chloronaphthalene. Your analysis using Method 608 measures 180 ppb. Are you now out of compliance with your permit? Probably not, but because the permitting process does not recognize ranges of numbers, but rather only numbers, you must report a non-compliance and your company will receive some form of civil sanction, possibly a fine.

If you do not report this apparent noncompliance, then the analytical chemist opens himself to possible criminal sanctions which could result in personal fines and/or prison.

One more example—assume the same conditions as before, only now you have a compliance audit by the Agency. The sample is split, your laboratory reports 50 ppb for 2-chloronaphthalene, but the Agency laboratory reports 150 ppb. Enforcement action is taken and your company is cited for noncompliance so you go to court. Remember Chief Judge Coffin's ruling. You bear a heavy burden to show the Agency's data are "infected with fatal flaws."

Until the rulemaking processes and courts recognize that analytical results are not numbers but ranges of numbers, then the foregoing scenario is altogether possible.

I realize that nothing I've said today is new. Emich dealt with uncertainty in 1918,[14] and Feigle[15] developed the "region of uncertain reaction" in the 1930's. But only recently have eminent analytical scientists, such as Professor L. B. Rogers, University of Georgia,[16] attempted to relate these basic uncertainty principals to the regulatory rulemaking process.

CONCLUSION

In conclusion, the variability of analytical data bases used in the development of a regulation must be acknowledged and taken into account in the enforcement of that regulation.

Inter-laboratory variability is important in compliance inspections where different laboratories analyze split samples.

Unless regulatory compliance is closely tied to statistically meaningful analytical results, then we might as well settle compliance disputes by a flip of the coin or role of the dice.

REFERENCES

1. Wessel, M. R., Science and CONscience, (New York: Columbia University Press, 1981).
2. Guidelines for Data Acquisition and Data Quality Evaluation in Environ Chemistry, *Anal. Chem.*, *52*, 2242 (1980).
3. ASTM E-691.
4. BASF Wyandotte Corp., et al. v. Costle, 13 ERC 1193, (1st Cir., 1979).
5. "Analysis of Chemical Wastewaters for the EPA Priority Pollutants," Radian Inc., McLean, VA, 79-217-001-003 (September 20, 1979).
6. Natusch, D.F.S., "Priority Pollutant Sampling and Analysis Protocol: An Evaluation of the Procedures Employed," Chemcial Manufacturers Association Seminar on Priority Pollutants, Norfolk, VA, January 16, 1980.
7. Kagel, R. O., McAdams, J. J., Park, C. N., Analytical Variability and Priority Pollutant Analysis - Industrial Perspective, Water Pollution Control Federation Conference, Houston, Texas, October 9, 1979.
8. 44 *Fed. Reg.* 47113, *et. seq.*, August 10, 1979.
9. "CMA/EPA Five-Plant Study," Engineering-Science, Inc., April 1982.
10. Implementation of the Federal Water Pollution Control Act; Report by the Subcommittee on Oversight and Review of the Committee on Public Works and Transportation, House of Representatives, December 1980, page 36.
11. 45 *Fed. Reg.* 69526, *et. seq.*
12. Prescott, W. B., private communication.
13. 45 *Fed. Reg.* 33290, *et. seq.*
14. Emich, F., Ber. 43:10 (1919).
15. Feigle, F., "Chemistry of Specific Selective and Sensitive Reactions," New York: Academic Press (1949).
16. Rogers, L. B., "Validation of Analytical Data: Problems Encountered at Trace-Level Concentrations," Chemical Manufacturers Association Seminar on Priority Pollutants, Norfolk, VA, January 16, 1980.

FUNDAMENTAL RESEARCH AND THE DESIGN OF ENVIRONMENTAL LEGISLATION

by
Clifford S. Russell
Resources for the Future
Washington, D.C.

One of the purposes of this conference is to persuade NSF, EPA, and other sources of research funding that fundamental research in environmental engineering has made and can continue to make valuable contributions to "better environmental decision making" (from the conference announcement). The fact that such a conference is thought to be desirable is itself a comment on the current design of U.S. pollution control legislation. It reveals the existing lack of incentive to seek long run improvements in technology; in our understanding of physical chemical and biological processes; and in our ability to determine whether pollution souces are complying with applicable permit terms. This lack can in turn be traced to the characteristics of the major laws:

— an uneasy mix of incentives *discouraging* technological advance (the rachetting down of standards) and of rhetoric proclaiming technical advance to be a major goal;
— a stated concern with ambient quality overlain and confounded by a reliance on technology— based standards and toleration, for regulatory purposes, of overly simple models of the environment;
— substantial neglect of the severe difficulty of defining and monitoring for compliance in a stochastically varying world using imperfect measurement techniques.

The debate about policy reform (among researchers as opposed to interested parties) has itself too often been oversimplified, however. This debate has focussed on two extreme and often poorly defined positions: an impractically complex, but statically efficient, effluent charge system; and a "command and control" system of regulation which amounts to a caricature of the existing laws. Some broadening and deepening of this debate would be desirable—though one despairs of it having any effect in the Congress.

Here, for example, are three suggestions for changes in emphasis and detail:

1. Ambient quality standards should consistently be placed at the center of the system, not paired awkwardly with treatment technology. The standards should be set (perhaps by the Congress itself, as have the auto emission standards) for indicators that can be tied both *back* to discharges and *forward* to human activities and ecological health. These ties should be established using the best natural systems models we can develop (the "best", not necessarily the most complex).
2. By region these standards and models should be used to establish fixed *totals* of allowed discharges by type of residual. These totals should be embodied in *tradable* discharge permits (rather as EPA is slowly taking the air pollution control system now). Either the total of permits or the trading rules should have built in some protection against the future development of "hotspots."
3. Monitoring requirements should be designed to detect violations of permit terms with some acceptable probability rather than simply to fulfill notional schedules of site visits.

I would not claim that these ideas are new, radical, or even the only desirable way to go. I do think that they have several very desirable properties, especially in fixing our attention on ambient results and placing long term incentives for compliance and technical improvement on individual polluters. Also in the long run, the tradable permit system allows decentralized adjustment to growth and change—inevitable features of our world which are already creating severe strains for the current system.

Finally, it should not be without interest to this group that such a departure in our environmental legislation would create demands for fundamental research:

— by dischargers for improved treatment and process technology to reduce their costs in the long run;
— by agencies and dischargers for improved natural system model to make the forward and backward connections;
— by agencies and dischargers for improved monitoring equipment—meaning cheaper for the same precision and other characteristics, or remote, or continuous and recording, or all of the above.

Thus, the world would beat a path to the door of environmental engineering professors rather than inventing polite excuses when you come round cap in hand for more research funding.

DISCUSSION

Dr. C. O'Melia
(The Johns Hopkins Univ.)

For Kagel:

Dr. Kagel, I have some reservations about the results you presented. Could you describe the basis for your statement that the Science Advisory Board was enthusiastic about the CMA report?

The Science Advisory Board said that the report incorrectly calculated intra- and inter-laboratory variability. Secondly, it said that, where the results were presented, most of the data were at concentrations below practical detection limits. The calculations which you presented, compared those variabilities, which were wrong in the first place, by extrapolating them to higher concentrations. My position is that much of what you presented is not based on fact.

Kagel's reply:

1. My remarks about the Science Advisory Board's (SAB) review of the EPA/CMA Five Plant Study are based on the verbal communiations between myself and the CMA people who presented the Study to the SAB. Their impression was that the Study was given high marks by the SAB.

2. I understand that there is a professional disagreement between CMA and SAB about the statistical treatment of data. In a sense, the data do represent apples, oranges and a few kumquats. This is primarily due to EPA's choice to use GC, rather than GC/MS, which the industry and its contractor used. However, this is representative of real-world situations that happen everyday in the development and enforcement of regulations.

Because this professional disagreement exists between the SAB and CMA over the Five Plant Study, I chose not to use these data to illustrate what can happen if analytical errors are not taken into account. I used a statistical analysis of the EMSL/EPA 20-lab round robin data to illustrate this point and that, sir, is fact.

Dr. D. Jenkins
(Univ. of California, Berkeley)

For Kagel:

When you made the comment that well-operated biological treatment plants effectively remove priority pollutants, do you include in your definition of removal transfer to the atmosphere or adsorption on sludge without degradation?

Kagel's reply:

Yes, removal included aeration of volatile organics, adsorption on the sludge and some biodegradation. We don't know how much of each occurs. (Note added: There exist no good data on mass balance of such systems. This is an area just ripe for some good research.)

Dr. W. Glaze
(Univ. of Texas, Dallas)

Comment:

When the 5-plant study was done, most of the data were taken during summer, with one point in October. There was no winter data. So saying what you said is an over generalization of the facts. Not only did you not know into what compartment the priority pollutants went, if they were removed, you do not know whether what you said is true over a full year's operation.

Kagel's reply:

Yes. The Five Plant Study was not designed for winter operation.

Dr. W. Snodgrass
(The Johns Hopkins Univ.)

For Kagel:

If treatment plants effectively remove priority pollutants, are there any research needs remaining?

Kagel's reply:

Not all priority pollutants were included in the Five Plant Study. Only about 60 priority pollutants were studied. There is more research to be done in this area.

Comment: William H. Glzae, Ph.D., The University of Texas at Dallas.

As a consultant to the EPA Science Advisory Board on this subject, I feel compelled to reply to some of the specific aspects of Dr. Kagel's presentation. Specifically, I refer to his contention that the Five-Plant Study showed that

— ranges of analytical methods for priority pollutants show such poor intra-laboratory variability that "The range of numbers measured for 100 ppb was

23-428 ppb" and the "range in concentration for a second value doubled."

— the inter-laboratory variability for 100 ppb sample "ranged from 17 ppb to 599 ppb for a single analysis and the range doubled for a second single value."

— "Well-operated biological waste treatment plants remove toxic pollutants."

In Dr. Kagel's presentation at the AEEP Conference, he showed summary slides to emphasize these points. These were apparently taken from the report of Engineering-Science, Inc. on the Five-Plant Study[1] which is referenced in his written transcript.

In mid-1982 the EPA Science Advisory Board was confronted with statements such as these and commissioned its Environmental Engineering Subcommittee to seek advice on the subject. The author of this rebuttal, together with Drs. R. Christman and D. Millington of the University of North Carolina was subsequently asked to analyze both reports which issued from the effluent studies of the organic chemicals industry. Summaries of our findings were presented to the SAB Subcommittee on July 30 and September 22, 1982. We severely criticized both the EPA Consultants Report of the verification phase[2] and the E-S analysis of the Five-Plant Study.[1] Appropriate to the remarks of Dr. Kagel, we pointed out that Engineering-Science Inc in our opinion had taken great license in analyzing the data base of the Five-Plant Study; that the Five-Plant Study was not appropriately designed to measure intra- and inter-laboratory precision of analytical methods for trace organics in industrial waste streams; and that E-S analysis pooled data for different compounds, from different matrices, using different analytical techniques, to come to its conclusions concerning intra- and inter-laboratory precision. Moreover, we pointed out the E-S report took a very meager data base on effluents generally obtained at concentrations below 10 ppb, pooled it inappropriately (see above) and then drew conclusions which were extrapolated to higher concentrations (100 ppb). In effect, E-S took a limited amount of data at concentrations which may very well be below the practical lower limit of detection of the methods,[3] and concluded dogmatically on the basis of this data that the analysis of priority pollutants at the higher concentration could not be done with acceptable precision.

Finally, although not the subject of our presentation to the EPA/SAB, we recognized that the Five-Plant Study was also not appropriately designed to study the efficacy of biological oxidation processes for removal of priority pollutants from industrial wastewaters. The study did not cover severely cold periods; only 60 of the 115 organic priority pollutants were examined in the study; and all of these were not examined in all five of the participating plants.

It is important that the recipients of these proceedings realize that Dr. Kagel's case study, chosen to illustrate the importance of statistics and error analysis in rule making, is itself a case of poor use of these principles. It may be that analytical methods for the measurement of trace concentrations of priority pollutants are grossly inadequate for the task to which they are to be put. But the reports from the verification phase or the Five-Plant Study cannot be used to show this is the case.

References

1. "CMA/EPA Five-Plant Study," Engineering-Science, Inc., Austin, Texas, 1982.

2. "Contractor's Engineering Report: Analysis of Organic Chemicals and Plastics/Synthetic Fibers Industries," U.S. Environmental Protection Agency, Effluent Guidelines Division, Washington, D.C., 1981.

3. G. Stanko, presentation at EPA/SAB Committee on Environmental Engineering meeting, September 22, 1982, Washington, D.C.

Reply: Ronald O. Kagel, Ph.D., Dow Chemical USA

I was involved with the CMA Five Plant Study at the inception and early planning stages and assisted in the development of sampling collection and analytical protocol. However, I was not aware of Dr. Glaze's report to the EPA Scientific Advisory Board, and to the best of my knowledge no one in industry has received a copy of this report.

The CMA/EPA Study plant was mutually agreed upon by EPA and CMA. The Study was never designed to be a 365-day (winter operations) study. The three objectives of the study are clearly stated in the Engineering Science Report:

"(1) assess the effectiveness of biological wastewater treatment for the removal of toxic organic pollutants;

(2) define the accuracy, precision, and reproducibility of the analytical methods used for measuring toxic organic pollutants in inorganic chemicals industry wastewaters; and

(3) ascertain if there is a correlation between biological removal of toxic organic pollutants and conventional and nonconventional pollutants."

The SAB concluded that two of the three objectives were met, but the statistical treatment of data was compromised. This represents a professional disagreement. The SAB conceded that if any of the objects of the Study had to be compromised, it should be the statistical treatment of data.

It is not true to say that the Study was not well designed. Over 90 percent of the analytical data was cross-checked by GC/MS in another lab or run in replicate. Less than 10 percent of the analytical data represent single values. In fact, for 60 data points, deuterated surrogate compounds were added to the samples and analyzed in replicate. I know of no other studies in the literature that would meet these criteria. The bottom line is that there exists a professional difference of opinions about one section of the Engineering Science Five Plant Study Report. The Engineering Science Five Plant Study Report stands, as published, and I stand on the data which I presented at the AEEP/NSF Conference. Suffice to say, it is perfectly acceptable practice in statistics to use a function of the detection limit in such cases. Based on Mr. Stanko's statistical treatment of rank-order data (done after the SAB review), the concentration levels that define the 99 percent confidence interval are very similar to those published in the Engineering Science report. The conclusions of the Five Plant Study did not change.

As for my statement regarding the removal of toxic pollutants by well-operated treatment plants, I believe that this is adequately explained in my response to Dr. Jenkins' question.

Dr. Glaze contends that my presentation was biased because I included the CMA/EPA Five Plant Study data. I could hardly exclude these data because they do, in fact, represent one of the data bases under consideration in the development of Effluent Guidelines for the Organic Chemicals Industry.

At this point in time, it is unimportant whose data are right or whose data are wrong. What is important is that the variability in real world data is large, in some cases too large in my opinion, to be the basis of a regulation. In this respect, I completely disagree with the concluding paragraph of Dr. Glaze's rebuttal.

Editor's Note:

Dr. Glaze's comment and Dr. Kagel's reply, as presented, are summaries prepared by the Editor due to the voluminous material received. Access to the original comments and correspondence may be obtained by a request to the Editor. A nominal copying charge will be made for this service.

FUNDAMENTAL RESEARCH NEEDS IN WATER MANAGEMENT

Conference on Fundamental Research Needs for Water and Wastewater Systems

FUNDAMENTAL RESEARCH NEEDS IN WATER TREATMENT OF SOLUBLE MATTER

by
Vernon L. Snoeyink
Department of Civil Engineering
University of Illinois
Urbana, IL
and
Philip C. Singer
Department of Environmental Sciences and Engineering
University of North Carolina
Chapel Hill, NC

INTRODUCTION

Significant advances have recently been made in the field of water treatment, especially in the area of treatment to control trihalomethane formation, but still many questions remain. The principal questions which can be used to organize the research needs can be listed as follows:

1. What contaminants are present in raw waters that are used for water supply and which are formed during the treatment of drinking water?
2. What are acceptable levels of impurities in drinking water?
3. How can undesirable levels of these impurities be reduced to acceptable levels at reasonable cost?

These questions may never be answered completely, but the degree to which research enables us to better answer one or more of them is a measure of the value of that finding. Thus these questions give us a means to prioritize research needs.

This paper presents a brief discussion of these issues as they pertain to dissolved impurities in drinking water. The discussion is divided into two major sections dealing with organic and inorganic contaminants in drinking water.

ORGANIC CONTAMINANTS

Type and Concentration or Organic Impurities in Raw Water Supplies and Which Form During Treatment

Improved techniques for the identification of specific compounds and groups of compounds in water continue to be an important research need. Especially important are techniques to identify the oxidized, polar compounds which form as a result of treating water with chemical oxidants to destroy nuisance substances and to disinfect the water. The techniques available for such measurements are not nearly as good as those for the volatile substances, or the hydrophobic, readily-extractable fractions which are present.

Identification and characterization of various fractions of compounds that are present in raw water are also very important needs. Much of the organic matter in natural waters falls into the category of humic substances. These materials are of importance because of their reactions with water treatment chemicals, the metal ions and organic compounds which may be associated with them and carried through the drinking water system by them, and their effects on the removal of other compounds. We need techniques to characterize this organic matter according to the reactions which they undergo and the effects which they have on treatment processes. The application of such techniques to raw water supplies would allow us to predict in advance their reactions and effects. The oxidants ozone, chlorine dioxide, and chlorine then could be more knowledgeably applied, and the removals that could be achieved by coagulation and adsorption, and the competitive effects on the removal of other organic compounds could be predicted.

Especially important is a technique to determine the biodegradability of the compounds that are present both before and after a given treatment. The emphasis which is now being placed upon application of the least chlorine possible makes this technique particularly desirable and necessary. As we reduce chlorine, we will experience more problems with biological slimes in distribution systems, and the corrosion and taste and odor problems that accompany such slimes. In many locations in Western Europe, the production of water that is biologically stable is an important objective of treatment. However, it is difficult to assess biological stability because an appropriate technique for determining the concentration of very low levels of biodegradable material does not exist. Two techniques are now being evaluated in Europe to accomplish this, including the growth rate of a pure culture of microorganisms on the organic fraction in question, and the growth rate of a mixed culture of microorganisms acclimated to the water in question.

We need more research on the nature of the compounds that form from the organic compounds present in water supplies when these waters are treated with chemical oxidants. The reactions of chlorine have been studied extensively but we are still ignorant of many of the reactions that could take place under different water treatment conditions, especially those which may occur when chlorine contacts organic compounds on surfaces such as anthracite and activated carbon. The surfaces may have a catalytic effect and this phenomenon has largely gone unstudied. Far less is known about the chemistry of ozone and chlorine dioxide when reacting with organic compounds; replacement or supplementation of chlorine in water treatment with either of these is a very definite possibility so more must be learned about them.

Group parameters are especially important for the efficient operation and control of a drinking water treatment plant. Specific compound analysis is useful in supplementing the information obtained from these measurements but the time and expense of specific compound analysis often makes it difficult to use them for operation and control. Group parameters such as total organic halogen (TOX), total organic sulfur (TOS) and UV absorbance hold much promise for use in water treatment. TOX may be especially significant if it can be shown that many of the potentially toxic of carcinogenic compounds are included in this group.

Acceptable Concentrations of Organic Compounds in Drinking Water

The question of what concentration of a compound is acceptable and which is not is perhaps one of the most difficult to answer. Evaluation of the current drinking water standards give little assistance in this regard. Currently there are primary maximum contaminant limits set for 6 pesticides, which are rarely found in drinking water, and total trihalomethanes. Under consideration are limits for 6 volatile organic compounds which are commonly found in contaminated ground waters. The absence of many potentially toxic compounds from this list does not indicate they are of no concern of drinking water; the drinking water standards are based on the assumption that the safest available source of supply will be used and that the contaminants in this source can be removed by conventional treatment. Direct reuse of wastewater is excluded by water supply regulatory agencies because it is classified as an unacceptable source. A recent National Academy of Sciences report deals with the procedures that should be followed to determine whether a wastewater has been satisfactorily treated for reuse.[1] This proposed procedure involves specific compound analysis followed by a series of mutagenicity and toxicity tests, each carried out in comparison to an existing supply with which an acceptable risk is associated. No attempt would be made to determine acceptability based upon absolute values of toxicity. Research to show the applicability of this procedure seems especially important. It should also be used to evaluate the role of various processes in reducing risk.

In the United States, it is generally assumed that a waste-water discharged to a surface water constitutes a far less serious problem than waste-water which is recycled directly to the water treatment plant, even if the waste-water constitutes only a small percentage of the total water supply. The inadvertent reuse involved in waste-water discharge to rivers followed by abstraction for water supply is common and in most cases, the treatment provided does not include steps to remove organic compounds other than those which cause odor and color. We need to better establish the risk associated with use of such supplies; perhaps the National Academy of Sciences Committee recommendations for direct reuse should be applied to water supplies which have significant percentages of indirect reuse.

Production of an Acceptable Water Quality From Water Containing Undesirable Concentrations of Organic Compounds

There are many processes available for removing organic compounds in municipal and point-of-use treatment units. These include chemical oxidation, adsorption, precipitation, aeration, membrane separation processes, or a combination of the above. In most instances where an organic compound removal process would be used, the types of organic substances present, the inorganic composition of the solution and the current state of our knowledge are such that the best way to determine process efficiency is to operate the process on the water to be treated. The result is information on how well the process worked at the site in question, but often little information is obtained that is transferable to another location. The application of activated carbon to ground and surface waters, for example, requires pilot testing for good design because of the competitive interactions of compounds, and the biological growth that may develop and affect removal performance. Pilot plant studies are costly to conduct because of their size, the need to do them on-site, and the limited number of factors that affects the process that can be evaluated during any given run. An important research goal would be to develop techniques for analyzing the organic compounds in the raw water such that the performance of large scale systems can be predicted without lengthy testing of pilot systems. Important research in this area includes the relationship between the thermodynamic properties of a given species and its removal characteristics by processes such as adsorption, membrane separation and aeration. Equally important are techniques for classifying the compounds into groups, each of which affect the adsorption or removal process in a certain way. The approximation of a complex mixture of organic compounds by a simple mixture of three to five compounds of specified characteristics appears to be a particularly effective way of dealing with adsorption from a heterogeneous mixture of compounds. Sontheimer and co-workers have been developing this technique over the past few years and have been obtaining good predictions of adsorption in the absence of biological activity. Further development is needed, however.

DISSOLVED INORGANIC CHEMICALS IN DRINKING WATER

Despite the increased concern regarding organic compounds in drinking water over the past eight years (since New Orleans and the discovery of trihalomethanes in finished water), inorganic species still represent a significant concern with respect to the quality and safety of our drinking water. The EPA National Interim Primary Drinking Water Regulations promulgated in December 1974 established maximum contaminant levels for ten inorganic contaminants: arsenic (0.05 mg/L), barium (1.0 mg/L), cadmium (0.01 mg/L), chromium (0.05 mg/L), fluoride (1.4-2.4 mg/L, depending upon temperature), lead (0.05 mg/L), mercury (0.002 mg/L), nitrate (10 mg/L as N), selenium (0.01 mg/L), and silver (0.05 mg/L). Additionally, the National Secondary Drinking Water Regulations promulgated in July 1979 established secondary maximum contaminant levels for chloride (250 mg/L), copper (1 mg/L), iron (0.3 mg/L), manganese (0.05 mg/L), sulfate (250 mg/L), and zinc (5 mg/L). The MCLs for the primary contaminants are all based on health considerations while the secondary MCLs are based primarily on aesthetics.

The fundamental themes underlying research needs in the area of dissolved inorganic chemicals in drinking waters are more difficult to isolate due to the wide chemical diversity of the types of inorganic species encountered in natural waters. Nevertheless, there are some basic concepts which are worthy of consideration when enumerating research needs dealing with dissolved inorganics in drinking water. With regard to the trace elements (As, Ba, Cd, Cr, Pb, Hg, Se, and Ag), there is a general lack of information as to the efficiency of conventional and nonconventional treatment processes in removing these contaminants. This is due in large part to a lack of information as to the chemical form in which the elements are found, i.e., dissolved or particulate, complexed or free, organic or inorganic, anionic or cationic. Additionally, many of these elements can be found in several different oxidation states. For example, arsenic can be found in both the +III and +V oxidation states, as the anions AsO_2^- (arsenite) or AsO_4^{-3} (arsenate), or as organic arsenicals. Selenium occurs in the +IV or +VI oxidation states, as the anions SeO_3^{-2} (selenite) or SeO_4^{-2} (selenate). Mercury can be found as organic mercurials such as methylmercury, mercurous mercury Hg_2^{1+}, the free mercuric ion Hg^{2+}, or the hydroxo and chloro complexes $HgOH^+$ and $HgCl^+$, respectively.

In addition, all the dissolved species are capable of being adsorbed by particulate material in natural waters, such as clays, iron and manganese oxides, and algae. The extent of such adsorption depends to a significant degree on factors such as pH, the oxidation state and charge of the element, the presence of competing major cations such as Ca^{2+}, the presence of dissolved organic material which may either enhance or retard adsorption, and the nature of the adsorbing particulate surfaces themselves.

Another major factor contributing to our lack of information as to the removal efficiency of various treatment process with regard to trace inorganic substances is that most of the conventional treatment processes which have been investigated are processes which are developed and designed for treatment objectives other than the removal of trace inorganic contaminants. For example, coagulation is directed at the removal of turbidity and color, but ferric iron and aluminum have been shown to effectively remove arsenic and lead. Lime-soda softening is aimed at calcium and magnesium removal, but lead, cadmium, mercury, and silver are also effectively removed by the process. While ion exchange is employed for water softening and demineralization, it is expected and has been shown to some degree that cation exchangers can remove silver, cadmium, barium, and mercury, all of which occur primarily in cationic form, while anion exchangers can remove arsenic, selenium and chromium (VI). Even activated carbon which is directed at the removal of dissolved organic impurities, has been demonstrated to remove chromium, cadmium, silver, mercury, zinc, and nickel.

Clearly, there is a need to more precisely define the speciation of these trace inorganic substances in waters which serve as sources for drinking water, as well as in natural waters in general. It is the chemical form of the elements which govern their transport in aquatic systems and their subsequent behavior in water treatment processes. Through the wide-spread use of atomic absorption spectrophotometry with its graphite furnace and complexation-solvent extraction features, we are in a position today to measure trace element concentrations at very low levels. However, AAS provides information only about the total concentration of a given trace element. Attempts have been made to fractionate water samples into dissolved and particulate components, and to further subdivide these components, for example, into organically complexed, absorbed, and free metal fractions. These classifications, however, are largely operational and have not been rigorously developed and standardized. Accordingly, while they provide some insight as to the distribution of trace elements in natural waters, they do not provide a sufficiently reliable basis for predicting trace element behavior under drinking water treatment conditions.

With regard to the effectiveness of various treatment technologies to remove trace inorganic substances from drinking water, there is a need for controlled investigations involving the adsorptive properties of clays, hydrous metal oxides, and calcium carbonate toward the various trace elements of interest. The effect of major dissolved inorganic species such as Ca^{2+} and organic components of natural waters should be included in such studies. These studies should be carried out at the low levels at which these trace elements are found in natural waters.

The coupling of various treatment processes should also be considered in making such an evaluation. Most significant among these is the impact of oxidative pretreatment chemicals such as chlorine, ozone, chlorine dioxide and permanganate on subsequent treatment processes. The application of such oxidants can be expected not only to change the oxidation state of the trace element under consideration, but can also be expected to alter the nature of organic substances in the water, thereby affecting its complexing properties toward the trace elements, both in the solution and particulate phases.

Nitrate and fluoride are additional dissolved inorganic substances including in the National Interim Primary Drinking Water Regulations. Groundwaters from shallow wells often have high levels of NO_3^-, due primarily to infiltration from animal feed lots, crop lands, and septic tanks. Conventional treatment technologies, except for ion exchange, are ineffective in removing NO_3^- to acceptable levels. While some strong and weak base ion exchange resins are relatively selective toward nitrate over other monovalent anions, the relatively high operating costs associated with ion exchange are leading researchers toward evaluating the applicability of biological denitrification as a means of eliminating nitrate from drinking water. Similarly, defluoridation is a relatively expensive process; at present, activated alumina is the method of choice due to its high sorptive capacity and selectivity for F^- compared with other adsorbents.

In addition to the inorganic species regulated by EPA, the Safe Drinking Water Committee of the National Research Council presented more than 250 published pages of information dealing with 16 trace metals and 6 other inorganic solutes.[2] Similar considerations regarding the health effects of dissolved inorganic chemicals and the establishment of acceptable levels of these contaminants apply to these agents as discussed previously for dissolved organics. The reader is referred to the critical discussions and the many specific recommendations offered in this valuable reference.

REFERENCES

1. National Research Council, Panel on Quality Criteria for Water Reuse, "Quality Criteria for Water Reuse," National Academy Press, Washington, DC, 1982.
2. National Research Council, Safe Drinking Water Committee, "Drinking Water and Health," National Academy Press, 1977.

FUNDAMENTAL RESEARCH NEEDS IN WATER TREATMENT—PARTICULATE REMOVAL PROCESSES

by
Joseph A. FitzPatrick
Department of Civil Engineering
Northwestern University
Evanston, IL

Introduction

Water Supply Engineering is a classical branch of sanitary engineering. Treatment is provided to protect public health from microbial infestations, eliminate toxic substances and improve aesthetic quality of a consumer's water. Through improved fundamental advances we are realizing the more intimate linking between these elements although we do not recognize all those linkages by any means. Considerable research on trace pollutants in the past decade has shown that many are associated with particulates. Microbial pollutants are generally colloidal material and unaesthetic turbid water results from colloidal light scattering. We still however have neither sufficient nor fundamental understanding of the "particle physics" to efficiently and reliably remove these pollutants between source and consumer.

Particle science and particulate (multiphase) process engineering are emerging disciplines and I share the belief with others that the framework in terms of both fundamental and applied understanding has begun to be laid out in this field; this should have far reaching implication for improved understanding and engineering management of our water and air environment.[1] Certainly we cannot afford abandoning basic research on hydrosols without the risk of short circuiting our ability to improve existing technology and its application or create new technology. This discussion will attempt to show what and how fundamental understanding may convert more of our art to science of water treatment.

This presentation is organized around three subjects: particles in water, processes for their removal and the linking of these processes in treatment facilities. This presentation and references will hardly be exhaustive but it is hoped that by tracing some accomplishments will exemplify directions of research needs. Five years ago Charles O'Melia[2] outlined research needs on particles and pollution in water treatment; many of those needs still exist today, and it is tempting to repeat some of his challenges. I will underscore some of their meaning in the course of this paper.

The objective of water treatment is to remove dissolved and supended impurities that have known or strongly linked adverse health or aesthetic effects. Pollutants associated with primary particles or generated by phase change are candidates for removal. The process is accomplished by pretreatment steps followed by solid-liquid separation. Relatively few processes are cost effective (low unit cost) at the scale of municipal water treatment, and continue to be those of convention, i.e., coagulation/flocculation, gravity settling and granular bed filtration.

Why do we need more fundamental research; does the process train fail to remove suspended particles from water? No, but our understanding is insufficient with regard to the suspensions to be removed and the processes to accomplish this. In many cases, we are hard pressed to consistently and economically achieve product water quality with arbitrary low level of particulates. Furthermore, many of the primary and secondary drinking water standards relate to particulates, or particulate form of pollutants and yet we find it difficult to improve our designs to selectively remove any given contaminant let alone optimize removal to meet a list of primary and secondary standards. To go further, we cannot even agree on how to optimize the single conventional unit processes mentioned above and the poor state of basic understanding of these individual processes is brought into even sharper focus when we attempt to optimize performance of conventional and/or novel process linked together in a system.

Particles in Water

In potable supplies, primary particles may come in all shapes, sizes and chemical and biological forms including clays, algae, bacteria, and mixed agglomerates of the same. In treatment, metal oxides/hydroxides, sulfides and carbonates may be generated. They may exist as gross or trace fractions but are usually dispersed at concentrations of no more than a few tens of mg/l. Even as we learn more about them we still find that most pollutants with potential adverse health effects for man are particles or precipitated, adsorbed or coalesced with them; bacteria, viruses, asbestosforms, humics as trihalomethane precursors, heavy metals and many organics.

What do we need to know about these particles in order to remove them? One of our fundamental problems is that in many instances *we don't know.* Recent fundamental research suggests size, shape, e.g., aspect ratio, density, surface chemistry, and aggregate deformability (floc strength) are necessary to predict particle pretreatment and removal from suspension. Many of these parameters are independently distributed in population parameter space and arriving at the necessary joint distribution of properties has only begun. In fact, in aqueous systems, the nature of just the basic particle size distribution has only recently received attention.[3] In the aerosol field, techniques like particle analysis by mass spectrometry (PAMS) are being developed to provide the joint distribution of size with chemical composition; similar rapid measurement techniques need be developed for hydrosols. Similarly, with great interest in removing specific organisms, rapid biological identification techniques are also needed. Overall, there is a significant problem to detect trace particulates by any type of rapid measure. Following this, we need to answer whether these are acceptable levels of particulate pollutants since there is often a non-unique and presently operationally defined association of pollutants with trace or even gross particulates.

Turning to water treatment processes for illustrative purposes it is interesting to note that a number of fundamental properties on which separation efficiency of generated (secondary) particles depend are poorly known. Three particles parameters; size, density and floc strength are noteworthy. Particle size has a most dramatic effect on transport and removal mechanisms. Turbidity is used as an operating control parameter and water quality standard but relation to particle size particularly in distributed suspension is non-unique.[4] Particle size measurement by automatic instruments has reasonable success with gross ($>2\mu m$ diameter) but not with trace($<2\mu m$ diameter) particles. Even then, ability to count and size porous, fragile flocs produced in water treatment is poor and continues to hamper fundamental studies requiring this information. Floc particle density is required to estimate sedimentation potential, scouring of deposits and granular bed filter contact opportunity, yet no adequate instrumentation exists to measure this. Flocs are formed by rate processes of aggregation in bulk solution (flocculation and sedimentation) or at interfaces (filtration or flotation). Deformability of these has significant effect on the efficiency and economics of the above mentioned processes. Recent floc studies in agitated systems have used a basic approach, i.e., floc breakage terms are included in a floc population balance and the dependence of the breakage terms on basic measurable chemical and physical parameters is determined.[5] Similar work is needed in interfacial floc systems. Early experiments used to guide basic understanding have employed clays and hydrolyzing metal coagulants. This work should be extended for other suspensions, solution chemistry and hydrodynamics indicative of other separation processes in water treatment. Closely compared experimental and theoretical studies should be most fruitful.

Model suspensions, particularly narrowly sized ones have been employed for numerous basic separation studies and should continue principally to elucidate chemical e.g., double layer variables in particle removal as well as trace pollutant transport.

Conventional processes for Particle Processing and Removal in Water Treatment

This area embodies most of the published literature on particles in water which is not surprising since the classical objectives of water treatment include pretreatment of turbidity causing particles, e.g., coagulation/ flocculation plus solid-liquid (floc-water) separation e.g., sedimentation, flotation, filtration or straining. Lest we overlook them, chemical precipitation, oxidation and disinfection also bear on particulate pollutants, and generate secondary pollutants, e.g., softening, sulfur oxidation and disinfection may generate hydroxy carbonates, sulfur and detritus respectively. The ranges of applicability of some of these processes have been described didactically[6] for two variables in treatment plant operations in Figure 1. Reality isn't that simple and a more complete list of variables affecting operation of these processes would quickly extend the concept into n-space and blur distinct regions of Figure 1. It then becomes obvious why system models are required to handle multiple mechanism interactions on particles distributed in many parameter space.

In the discussion that follows I address some basic phenomena requiring research for the major processes coagulation/ flocculation, gravity separation and filtration. Emphasis is somewhat arbitrary.

Coagulation/Flocculation/Precipitation

Coagulation and flocculation have often been conceptually described as a two step process; destabilization and aggregation. When hydrolyzing metal coagulants are used, equilibrium chemistry predicts coagulant speciation. How these species interact among themselves and with the colloids to be removed may be computable using equilibrium models e.g., MINEQL.[7] This is but a first step and needs extension in time to describe the kinetics of such reactions. Synthetic polymers often used in water treatment can react with hydrolyzing metal coagulants, themselves, colloids present and even disinfectants like chlorine. Nomenclature and standard test protocol for characterizing polymers and hydrolyzing metal coagulants including polymeric metals[8] is still lacking but needed for equilibrium and kinetics modelling as outlined above.

Particle size strongly affects aggregation transport mechanisms and heterogeneity of suspensions dramatically improves contact opportunity for submicron hydrosols such as viruses.[9] This effect needs more extensive corroboration in real suspensions with distributed size and other population parameters.

A coefficient has been employed in much hydrosol and colloid stability work, namely the use of an A factor to describe the success ratio of colloid-colloid or colloid-floc encounters. Even if correct it is not known how these factors depend on both colloid chemistry and close interaction hydrodynamics in particle clusters. Fundamental approaches might use trajectory analysis of particle aggregate encounters with realistic surface chemistry and hydro-dynamic boundary conditions. Put another way—our present aggregation models are highly physical wilth only primitive chemical and dynamic insight.

Nucleation and crystalization, steps involved in precipitation reactions are important but poorly understood. While these generate secondary particles and may be involved in scale formation the role of nucleates, crystal habit modifiers and fluid mechanics are also needed to understand how trace pollutants may be co-precipitated— or absorbed on precipitating phases.

Gravity Separations

In the past decade, simulation models have been constructed/ adapted to describe dilute phase gravity settling as practiced in water treatment. We have seen a move from the deterministic to more stochastic descriptions including finite difference solutions of turbulence field equations and non-ideal flow. These have added significant potential to understanding the large scale flow in settling basins. Improved coupling of the sedimentation to realistic hydrodynamics remains a major goal and underscoring all of this is an almost desperate need for quality data to guide physical understanding and model improvement.

"New devices" with improved throughput and efficiency per unit volume have various flow configurations and confining boundaries; tubes, plates, channels or grids but the fundamental mechanics of many of these devices is poorly understood and generalization of the "best" new technology is hampered. Particularly where a concentrated phase "sludge" interacts closely with a clarified flow layer the description of multibody hydrodynamics and aggregation of particles may hold the key to developing improved design relations but this will still be many years away without more concerted effort.

One area that has received too little attention in sedimentation theory is scale effects. Practitioners would like to be able to have calculable scale-up factors but basic understanding of settling processes still won't permit it. A rational base for scale-up of settling devices is badly needed.

Filtration

Conventional granular bed filtration is in many ways the most important particle removal step in water treatment; it is often the last step and in direct filtration the only step. The process is called on to yield large volumetric throughput and high capture efficiency, i.e., opposing goals. Filters must polish at often greater than 99% compared to settling basin 50-80% removal efficiency and moreover will be called on to remove more specific size ranges and particle types including virus and bacterial species, specific algae and cysts not to mention other organic and inorganic particulates. Wider application also warrants a broader definition of filtration processes to unify phenomena from as diverse areas as bank or earth filtration, contact or direct filtration, pebble filtration, biological filtration, flotation and fluidization. A number of these have common underlying phenomena which may be elucidated with a more fundamental approach. Greater dialogue between disciplines with common interest in this area is also indicated.

Fundamental studies have focused on clean filters and now permit explanation of effects of several variables individually and collectively during the early period of filtration. However, even there, the prediction of double layer chemical effects for bacteria sized particulates is extremely poor. The large decreases in capture predicted by most theories is just not observed experimentally. Added well-defined experiments are indicated along with improved theory to explain the interaction of field forces e.g., gravity along with classical colloidal forces in liquid filtration. Furthermore, considerable disagreement among researchers over microscopic model detail and inability to predict many practical effects has caused the design community to shun employing mechanistic models. This trend needs to be reversed.

Turning from clean filters to dirty ones, an important area that needs pursuit is the evaluation of deposit on subsequent particle capture. When particles collect in a filter, they act as very efficient collectors for other particles. So, filters are composed of the media we make from pretreating particles and the media we buy to serve as supports for these really effective internal media. Elucidation of the form of this internal media including biological influences is expected to shed much light on predicting dynamic behavior of granular filters.

Optimization of a granular bed filter will be closely allied wilth defining conditions by which distribution of these collectors is manipulated to maximize capture while minimizing development of flow resistance. Some microscopic scale studies recently performed[10] to observe the phenomenon just described yielded complex patterns of pendants and porous pouches that were truly complex. To describe these observations mathematically puts us back to square one and may require qualitatively different approaches.

Mechanistic modelling of non-steady behavior of packed beds have taken at least two main approaches. One approach considers building of particle deposits in a series of dendrites and describing the capture and drag production for their assemblage geometry. A second approach equates increased flow resistance and capture rate to the accumulated production of more collision targets each with a defined interception efficiency. Flow resistance increase is assumed proportional to captured particle surface area. The former appears more rigorous but may require too many adjustable constants while the latter preserves essential physical features but with greater computational ease. Various other approaches need exploration.

Two major areas lacking from a water filtration theory standpoint relate to disaggregation or "decolmatage" and the relative importance of straining and depth filtration which occurs simultaneously for most heterodisperse suspensions. Description of disaggregation, sloughing, floc breakup or reentrainment should be a necessary component of any kinetic model of flocculation or filtration. Models that consider only transport and attachment will continue to have limited applicability in real filters. Methods are needed to measure the forces of adhesion and cohesion of aggregates and deposit within a bed so that pretreatment of packed bed filtration may be "optimized."

Straining of coarse particles in a raw water suspension is likewise an aspect of filtration dynamics that is too frequently ignored in model attempts. Furthermore, both depth and surface filtration occur to varying degrees in every filtration application and surface clogging may be of major importance in shortening the filter run. Here, careful consideration of influent particle distribution will be necessary in modelling efforts.

Innovative Processes for Particle Transport and Removal

The requirement that water treatment processes have a low unit cost to be competitive is a serious stumbling block and significant reason why no "new" general separation processes have been introduced in the past generation. Improvements in classical processes have been evolutionary rather than revolutionary. Major advance has come in applying more efficient pretreatment chemicals. Improvements in flow stream contact with interfaces largely classifies the other improvements. Disappointingly, enhanced field effects have not been very effective. All in all, even though the separations field is young and unifying ideas on the rise[11] it will be very difficult to invent really new inexpensive particulate water treatment processes.

Treatment Plant Performance, Analysis and Control

Improved understanding of the role of hydrodynamics and colloid chemistry in multi-particle systems including real suspensions with distributed sizes will be crucial in developing broadly predictive models for the three common water clarification processes; coagulation/flocculation, sedimentation and filtration. Nevertheless, while waiting for the day of much better models to arrive we need to forge ahead with cruder state of the art simulation models of these water treatment particle processes. Our objectives are to explain both the obvious and subtle responses seen in practice but, more importantly, to investigate how linking the processes in a system provide trade-offs in achieving specific particle removal for various plant size (capital cost) and energy and chemical expenditure (operation and maintenance cost).

This is, in my opinion an area of very fruitful and important research because it provides one of the key missing links between theory and practice. Empirical design and operating rules for treatment plants have been used for a long time and may be counter productive despite the common belief that many processes have been around so long that optimal results have been achieved by trial and error. This is not true, since treatment objective functions have changed rapidly. Systems studies will, in the short run conceptually improve individual unit operation process models and help define important characteristics of product water that we don't yet know or understand. Furthermore, systems models of water treatment plants could be used as a screening tool to separate good from bad choices for further refined study or experiment. Researchers and practitioners alike desire answers to questions like: how much flocculation or settling should be provided; how much solid recycle; is there an optimum size distribution for filtration; are there optimum flocculation reactor designs; can we seed filters effectively and so on. While answers to these may be within conceptual reach these models should not be expected to accomplish definitive design or control of treatment plants at this time.

Three system models are compared on Table 1.[12] These models link the conventional unit processes of coagulation/ flocculation, sedimentation and filtration. The models have different levels of detail in process description and form. The first employs total suspended mass concentration and the latter two use particle population with various assumptions on initial and intermediate size distributions. An evaluation of performance is based on process cost and process sensitivity in conventional engineering terms of effluent quality and headloss or net water production.

Sensitivity analyses of these models for ranges of typical treatment plant operating variables and possible novel applications such as direct filtration and high rate sedimentation, have been made. Some of the findings agree with intuition; there is a concentration of raw water suspended solids that minimizes treatment cost and combinations of flocculation settling and filtration variables maximizes water production. These conclusions are still based on first generation models and assumptions have not been sufficiently verified. Verification at both pilot and full scale is indicated but because many parameters in a detailed mechanistic model have not been historically measurable, this job has only begun.

The development of these models has paralleled practitioner employ in plant control of some of the features of model computations such as the form of the particle size distribution through the treatment processes. In addition, plant engineers have been able to empirically select operating conditions to minimize total particle number or number in given size ranges in finished water. Practitioners and conceptualists need to continue fruitful exchanges of such ideas.

Conclusion

Particulates in water are sources of or associated with trace or other pollutants. Treating these suspended particles and removing them efficiently and cheaply from water remains a difficult task. New theoretical approaches to better understand the hydrodynamics of multiparticle systems are indicated. Improvements are needed in our primitive understanding of colloidal and interfacial chemical roles in agglomeration and filtration. More precise definitions of separation mechanisms are needed to allow generalization and perhaps identification of new processes. System models need further development and testing to synthesize our expanding knowledge on particle movement in and removal from water. Greater technology and applied science transfer among particle scientists, colloid chemists, chemical and mineral engineers and environmental and sanitary engineers is required. Finally, practitioner-researcher communication must be nurtured and expanded if we are to abbreviate the time of application of the concepts discussed here.

References

1. Freeman, M. P. and FitzPatrick, J. A. *Theory Practice and Process Principles For Physical Separations*, Engineering Foundation Publications, AIChE, New York, 1981.

2. O'Melia, C. R., "Fundamental Research Needs in Water Treatment: Particles and Pollution," in Fundamental Research Needs for Water and Wastewater Treatment Systems, Proceedings of a conference sponsored by the National Science Foundation and Association of Environmental Engineering Professors, J. H. Sherrard, Editor, pp. 61-68, 1978.

3. Kavanaugh, M. C. and Leckie, J. O., eds., *Particulates in Water, Characterization Fate, Effects and Removal*, Advances in Chemistry Series 189, American Chemical Society, Washington, D.C., 1980.

4. Treweek, G. P. and Morgan, J. J., "Prediction of Suspension Turbidities from Aggregate Size Distribution," in *Particulates in Water*, Kavanaugh and Leckie, eds., Adv. in Chem. Series 189, ACS, Washington, D.C., 1980.

5. Pandya, J. D. and Spielman, L. A., Kinetics of Floc Breakage in Agitated Liquid Suspensions, Theory and Data Processing Strategies, 90, 2, 517–531, 1982.

6. Stumm, W., Chemical Interaction in Particle Systems, *Environmental Science and Technology*, *11*, 12, 1066-1070, 1977.

7. Iyer, D. R. and Letterman, R. D., "Predicting the Effect of Hydrolyzing Salts on Flocculation Efficiency using Computerized Chemical Equilibrium Models," Annual Conference Proceedings, American Water Works Assn., Denver, CO, 1982.

8. Bottero, J. Y., Poireier, J. E. andf Fiessinger, F., "Study of Partially Neutralized Aqueous Aluminum Chloride Solutions," *Progress in Water Technol.*, *12*, 601-612, 1980.

9. O'Melia, C. R., "Aquasols: The Behavior of Small Particles in Aquatic Systems," *Environmental Science and Technology*, *14*, 9, 1052-1060, 1980.

10. Payatakes, A. C., Park, H. Y. and Petrie, J., "A Visual Study of Particle Deposition and Reentrainment During Depth Filtration of hydrosols with a Polyelectrolyte," *Chemical Engr. Sci.*, *36*, 1319-1335, 1981.

11. Lee, H. L., Lightfoot, E. N., Reis, J. F. G. and Waissbluth, M. D., "The Systematic Description and Development of Separations Processes," in *Recent Development in Separation Science*, N. N. Li

(ed.), Vol. III, Part A, pp. 1-66, Chemical Rubber Co., Cleveland, 1977.

12. Hammer, M. J., Jr. and FitzPatrick, J. A., Review of Simulation Models for Surface Water Treatment Facilities, Annual Conference Proceedings, American Water Works Assn., Denver, CO, 1980.

13. Lawler, D. F., O'Melia, C. R. and Tobiason, J. E., "Integral Water Treatment: from Particle Size to Plant Performance," in *Particulates in Water*, Kavanaugh and Leckie, eds., Adv. in Chem. Series 189, ACS, Washington, D.C., 1980.

14. Gross, D. C., "A Model of the Flocculation-Sedimentation-Filtration Process for Removal of Iron Oxide Flocs," M.S. thesis, Northwestern University, Evanston, Illinois, 1980.

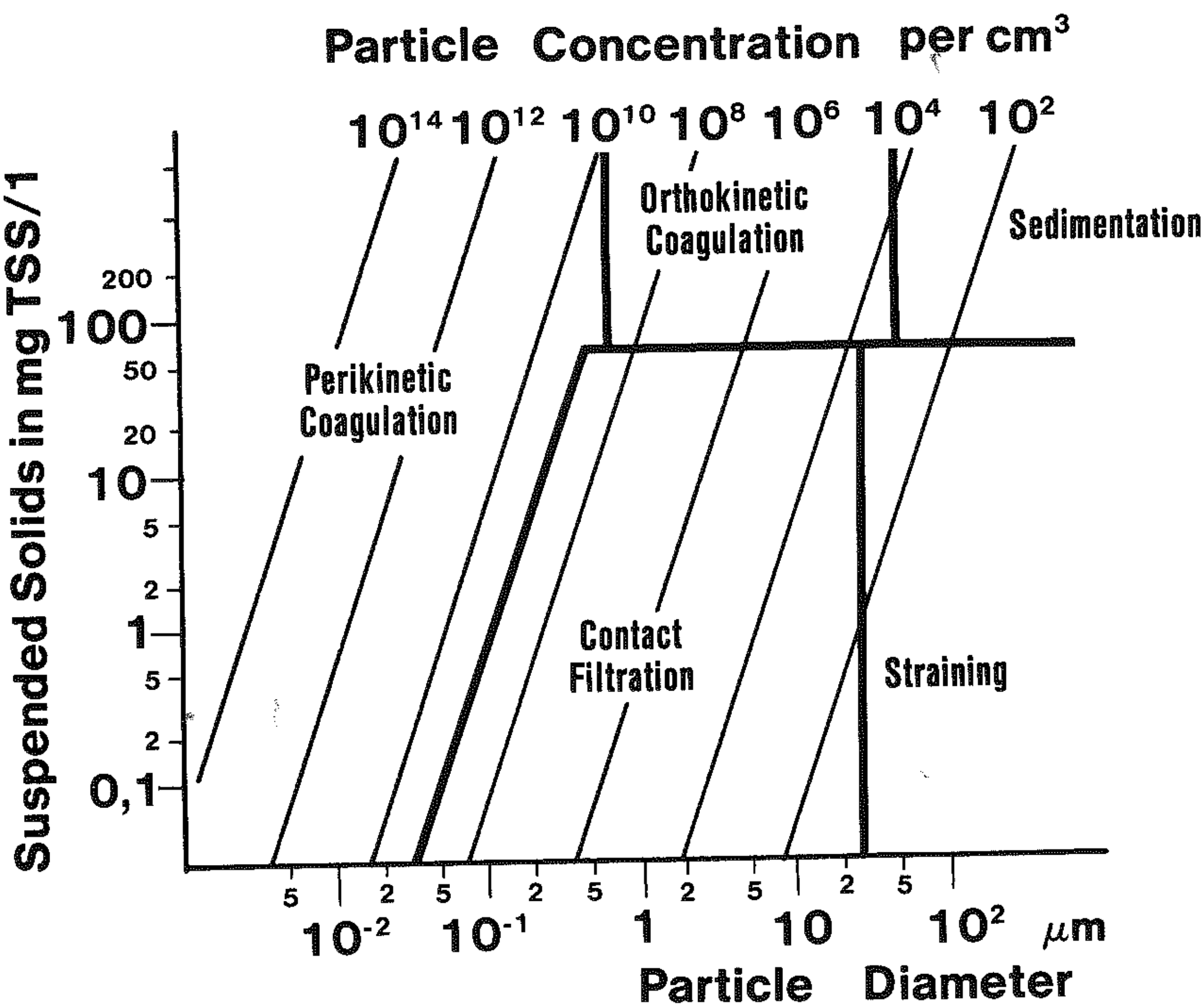
15. Letterman, R. D., and Iyer, D. R., "Process Model Application in Potable Water Treatment," *Theory Practice and Process Principles for Physical Separations*, Freeman and FitzPatrick, eds., Engineering Foundation Publications, AIChE, 1981.

TABLE 1

COMPARISON OF SYSTEM MODELS

Process \ Model	Letterman and Iyer[15]	Lawler et al.[13]	Gross[14]
Suspension	Primary particles, concentration averages, bimodal size in flocculation but not other processes.	Hyperbolic distribution of light or dense solids influent. No size density relation.	Primary particles arbitrary density and size. Lagvankar (size density relation).
Flocculation	First order growth, zero order break-up. Lagvankar (size density relation). Empirical C/C_o vs. coagulant dose.	Smoluchowski: (Brownian and Shear) coalescence, no break-up. Coagulant added mass not determined.	Smoluchowski: (Brownian and Shear) Vold floc aggregation, break-up semi-empirical. Coagulant added mass not determined.
Sedimentation	Stoke's Law. No flocculant settling.	Stoke's Law (4 cells). No differential settling, Brownian aggregation.	Stoke's Law (n cells). Differential settling and Brownian aggregation.
Filtration Headloss	Based on influent solids concentration (empirical).	Mono-sized suspension, Kozeny-Carmen eqn.	Poly-sized suspension, Kozeny-Carmen eqn.
Breakthrough	Not determined.	Not determined.	Breakthrough: semi-empirical.

Fig. 1 Ranges of Application of Various Particle Removal Processes adapted from [6]



FUNDAMENTAL RESEARCH NEEDS IN WATER MANAGEMENT: MICROBIOLOGY

Vincent P. Olivieri and Kazuyoshi Kawata
Department of Environmental Health
The Johns Hopkins University
Baltimore, MD

INTRODUCTION

Microorganisms are responsible for transmission of acute diseases by water and have a noticeable impact on water quality. Fundamental research should be directed toward providing a firm scientific data base to permit an understanding of factors responsible for the removal and inactivation of microorganisms and for the growth and proliferation of microorganisms in potable water systems.

WATERBORNE DISEASE

The application of sound water treatment practices in the United States since the turn of the century has resulted in the near eradication of massive waterborne outbreaks of diseases caused by the classic enteric bacteria. Despite this enviable achievement, more than 50 outbreaks of waterborne disease involving 20,008 cases were reported to the Center for Disease Control (CDC) in Atlanta in 1980 (1). This represents a morbidity of approximately 10 cases/100,000 population. Table 1 shows the waterborne outbreaks from 1971 to 1980. Since the beginning of the current surveillance system, there has been a consistent increase in the number of reported waterborne outbreaks and community water supplies were consistently implicated in a major portion of the outbreaks (36% of the total 1971-79 and 46% in 1980). It should be noted that the consistent increase in reported outbreaks probably is due to the improved surveillance and the reporting system developed by CDC and EPA. However, the number of outbreaks of waterborne disease reported to CDC and EPA was generally considered to represent only a portion of the waterborne outbreaks and does not represent the true incidence of waterborne outbreaks and does not represent the true incidence of waterborne disease. Clearly, methodical, carefully designed epidemiological studies are needed to define the magnitude of the disease that occurs by the water route.

The etiology of the waterborne disease outbreaks in 1980 is shown in Table 2 and provides additional insight to needed research (1). The majority of the outbreaks (56%) were acute gastrointestinal illness (AGI) where no microorganism or other agent was identified. *Giardi lamblia* and Norwalk agent were responsible for 14% each of the reported incidents. The remaining outbreaks (2% each) were caused by *Shigella*, *Campylobacter* and infectious hepatitis. *Giardia*, a protozoan which forms a cyst; Norwalk agent, believed to be a small DNA containing virus; and *Campylobacter*, a bacterium, only recently have been implicated as etiological agents in waterborne disease. Several workers suggest that the Norwalk agent (2) and or rotavirus (3) may be responsible for a portion of the AGI observed each year.

Microbiological Methods

Surprisingly, the methods for the recovery of the microorganisms responsible for waterborne disease in re-

Table 1. Waterborne Disease Outbreaks, by Year and Type of System, United States, 1971-1980 (1)

	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	Total (%)
Community	5	10	5	11	6	9	12	10	23	23	114 (36)
Noncommunity	10	18	16	10	16	23	19	18	14	22	166 (53)
Private	4	2	3	5	2	3	3	4	4	5	35 (11)
Total	19	30	24	26	24	35	34	32	41	50	315
Total Cases	5182	1650	1784	8363	10879	5068	3860	11435	9720	20008	77974

Table 2. Waterborne Disease Outbreaks by Etiology and Type of Water System, 1980 (1)

	Total	
	Outbreaks	Cases
AGI[a]	28	13220
Giardia	7	1724
Chemical	7	2298
Norwalk Agent	5	1914
Shigella	1	4
Campylobacter	1	800
Hepatitis	1	48
	50	20008

[a]Acute Gastrointestinal Illness

cent years are unreliable or non-existent. At present, the nature of the agent for AGI is not clear.

Strains of *Escherichia coli*, generally referred to as enteropathogenic or enterotoxigenic, have been reported to be responsible for disease transmitted by drinking water. Several workers suggest (4) that enteropathogenic *E. coli* may be responsible for the AGI reported in recent years. Serological methods for distinguishing enteropathogenic from other *E. coli* are available but are not well developed for water samples. Norwalk agent and rotavirus also have been implicated in AGI. Methods for these agents consist of laborious concentration procedures followed by immunological or electron microscopic techniques. Reliable cultural methods are not available at present and little or no data base is available to evaluate recovery. The recovery of cysts of *Giardia* from large volumes of water appears reasonable, but evaluation of viability has been limited to hit or miss feeding trials with beagle puppies. Recently, culture methods have been developed to evaluate the viability of cysts in laboratory cultures of *Giardia*. Little work has been done on field samples. *Shigella* has been recognized as a waterborne pathogen since the turn of the century. While culture and recovery procedures are available from fecal specimens and body fluids, reliable methods for the recovery from water are not available. Other microorganisms found in water in much greater numbers are often antagonistic to *Shigella* and inhibit or mask the *Shigella* in enrichment cultures. Infectious hepatitis has also been recognized as a waterborne pathogen but only one outbreak reported the recovery of this agent (5) from water using a radioimmunoassay technique. Cultural methods are confined to propagation in marmosets. Tissue culture techniques are slowly developing. methods are available for the recovery of *Campylobacter* from fecal specimens, but have received limited application in recovery from water. No standard method for routine analysis has been available.

INDICATORS

Early efforts to isolate pathogenic microorganisms from water were unproductive. Prescott and Winslow (6) reviewed the isolation of specific pathogens and summarized the reports to 1904:

> "On the whole it seems that since a positive result is always open to serious doubt, and a negative result signifies nothing, the search for the typhoid bacillus itself, however desirable theoretically, cannot be regarded at present as generally profitable."

While the methods for the isolation of the spirillum of cholera from water were considered less difficult than those for the typhoid bacillus, similar conclusions as to the practical utilization of these methods were drawn. Despite the dramatic improvement in culture media, isolation, identification, and enumeration methods for bacteria, virus and protozoa since 1904, the isolation of specific pathogens still "cannot be regarded at present as generally profitable."The low numbers of pathogenic microorganisms relative to the natural microbial populations and the wide variety of pathogens that may be present are severe limitations to the usefulness of procedures for the enumeration of pathogens. As a result, microbiologists suggested a surrogate microbial determination to indicate the presence of fecal contamination rather than rely on the isolation of pathogens.

The general criteria for the surrogate measurement of microbial quality are listed below. The primary function of the surrogate was to indicate the absence of recent fecal contamination.

1. The indicator should always be present when the source of the pathogenic microorganisms of concern is present and absent in clean uncontaminated water.
2. The indicator should be present in numbers much greater than the pathogen or pathogens.
3. The indicator should respond to natural environmental conditions and water and wastewater treatment processes in a manner similar to the pathogens of interest.
4. The indicator should be easy to isolate, identify, and enumerate.

While the criteria for indicators of microbial quality appeared to be simple and straightforward, no one microorganism or group of microorganisms, chemical or biological test adequately satisfy all of the above criteria.

The early workers recognized the presence in large numbers in feces and sewage of a group of bacteria whose most significant member was *Bacillus* coli. Although the name of the indicator was changed several times over the years, the current coliform group is essentially the same group of microorganisms that has served since the late 19th century as an indicator of the presence of fecal contamination of water. The coliform group is currently defined in the 15th edition of *Standard Methods for the Examination of Water and Wastewater* as "aerobic and facultative anaerobic, gram-negative, nonspore forming, rod-shaped bacteria which ferment lactose with gas formation within 48 hr at 35°C (7).

To date, the coliform group is still the most reliable indicator for potable water. No other organism or group of organisms has been found to more closely fulfill the above criteria for potable water. The recent National Research Council report on Drinking Water and Health (3) states:

> It would be undesirable and extremely risky to substitute any organism for the coliform group now, although research studies that compare other indicator organisms with coliform are warranted.

Aside from the determinative difficulties associated with the evaluation of the presence of groups of microorganisms rather than a single species and the not uncommon false positive, false negative, and atypical reactions, the major deficiencies of the coliform group are listed below:

1. The natural die-away of members of the coliform group differs markedly from the die-away of nonbacterial pathogens. The enteric viruses in particular and probably cysts of pathogenic protozoa survive for longer periods of time in the natural aquatic environment.
2. The response of coliform to conventional water treatment processes, particularly disinfection, also differs from the nonbacterial pathogens. The coliform group appears more sensitive to disinfection than virus and probably the protozoan cysts.
3. The aftergrowth of some members of the coliform group differs from enteric pathogens.
4. The suppression of the growth of coliforms by high populations of other microorganisms influences recovery and enumeration.

Other approaches to surrogate determinations would also be of value. The current coliform determination in potable water requires 24 to 48 hours to obtain presumptive information. Development of a rapid real time indicator would provide data on water quality that would be more useful. Suggested additional rapid determinations include epifluorescence, carbon-14 release methods, endotoxin and adenosine triphosphate determinations, and fecal sterol measurements. Little evidence is available to permit an evaluation of these indicators.

Multiple Barriers

Almost all the outbreaks of waterborne disease reported in 1980 were due to some deficiency in the water system (1). Treatment deficiencies were implicated in 22 of the 50 outbreaks (44%). Most often, several of the multiple barriers to disease constructed over the last 100 years, had fallen down and were coupled with the difficiencies in treatment. While fundamental scientific research will have limited impact on these outbreaks of disease, studies directed toward developing a better understanding of the barriers and processes will provide basic information toward minimizing disease transmission.

Die-Away

Storage has been routinely employed to permit natural die-away of microorganisms in water systems before treatment. Volumes of reports are available in the literature on the survival of numerous enteric and other microorganisms of concern in water under a variety of physical, chemical, and biological conditions. These reports were generally "in vitro" studies with cultured microorganisms performed in the laboratory. Few studies were conducted "in situ" with natural populations of microorganisms. Certainly, the laboratory trials provide a reproducible baseline of data but the "in situ" investigations are necessary to obtain more reliable estimates of die-away in the aquatic environment. Current changes in the sewage disinfection practices permitting seasonal disinfection will add an additional burden of pathogens to the environment. Most of these microorganisms will be found in the sediments. Little data can be found to accurately estimate survival in sediments and little attention has been directed to understand fundamental mechanisms responsible for inactivation of natural populations of microorganisms. These studies should include indicator bacteria, pathogenic bacteria, viruses, and parasites.

Disinfection

The intentional application of biocidal compounds to water and wastewater to inactivate infectious agents has evolved as the primary barrier against the transmission of disease by the water route. In many potable water systems, disinfection is the only treatment before the water is passed to the distribution network. In the United States, chlorine has been almost universally employed for the disinfection of water and wastewater. Recent concern for the formation of deleterious compounds during chlorination has prompted a renewed interest in the chemical reactions of chlorine and in the application of alternative disinfectants. Despite the long history of the use of disinfectants in water, there are decided gaps in our knowledge and understanding of this useful chemical.

Numerous reports are available on the inactivation of microorganisms with a wide variety of biocidal agents but little attention has been directed toward understanding the mechanism of microbial inactivation with these compounds. A better understanding of the reactions responsible for the inactivation will provide a firm scientific base to choose the best disinfectant for a given situation.

The current interest in disinfection has produced a flurry of articles on the efficacy of numerous alternative biocides. Limited studies have been conducted to provide data that would allow thorough and reliable comparisons of disinfectants in clean laboratory experiments and "real world" field studies. At present, information varies from laboratory to laboratory and standard test systems are not employed. The development of standard test procedures is imperative if useful data are to be collected. Included in the test methods should be an internal biological control that can be employed in each experimental trial. Rigorous chemical measurements should be employed to determine the species of disinfectant responsible for the microbial kill. The latter is often overlooked. Wyss (9) accurately assessed the problem in 1960 and noted:

> Of the factors affecting death of microbes through the action of chlorine the one which has ruined more good experiments is that which concerns the disinfectant-wasting side reactions. When someone reports that it takes 20 mg/l chlorine about twenty minutes to kill a reasonable number of bacterial vegetative cells most people who work in this field now understand that during this period probably 19 mg/l of chlorine have been destroyed by some disinfectant-wasting side reaction and that a fraction of a mg/l has succeeded in killing the organisms during the period. Yet papers where this fact is ignored continue to be published by veterans who should know better and by novices who do not.

More than twenty years later reports that ignore the chemistry and reactivity of disinfectants continue to appear in the literature.

The inactivation of parasites in water and wastewater has not received the necessary attention. This major gap in our knowledge is due primarily to the limited methods and techniques for the culture of these organisms and procedures to evaluate viability in control and treated preparations. Reports in the literature are often difficult to interpret since supporting chemical information is scarce and, thus, disinfection data from report to report are contradictory. Reliable inactivation studies are needed for protozoan cysts (*Giardia* and *Acanthamoeba*) and other parasites with halogens and other alternative disinfectants.

There is generally a discrepancy in disinfection results from laboratory to field trials. Cultured microorganisms are more sensitive to disinfectants than natural population. Microorganisms can routinely be isolated from potable water under conditions where disinfectants are present in sufficient concentrations and contact times to yield inactivation. These same microorganisms when cultured in the laboratory, however, yield the expected rates of kill when exposed to the disinfectant. Does the microorganism escape inactivation simply by chance? Are microorganisms "in situ" protected by some mechanism? Do the microorganisms grow differently in the aquatic environment? Can resistant strains to the commonly employed water disinfectants be developed? Certainly the answers to these questions would have a dramatic effect on the choices of disinfectant and disinfection practices.

WATER DISTRIBUTION SYSTEM

The water distribution network is the most costly and most vulnerable portion of a potable water system. Microorganisms can enter the pipe system by a variety of routes ranging from insufficient treatment to breaks in the pipe. The pipe system is not sterile nor was it ever intended to be sterile. Microbial processes within the water distribution network can have a noticeable impact on the quality of water delivered to the consumer's tap. Growth of microorganisms can significantly contribute to corrosion, increase hydraulic roughness, impart undesirable tastes and odors and cause "red" (iron) or "black" (sulfide) water (10).

A large body of information has developed on the growth and proliferation of microorganisms in the humans and others animals. The culture of a wide variety of microorganisms is accomplished routinely in laboratories across the country. However, the details of the factors and conditions that influence the growth of microorganisms in the pipe network remain to be elucidated.

Residual disinfectant in the pipe network provides a relatively effective barrier to growth of microorganisms (11) and some protection against the transmission of disease due to post treatment contamination (12). The maintenance of a chlorine residual throughout the distribution network is sometimes difficult. The nature of the reactions that consume the disinfectant residual is not clear. The factors and conditions that affect the stability of residuals need considerable attention.

SUMMARY

The importance of microorganisms in water have long been recognized. In the United States, the massive waterborne outbreaks common in the past are no longer observed. The etiology of waterborne disease has shifted dramatically from the typhoid and cholera of the late 19th century to AGI and giardiasis. The nature of the agent of AGI remains to be determined. The methods for reliable recovery of pathogens from water need to be developed. These techniques are necessary for understanding the routes of disease transmission in water. Indicators of fecal contamination have been useful to develop the barriers to the transmission of disease. Other indicators or surrogate measurements need to be developed to overcome the deficiencies of the coliform group. The die-away of natural populations of enteric microorganisms "in situ" needs attention. The inactivation of microorganisms with disinfectants under conditions found at the water treatment plant requires study. Any knowledge of the reactions responsible for microbial inactivation would certainly be useful when alternative disinfectants are considered.

The control of microbial populations in the water distribution system will have an impact on the water quality provided at the consumer's tap, the cost for operation of the pipe network, and the maintenance and longevity of the distribution system. The factors affecting proliferation of microorganisms under conditions found at the pipe surface need to be understood. A fundamental knowledge of the growth of microorganisms under conditions found in the distribution system still permit a more efficient operation of the pipe network.

A fundamental understanding of the inactivation and growth of microoragnisms at the water source, the water treatment plant, and the water distribution system would provide a firm scientific foundation to permit the development of sound treatment practices without sacrificing the accomplishments of the past 100 years.

REFERENCES

1. Center for Disease Control: Water-related disease outbreaks annual summary 1980. Issued February, 1982.

2. Cabelli, V. Waterborne viral infections. Symposium on Viruses and Disinfection of Water and Wastewater, University of Surrey, Guilford, United Kingdom, September, 1982.

3. Lycke, E., J. Blomberg, G. Berg, A. Erikson, and L. Madison. Epidemic acute diarrhea in adults associated with infantile gastroenteritis virus. Lancet 1056-1057, 1978.

4. Committee on the Challenges of Modern Society. Pilot study on drinking water supply problems, Project area III: Microbiology. (NATO) in press.

5. Hejkal, T.W., B. Keswick, R.L. LaBelle, C.P. Gerba, Y. Sanchez, G. Dreesman, B. Hafkin, and J.L. Melnick. Viruses in a community water supply associated with an outbreak of gastroenteritis and infectious hepatitis. Journal Am. Water Works Assoc. 74:318-321, 1982.

6. Prescott, S.C., and C. Winslow. Elements of Water Bacteriology, John Wiley and Sons, New York, 1904.

7. Standard Methods for the Examination of Water and Wastewater, 15th edition. American Public Health Association, New York, 1981.

8. Drinking Water and Health, Volume 1. National Academy of Sciences, Washington, 1977.

9. Wyss, O. Theoretical aspects of disinfection by chlorine. Proc. Rufolfs Research Conf., Rutgers University, 40-418, 1961.

10. Drinking Water and Health, Volume 4. National Academy of Sciences, Washington, 1982.

11. Buelow, R.W. and G. Walton. Bacteriological quality vs. residual chlorine. Journal, Am. Water Work Assoc. 63:28, 1971.

12. Snead, M.C., V.P. Olivieri, C.W. Kruse, and K. Kawata. Benefits of maintaining a chlorine residual in water supply systems. EPA-600/2-80-010, 1980.

DISCUSSION

Dr. C. Chamberlin
(The Johns Hopkins Univ.)

Comment:

I would like to add to Vince's listing of barriers between sources of contamination and the consuming population. One major barrier is omitted: a satisfactory sanitary survey. Such a survey examines the system as a whole—all the way from raw water source to the consumer's tap. Particularly in the small systems where much of our ongoing problems with outbreaks are concentrated, a satisfactory, well-conducted survey is the most reliable protection. It is certainly far better than infrequent coliform counts. Yet we have neglected this barrier for decades. It is time to redevelop and modernize the techniques and to retrain ourselves and our students.

Dr. F. M. Saunders
(Georgia Institute of Technology)

For FitzPatrick:

Joe, you talked about removing particles from water, but we haven't actually talked about getting them to disposal. What research do we need on the impacts of the processes to remove the particles from the water on the methods used to dewater the final sludges to get rid of the contaminants? Also, what do the conditioning techniques to enhance dewatering do to the trace contaminants that we are removing, in terms of the recycle water that goes back to the treatment system? Might we simply be recycling materials that have been of greatest concern to us in initial removal?

FitzPatrick's reply:

Mike, you raise some excellent points and difficult questions. There are obviously important economic benefits here. James and O'Melia (JAWWA, *74*, 3,148, 1982) showed a combination of alum and polymer most effectively removed TOC and turbidity while reducing sludge volume produced compared to alum alone.

Certainly alum sludges are more difficult to deal with than polymer or polymer plus alum sludges but fundamental dewatering and disposal research on these has not been but should be a higher priority.

We know something about how conditioning agents change macroscopic properties like solids specific gravity and maybe microscopic properties like mean floc size. Improper chemical dosing can certainly change the latter. As far as transport of trace contaminants into the interstitial waters that are expressed or drained we may have significant non-equilibrium processes, i.e., this is not like a staged equilibrium separation process.

We need to know how conditioning agents affect interparticle as well as intraparticle water. How are the toxic materials partitioned between the two types of water in relation to drainage or expression time will be important. We really haven't done enough research to answer these questions.

Dr. M. Kavanaugh
(J. M. Montgomery Consulting Engineers)

For Snoeyink:

Monitoring programs are increasingly expensive, and toxicological tests will impose another cost burden. Is there any way we can go, perhaps in a more fundamental direction, to determine which sources of water are suitable for treatment and which should best be left?

Snoeyink's reply:

I agree that monitoring and toxicity testing is expensive, but I do not think we will be able to answer the question "can water supplies with a large component of waste water be made into good quality drinking water?" without them. They should not be used blindly, however. To minimize cost, they should only be applied at projects that are carefully selected so the information that is obtained can be applied to other potential projects.

Dr. B. Rittmann
(University of Illinois)

Comment:

Snoeyink and Singer emphasized that we need fundamental knowledge on the physical/chemical state and toxicological properties of soluble contaminants. They stressed analytical and methodological advances to increase our knowledge. Another major need is effective, economical, and reliable processes to remove hazardous materials. European experience suggests that biological processes can play a role in water treatment. Slow sand filters, rapid sand filters, sedimentation filters, ground infiltration and so-called "BAC" are examples of water treatment steps that utilize microbial activity. U.S. practice would be furthered greatly if we had a better fundamental understanding of the possibilities of biological processes in water treatment. Since U.S. water works try to prevent biological activity, a major research effort is necessary to demonstrate the potential and advantages of biological processes. A strong fundamental understanding is needed to convince American educators and practitioners that biological processes can have a place in water treatment.

FUNDAMENTAL NEEDS IN WASTEWATER MANAGEMENT

Conference on Fundamental Research Needs for Water and Wastewater Systems

FUNDAMENTAL RESEARCH NEEDS FOR WASTEWATER TREATMENT: BIOLOGICAL PROCESSES

C. P. Leslie Grady, Jr.
Environmental Systems Engineering
Clemson University
Clemson, SC

INTRODUCTION

The long-term objectives of fundamental research can be grouped into two categories: (1) the development of a better understanding of existing technology thereby leading to improvements in its application and (2) the creation of new technology which will either solve existing problems more effectively or will provide the means for solving new problems which are as yet unforeseen. Five years ago at a similar conference, Perry McCarty identified microbial ecology as a fundamental research area which had the potential for ultimately improving our ability to control activated sludge bulking, a problem associated with existing technology. Now, as a result of classical fundamental studies by David Jenkins and his colleagues at the University of California at Berkeley, we have a much better understanding of the microbial interactions associated with bulking and are on our way to controlling it. This is but one example of how fundamental research in biological wastewater treatment has helped engineers improve existing technology. As far as the development of new technology is concerned, biological fluidized bed reactors provide a good example of the application of fundamental research to meet new needs. Without an understanding of the kinetics of denitrification and the hydrodynamics of fluidization, the inventors of that process would not have had the insight required to develop it. This illustrates an important point: the payoff from fundamental research is often unforeseen. This fact emphasizes the importance of investigator-generated (i.e., unsolicited) research. While a degree of direction within research programs is necessary for timely progress, the breadth of vision generated by diverse viewpoints is imperative for the success of a fundamental research program.

Because of my belief in the importance of broad-based generation of research ideas, I felt more than a little uncomfortable in the preparation of this document. One way to ease that discomfort was to read what others had to say on the topic. The bibliography at the end of this paper contains documents which influenced my thinking and which are worthy of note by readers seeking broader viewpoints.

RESEARCH TO IMPROVE EXISTING TECHNOLOGY

Research on existing technology can be approached from two points of view: technology-specific and general. For example, from the technology-specific point of view, if we wanted to know how the activated sludge process would respond to the input of a toxic anthropogenic compound we would set up a prototype system and subject it to such an input, observing the response for a number of different situations. In contrast, from the more general point of view we would first seek to understand how a growing microbial culture would respond to the compound. We would then combine that knowledge with reactor engineering principles to predict how the process might respond under a variety of conditions, thereby identifying the most critical ones for subsequent testing in a prototype unit. In my opinion the latter approach is more consistent with fundamental research because the long-term goal of such research is the establishment of intrinsic information which can be used in system-specific models (both mathematical and conceptual) which account for the physical peculiarities of the particular system of interst. This suggests that the intrinsic information must encompass all aspects of the system: physical, chemical, and biological. Engineering has traditionally focused on the physical and chemical aspects of engineered systems. Nevertheless, many fundamental questions remain, as discussed in other papers. The focus here, however, will be on the biological aspects, even though it must be recognized that the other aspects are equally important.

Many of the fundamental research needs for biological wastewater treatment came into sharper focus when our perspective shifted from general performance parameters like the removal of BOD_5, COD and TOC to the fate of specific organic compounds. Thus one might say that a benefit of the "toxics age" was to get us to direct our thinking to a different level; to be, as it were, more fundamental.

Starting inside the cell we have to ask questions that microbial biochemists have bypassed. For example, what concentration of a specific organic compound is required in the bulk liquid to induce the enzymes responsible for its degradation? Is there a lower limit below which induction can't occur? How are those concentrations influenced by the presence of other organic compounds in the liquid? Will they raise or lower the minimum concentration? These are important questions for both treatment processes and the natural environment because they relate to persistence. How many persistent compounds are considered to be so because they are present at levels too low to allow development of the requisite enzyme system for their degradation? Another aspect of the same question deals with the maintenance of the enzyme systems in the absence of the inducer. How rapidly are they lost? Is the rate of loss influenced by the presence of other organic compounds or by the growth rate of the culture? What could be done to reduce that rate? These questions are important to the proper operation of many industrial wastewater treatment facilities where intermittent discharges of exotic organic compounds are the rule, rather than the exception.

Going up one level in scale, we might focus on the intrinsic kinetics of microbial cultures rather than on the synthesis of individual enzymes within them. How common are high affinity enzyme systems that allow relatively rapid removal of specific organic compounds at low concentrations? Are they limited to oligotrophic organisms? Are they likely to develop in wastewater treatment systems? Will the configuration of the system determine whether they develop? If they will develop, can their existence be stabilized? In spite of all of our work on the kinetics of removal of general "organic matter" by microbial cultures, we know little about the rates at which individual organic compounds are removed from mixtures. Can we extrapolate kinetic studies done with a single substrate to multisubstrate systems? At what level of media complexity do the effects of further increases in complexity become insignificant? Answers to questions such as these will not only help us understand how wastewater treatment systems function but will help us do a better job of planning the studies from which design data are obtained.

When we focus on anthropogenic compounds in wastewaters the question of toxic organics always arises. In spite of that, we know relatively little about the kinetics of biodegradation of inhibitory substrates. For example, are the kinetics of removal of inhibitory organic compounds influenced by their contribution to the total organic input? Some research indicates that an important factor influencing the degree of inhibition is the ratio of inhibitor to total biomass, which suggests that the percentage of organic matter contributed by the inhibitory material would be important. Nevertheless, few models depicting the kinetics of biodegradation of inhibitory material include that factor. Should they? How important is it? Most models for biodegradation of inhibitory substrates have acted as if all of the organic material was inhibitory even though in most cases it is not. What happens to the rates of removal of noninhibitory organics when inhibitory organics are present and undergoing biodegradation? Many questions could be posed about inhibitory materials but few have been answered in spite of the importance of such materials in many industrial wastewaters.

All of the preceding needs have been expressed within the context of steady-state kinetics. Because few wastewater treatment systems are ever at steady state we also need to pose similar questions for the dynamic state. This will be more difficult, however, because dynamic modeling is more difficult, our understanding of dynamic-state growth is less advanced, and relatively little work has been done on the dynamics of removal of individual organic compounds in complex mixtures. Such work should necessarily follow the steady-state research. However, it, too, must be done if we are to develop the ability to effectively evaluate the reliability of biological processes.

Before leaving kinetics, it is important to mention the area of biodegradation testing. Even though a number of procedures have been developed for determining the biodegradability of individual organic compounds or of complete wastewaters, there is little consensus on how the problem should be approached. Since the results have often depended in large part upon the techniques employed, it would appear worthwhile to establish a standardized protocol which could be broadly applied.

Moving to another level of complexity in our view of biological reactors, it is apparent that in spite of the real progress that has been made in our understanding of microbial ecology we still have a long way to go. Of course, research on the ecology of biological wastewater treatment systems should be focused on how to maintain a desirable and stable microbial community within them. This will require, however, fundamental work on microbial interactions in slurries and in films. Much of the work that has appeared recently has been focused on competition, but there are many other interactions which must be understood. What types of interactions are important within microbial consortia required for complete degradation of exotic organic compounds? How are those interactions altered by the presence of other ordinary organics? Does an ecosystem ever reach a true steady-state when grown under constant conditions or will it always be dynamic? Will the most stable ecosystems develop in completely-mixed reactors receiving constant inputs or will they arise in macroscopically dynamic systems like sequencing batch reactors? What is the ecology of a biological fixed-film? Is it different when receiving a multicomponent feed than when receiving a single component one? Where do the organisms responsible for the degradation of individual constituents reside within a film? Do they vary spatially in all dimensions? How is that spatial heterogeneity influenced by the media type? What factors influence the proliferation of nuisance organisms? The questions could go on and on, reflecting the relative neglect of this area by wastewater treatment researchers, in spite of the notable exception mentioned in the introduction. If true progress is to be made in our ability to control biological processes we can no longer afford to treat them as black boxes or to wait for traditional microbial ecologists to study them for us.

Having mentioned the ecology of biofilms, it is important to recognize that many research needs are specifically associated with them that might not be identified if we concentrated entirely on biological processes that employ bacterial slurries. For example, what are the biological and physical factors associated with biofilm formation and retention? What types of surfaces encourage or discourage attachment? How are the mechanisms of biofilm formation in wastewater treatment systems related to the mechanisms observed in dental and heat exchange studies? How does attachment affect microbial physiology? Does it alter the cell surface sufficiently to change the intrinsic kinetics? How important is adsorption of substrate at the solid support surface to the development of a biofilm? Once a film is formed, does adsorption continue to play a role in the provision of substrate to the film? Finally, what is the relationship of attrition to physical and ecological factors? Do physical factors have to be negligible before ecological factors become important?

Continuing with the theme of microbial interactions at surfaces, it is important to know how related the mechanisms of biofilm formation are to the mechanisms of bioflocculation. Are the forces causing attachment within films the same ones that cause aggregates to be formed in slurry reactors? What are the materials which actually cause bioflocculation to occur? There is an amazing diversity of opinion on that subject. Is the diversity due to real differences within the flocculent slurries or to the experimental techniques employed? How should we go about studying bioflocculation? Perhaps the most troublesome problem plaguing activated sludge systems is sludge settleability. In spite of that the effort expended on studying it has been relatively small. Nevertheless, given the progress made during the past 10 years in a number of difficult areas, it would appear that this problem should yield to sound fundamental study.

Anaerobic processes play an important role in wastewater treatment systems and much progress has been made in our understanding of them because of fundamental studies conducted by microbiologists and biochemists studying the rumen. Furthermore, that information is being applied and extended by wastewater treatment researchers, thereby improving the reliability and efficiency of those processes. One area where environmental engineering researchers have taken the lead, however, is anaerobic biodegradation of toxic organic compounds. Their studies have shown that anaerobic processes are much more robust and versatile in their biochemical mechanisms than originally thought. Furthermore, they appear to be able to perform some biodegradations that are difficult to perform aerobically. This suggests that we are just beginning to understand and exploit these complex ecosystems and that a continuation of this fundamental research will lead to much greater application of this energy efficient process.

Although the research needs which have been listed constitute only a small part of the total which could be identified, they are sufficient to give some insight into the many problems that will yield to fundamental research. There is, however, one important impediment to maximum application to that research: lack of a means for critically reviewing it and synthesizing it into a coherent body of knowledge. Many other fields have excellent mechanisms for publishing in-depth, critical reviews which summarize research findings and put them in perspective. Notable among these are the *Annual Reviews In* . . . published by Annual Reviews, Inc. and *Microbiological Reviews* published by ASM. Environmental engineering badly needs such a vehicle for transmitting research results. I hope that one outcome from this conference will be the development of a suitable review mechanism.

RESEARCH TO DEVELOP NEW TECHNOLOGY

In addition to research which can be identified with existing technology it is important that we also pursue fundamental research which will eventually lead to new processes which have not yet been conceived. Currently, there are two areas which should receive intensive investigation: exploitation of unconventional microorganisms and natural genetic engineering.

Kobayashi and Rittmann have done an excellent job of drawing together literature on the potential exploitation of unconventional organisms. They stated the concept as follows: "... Microorganisms not normally associated with biological waste treatment have potential advantages when the removal of anthropogenic compounds is the goal. A broadened perspective into what constitutes biological treatment opens promising new areas of research and application." They then go on to list several unique organisms which might be exploited. One group consists of oligotrophic bacteria, which were mentioned earlier. Although these organisms are little known and relatively unstudied they are potentially useful for removing trace concentrations of organic contaminants because they can survive under low nutrient conditions. It is unclear at this time just what causes adaptation to oligotrophy and thus considerable basic work will be required before these organisms can be exploited.

Prototrophs (i.e., algae, cyanobacteria, and photosynthetic bacteria) constitute another group of potential importance. Because they receive their growth energy from light, it is possible to grow relatively large masses of them even in low nutrient environments. They then have two potential uses. First they transform chemicals which might be present in low concentration, thereby making them more amenable to attack by other organisms within the system. Second, they can bioaccumulate hydrophobic compounds, thereby acting as a biological sorbent.

It would also appear that processes could be designed to exploit the wide metabolic diversity of fungi. For example, they have a greater ability to degrade or transform hydrocarbons of complex structure or long chain length than do most bacteria. As with many of the other nonconventional organisms, however, the result of their activity is the excretion of metabolic products and thus it appears that most will have to be used in association with other microbes in mixed communities. Consequently, many of the fundamental research needs associated with this area are really much the same as the ones enumerated earlier. It is important to recognize, however, that we should broaden our horizons and look at biological processes in a completely new way.

The other area of research related to the development of new technology is natural genetic engineering. It is now well established that bacteria contain accessory DNA elements which include viruses, plasmids, transposons, and insertion sequences. Furthermore, it has also been established that some of these elements can move from organism to organism and from species to species thereby distributing new genetic information quite broadly. This has revolutionized the thinking about the evolution of degradative pathways and the meaning of the term acclimation. It had long been thought that the complement of DNA in a particular species of bacteria was fixed and that acclimation consisted primarily of induction and synthesis of the requisite enzymes. The evolution of new degradative pathways was thought to involve mutations in the DNA coding for enzymes capable of catalyzing reactions similar to the ones needed to attack a given compound. The discovery of accessory DNA elements and the recognition of their mobility led to the realization, however, that nucleic material can be spread from organism to organism. This required a redefinition of acclimation to include the possibility that pathways may evolve because of that spreading. For example, through a point mutation one strain of bacteria may develop an enzyme allowing it to catalyze a reaction involving an anthropogenic compound present in its environment, thereby receiving some benefit, such as energy. Because the reaction product is also new to the organism, however, it is not likely that it would be degradable. Instead, it would be excreted into the medium where it would serve as a potential substrate also. Consequently, another strain might develop an enzyme catalyzing a reaction with it, releasing a second product, etc. As a result of a sequence of such events, a consortium of bacteria would develop which, in toto, was able to break down the compound. Now suppose, however, that the first strain in the consortium contained an accessory DNA element which allowed the DNA coding for the first new enzyme to be transferred to the second strain. The second strain would then be able to perform the first two steps in the degradation, thereby giving it a competitive advantage. Continuation of this process would eventually lead to a totally new strain containing the entire pathway for degradation of the original substrate. In other words, the pathway evolved by gene transfer.

Development of a new pathway by direct manipulation of the DNA (i.e., genetic engineering) would require a priori knowledge of the desired pathway as well as awareness of the exact genes to be manipulated. Natural genetic engineering, on the other hand, is based upon the premise that the outcome of natural evolutionary processes can be accelerated by maximizing the opportunity for DNA exchange in a mixed culture placed under selective stress. The technique is very similar to the acclimation procedure routinely practiced by wastewater treatment engineers—with one notable exception. The culture is continually seeded with a variety of organisms known to carry accessory DNA elements, thereby maximizing the potential for DNA exchange. Although the technique was used to develop organisms capable of degrading 2,4,5-T, the key question is whether it is efficacious. Does it really offer advantages over normal acclimation and enrichment techniques? If it does it has great potential for the development of specialized cultures for both industrial wastewater treatment and cleanup of toxic spills. Another important question has to do with the stability of pathways once they have evolved. Although studies with drug resistance suggest that they are quite stable, little work has yet been done with degradative pathways. Finally, a crucial issue which must be resolved before either natural genetic engineering or exploitation of non-traditional organisms can be applied concerns retention of organisms in the treatment system. How can that best be accomplished? Only fundamental research will provide the answer.

SUMMARY

A number of questions associated with fundamental research in biological wastewater treatment have been posed. The list is by no means exhaustive, but rather was presented to stimulate thought. It is evident, however, that while we have learned much from fundamental research we still have much to learn. The best source of ideas, however, will continue to be the individual investigator who is delving deeply into a given topic. That technique has served us well in the past and will continue to do so in the future.

BIBLIOGRAPHY

Bourquin, A. W. and Pritchard, P. H. (1979) *Proceedings of the Workshop: Microbial Degradation of Pollutants in Marine Environments*, USEPA Report #EPA-600/9-79-012.

Campbell, A. (1981) "Evolutionary Significance of Accessory DNA Elements in Bacteria," *Ann. Rev. Microbiol.*, *35*, 55-83.

Kobayashi, H. and Rittmann, B. E. (1982) "Microbial Removal of Hazardous Organic Compounds," *Environ. Sci. Tech.*, *16*, 170A-183A.

Maki, A. W., Dickson, K. L., and Cairns, J., Jr. (1980) *Biotransformation and Fate of Chemicals in the Aquatic Environment*, Proceedings of a Workshop, Pellston, MI, August 1979, ASM, Washington, DC.

McCarty, P. L. (1978) "Fundamental Research Needs in Wastewater Treatment for Biological Processes," in *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, J. H. Sherrard, Editor, pp. 72-76.

Reanney, D. C., Roberts, W. P. and Kelly, W. J. (1982) "Genetic Interactions among Microbial Communities," in *Microbial Interactions and Communities*, Vol. 1 edited by A. T. Bull and J. H. Slater, Academic Press, New York, pp. 287-322.

Saunders, F. M. et al. (1982) *Research Needs Associated with Toxic Substances in Wastewater Treatment Systems*, Water Pollution Control Federation, Washington, DC.

FUNDAMENTAL RESEARCH NEEDS FOR SLUDGE TREATMENT, UTILIZATION, AND DISPOSAL

by
James M. Gossett
Department of Environmental Engineering
Cornell University
Ithaca, New York

Richard I. Dick
Department of Environmental of Engineering
Cornell University
Ithaca, New York

Thomas D. Hinesly
Department of Agronomy
University of Illinois
Urbana, Illinois

INTRODUCTION

The historical lack of fundamental research concerning sludge treatment, utilization, and disposal has been clearly manifested in recent decades as increasing wastewater sludge quantities, escalating costs, and intensified environmental awareness have made sludge management a major problem in water pollution control. Indeed, the U.S. Environmental Protection Agency and the Water Pollution Control Federation recently identified sludge management as one of two areas (along with control of toxic substances) deserving high research and development priority.

As a result, the Research Committee of the Water Pollution Control Federation recently developed "An Analysis of Research Needs Concerning the Treatment, Utilization and Disposal of Wastewater Treatment Plant Sludges" (WPCF, 1982). All three authors served on the subcommittee that prepared the report, and extensive use of the earlier report in developing this paper is acknowledged. Some differences must be emphasized, however. This paper addresses only fundamental research needs, while the WPCF report is additionally directed towards applied research. Also, the WPCF report contains a brief, documented assessment of the current state-of-the-art with respect to sludge management, and readers seeking justification or amplification of fundamental research needs noted in this paper are referred to the WPCF document.

FUNDAMENTAL PROPERTIES OF SLUDGES

Considering the importance of the sludge management problem, it is somewhat surprising—and certainly humbling—to realize just how little is known of the fundamental physical, chemical, and biological properties of sludges. As an illustration, consider that even if we measure every conceivable fundamental property of a sludge, we are unable to predict the performance of the sludge in conditioning or dewatering processes. Instead, we must rely upon bench or pilot-scale experiments which essentially duplicate conditions to be imposed at plant scale. Perhaps even more exasperating is that the fundamental physical/chemical properties of a composite sludge produced by blending two separate sludges cannot be predicted from fundamental, measured properties of each sludge alone; nor can thickening, conditioning, or dewatering performance of the composite be predicted from pilot-scale tests conducted with the two separate sludges.

If the physical, chemical, and biological properties of sludges were better understood—particularly their relation to sludge treatment process performance—then control of these properties might be better exercised in sludge generating (wastewater treatment) processes. For example, numerous studies using 100 percent biological sludges have demonstrated that biomass "condition" markedly influences clarification, thickening, and dewatering properties of the sludge. Presumably, biomass "condition" can be influenced—and thereby controlled—by advantageous manipulation of biological process operating conditions. However, the parameters defining "condition" of biomass have not been adequately defined, nor is it known what effect biomass condition has on properties of sludges with a large nonbiological component. And despite efforts of the preceding decades, it is not yet possible to accurately measure the biomass fraction of a sludge.

Physical/chemical properties of sludges limit the performance of many sludge treatment processes, but the properties are poorly understood. We have not adequately defined the factors which control formation of flocculent sludge particles, their response to shear forces, and the mechanisms by which they hold water. Such an understanding will require investigation of the surface chemistry of sludge particles, and the way in which surface properties are influenced by wastewater treatment and sludge treatment processes.

Other necessary fundamental research on chemical properties of sludges is related to potentially toxic constituents, such as heavy metals and synthetic organic compounds. A thorough understanding is needed of the mechanisms by which these compounds become incorporated into sludges. This would aid in anticipating the fate of toxic constituents in sludge treatment and ultimate disposal facilites, and in achieving economical extraction of the constituents.

The biodegradability of a sludge is an important fundamental parameter which affects the performance of biological sludge stabilization processes and which defines stability of sludge in ultimate disposal. There are sufficient numbers of experimental assays currently available to estimate aerobic or anaerobic biodegradability (WPCF, 1982). What is needed is a more fundamental understanding of the relation between structure (both molecular and macromolecular) and biodegradability, and the role of environmental conditions in the biodegradation of organic materials. A number of techniques have been suggested for use in quantifying sludge stability in ultimate disposal, such as oxygen uptake rate, gas production rate, and ATP/VSS ratio. However, the types of organisms contained in the sludge apparently influences "stability" as defined by such measures. Also, stability defined under one set of environmental conditions does not necessarily coincide with absolute stability. For example, aerobically-stabilized sludges are not stable when subjected to anaerobic environments (and vice versa).

PROCESSES FOR REMOVING WATER FROM SLUDGES

Much of the cost of sludge management is associated with reduction of sludge volume by removal of water. Conventional processes for moisture removal include thickening, conditioning, dewatering, and drying. Recently there has been renewed interest in dehydration of sludges. The key to a more fundamental understanding of all these processes is a better understanding of basic sludge properties as outlined in the previous section of this paper.

Thickening

An understanding of thickening mechanisms at the particle level is needed. Changes in particle size and water content and the interactions between particles as thickening ensues should be investigated. Phenomena associated with bubble attachment in flotation thickening should be explored. Development of a basic understanding of thickening mechanisms would allow variables such as depth, time, and stirring to be taken into account in the design process.

Our lack of understanding of thickening phenomena is exemplified by the widespread use of "air-to-solids" ratio (A/S) as a parameter of importance in design and operation of flotation thickeners. There are infinite combinations of recycle rate and pressure all yielding the same A/S—but different thickener performances. Air-to-solids ratio is an irrational parameter in the sense that its effect depends upon how its particular value is achieved.

Mechanical thickening is currently carried out by centrifugation. As additional information is gained on the basic physical/chemical properties of sludges, alternative mechanical thickening equipment may be developed.

Conditioning

As used here, conditioning refers to any technique for altering the properties of sludges to facilitate the release of water. Perhaps no other process in sludge management has as much ignorance associated with it as has sludge conditioning. Present practice resembles alchemy as much as science. It is to be hoped that the exploration of basic physical/chemical properties of sludges will be accompanied by insights which will transform conditioning from an art form to an effective and economical means for altering sludge properties.

The mechanisms by which inorganic and organic chemical conditioners achieve a change in the physical properties of sludges have not been adequately elaborated. For example, conditioning with iron or aluminum salts is often explained via theories originally postulated to describe coagulation of hydrophobic colloids. This explanation does not account for removal of "bound water" and other presumably important conditioning mechanisms. Ultimately, fundamental investigation of conditioning chemistry should yield methodologies for selecting conditioners and establishing dosages on a more rational and efficient basis than is currently possible with our present "trial and error" procedures. Similarly, mechanisms involved in physical conditioning using heat and cold must be explored. With improved understanding of conditioning mechanisms, it should be possible to tailor techniques to specific sludges and specific types of thickening and dewatering processes.

Dewatering

In order to significantly advance the technology of dewatering, it is necessary: to investigate the basic physical/chemical properties of sludges; to elaborate the fundamental mechanisms by which the various mechanical dewatering processes work; and to define (and interrelate) those sludge properties and operating parameters which are specifically important to a given dewatering process. We are far from these goals. For example, the response of sludges to fluid drag forces and compressive forces such as occur in mechanical dewatering equipment—and the resulting change in the ability of water to move through the mass of sludge solids—is virtually unknown.

There are a variety of levels at which one can seek meaningful techniques for analysis, selection, and design of sludge dewatering processes. At present, the only reliable method is to test at pilot-plant level the specific sludge in question with the specific dewatering process under consideration. Ultimately, we would like to be able to accurately predict performance of any given dewatering process as a function of process parameters and fundamental properties of the specific sludge in question.

At an intermediate level of development, we would be content to have techniques for measuring non-fundamental, but meaningful sludge dewatering properties using small, laboratory-scale procedures. The two parameters most frequently used to characterize the dewaterability of sludges—specific resistance and capillary suction time—do not offer the promise of serving as the basis for rational design of a variety of different dewatering processes. Specific resistance appears the more fundamental of the two, being derived from considerations of flow through porous media. However, it is of little practical value, even in the design and analysis of vacuum filters. And it is of no use in design or analysis of belt filter presses and centrifuges, where dewatering mechanisms differ from those in the specific resistance procedure. Much additional work is needed to develop more suitable measures for the different dewatering processes.

Drying and Dehydration

Heat drying for the purpose of producing a dried, finished product is not considered to warrant research. However, drying in association with sludge combustion processes may warrant attention.

Currently, there is interest in decreasing the energy required to reduce the moisture content of sludges to low levels by use of dehydration processes. Proprietary processes involving solvent extraction (followed by centrifugation) and oil emersion (coupled with multiple effect evaporation) are being evaluated. Results of large-scale work underway at Los Angeles may provide an indication of fundamental research needs associated with dehydration processes.

STABILIZATION

Stabilization processes are those whose aim is the production of a sludge end product that is biologically inert, minimizing odor and other nuisance problems in ultimate disposal. Research needs relating to the various individual stabilization methods are discussed below. Common to all is the need for further fundamental research concerning the fate of persistent—often hazardous—compounds in the various stabilization processes. Such research should address (where applicable) biological transformation, volatilization, and interphase partitioning. Also common to all stabilization processes is a great confusion concerning the effect of stabilization process design and operating parameters on performance of subsequent solids handling operations. Additional research is also warranted to define the manner in which stabilization process parameters influence pathogen destruction. Increased attention should be devoted to some novel schemes being proposed, such as the use of earthworms in the stabilization of raw sludges and the immobilization of sludge constituents via solidification.

Anaerobic Digestion

Anaerobic biological stabilization is presently used at approximately 70 percent of the wastewater treatment facilities in the United States. The anaerobic digestion process has, however, developed the reputation of being an unreliable process, one susceptible to upset. While failure may result from introduction of inhibitory materials, surveys conducted in Great Britain indicate that operation/design errors are more likely culprits. The whole question of reliability—and operational strategies to increase reliability—needs further investigation. Great gaps in our fundamental knowledge of the ecological relationships among the anaerobic microflora—particularly with respect to thermophilic digestion—hinder advancement of dynamic modelling of the process. Continued basic research pertaining to microbial ecology of anaerobic sludge digestion is warranted. This should include refinement of enumeration techniques, fundamental studies of the kinetics of individual populations comprising the active microflora, and identification of important growth factors.

One factor which has prevented more widespread use of anaerobic processes is the belief that many types of compounds (for example, certain aromatics and chlorinated hydrocarbons) are toxic, or at least not degradable in anaerobic systems. However, recent studies have demonstrated acclimation to a wide variety of aromatics and petrochemicals. More widespread application of anaerobic processes to industrial wastes would perhaps result from fundamental research in this subject area.

The physical appearance of anaerobic sludge digesters and appurtenances has changed little in the last 40 years. Novel process schemes and configurations need further examination and development. Particularly promising are the many varieties of the anaerobic filter process which are currently being proposed.

Aerobic Digestion

Aerobic sludge stabilization is an alternative to the anaerobic digestion process; as conventionally practiced, it is essentially similar to the activated sludge process. Little work has been done concerning the detailed ecological relationships and successions among aerobic digester organisms. Though the process does not appear to suffer upset as easily as does anaerobic digestion, ecological studies are nonetheless called for because species dominance appears to relate to process performance and the "stability" of the resulting sludge.

Due to its high energy requirement, aerobic digestion will not likely compete successfully with anaerobic digestion at larger treatment facilities unless significant advances in process development are made. Process configurations achieving "autoheated" thermophilic conditions seem particularly attractive.

Composting

Though much valuable information has been gained from recent composting installations, the engineering fundamentals of composting are in a state of comparative infancy relative to the level of understanding of other processes. Basic research is needed to develop models interrelating physics, thermodynamics, and kinetics for compost systems, allowing quantification of mass, energy, moisture, and volume balances. For example, the aeration rate, moisture content, and type of bedding material used influences in a rather complex manner the internal temperature profile and biological rate of decomposition over time. To develop such a general model, background studies are needed concerning kinetics and ecological relationships among composting organisms (including delineation of their temperature optima, for which wide variations in published data exist), extent of breakdown and degradation kinetics of commonly-used amendments, etc.

Chemical Stabilization

Chemical stabilization processes do not achieve reduction of sludge mass or volume, relying instead on the creation of an environment (usually high pH resulting from lime addition) in which biodegradation cannot occur. Using this strategy, stability of sorts can be achieved (at least temporarily), even though the intrinsic biodegradability of the solids is left unaffected. Chemical stabilization is, therefore, usually an interim, emergency, or seasonal stabilization alternative. Research is needed concerning effective means for simultaneously conditioning and stabilizing sludge and for stabilizing previously conditioned and dewatered sludge. Physical, chemical, and microbiological changes in lime-stabilized cakes with storage periods should be investigated.

INACTIVATION OF ORGANISMS AND VIRUSES

In recent years, increased attention has been given to processes such as pasteurization, high energy electron irradiation and gamma irradiation for inactivating organisms and viruses in sludges from municipal wastewater treatment. The greatest research need in this area is considered to be epidemiological studies to indicate situations in which expenditures for inactivation of organisms are justified. Of lower priority is the further exploration of necessary treatment conditions to achieve a desired degree of inactivation and acquiring additional information on the effect of inactivation processes on other properties of sludges so as to permit their effective design and integration into sludge management schemes.

THERMAL PROCESSES

Combustion of sludges has been practiced for many decades. Recent developments with regard to energy prices and environmental standards have created a need for additional research, however. Much of the thermal process research that should be carried out is of an applied nature, involving, for example, means for improved heat recovery. However, fundamental research is also needed to improve abilities to effectively and economically incinerate sludges.

A major priority for fundamental research to improve the economics of sludge incineration has been described in earlier sections. That is, improved understanding of basic physical properties of sludges, of conditioning mechanisms, and of the performance of dewatering processes could lead to improved ability to reduce the moisture content of sludges fed to incinerators. This is the single most important requirement for improving fuel economy.

Variations of conventional combustion, such as starved air and wet oxidation, offer potential advantages. Much of the research needed to test their applicability is applied. However, basic research on the transformations that occur in wet oxidation and starved air combustion and the factors that influence those transformations would be useful in guiding the applied studies.

Improved understanding is needed on the influence of sludge characteristics and thermal conditions on the loss of volatile forms of metals. Factors affecting the destruction and possible formation of toxic organic compounds during combustion must also be further explored.

RECLAMATION

Ultimately, it is necessary to develop techniques for recycling sludge constituents into productive uses. Currently, reclamation is being practiced to a significant extent only with sludge of quality suitable for use on agricultural land.

Land application of clean sludges is feasible only because heterogeneous sludges can be applied without the need for separation of sludge constituents. Fundamental research to support other potential applications using heterogeneous sludges needs to be completed. Such potential applications include use of ultraclean biological sludges from particular industries (for example, food processing) as an animal feed supplement, and development of building materials which effectively immobilize constituents of heterogeneous sludges that are less clean.

As more is learned about the basic physical, chemical, and biological properties of sludges, fundamental research should be carried out to develop means for recycling constituents of heterogeneous sludges. Protein, metals, and trace organic compounds such as vitamins are examples of sludge constituents of significant value if techniques can be developed to separate them from other sludge constituents. In many industries, problems of separating sludge constituents may not be severe. Indeed, it may be possible to produce nearly homogenous sludges that may be recycled with ease.

ULTIMATE DISPOSAL

Land Spreading and Soil Incorporation

Land disposal of sludges has long been carried out effectively at small wastewater treatment facilities, surrounded by extensive areas of open lands. If industries that could be potential dischargers of toxic waste to sewer systems exist in small communities, they are easily identified and controlled. Thus, the need for further research concerning the disposal of wastewater sludges by land spreading is not great for small communities. But a great deal of further information is needed to develop environmentally safe plans for land spreading of sludges from large, industrialized, metropolitan areas where sources of toxic substances are difficult to identify and control.

Research should be directed toward supplying information needed to guide the selection of permanent sites and operations that will permit the disposal of maximum amounts of sludge into perpetuity, while safeguarding public health.

Nitrogen. — The need to protect groundwater supplies against excessive accumulations of nitrate-nitrogen limits annual application rates of a metalliferous digested sewage sludge on soils where field crops are grown. Nearly all available information regarding the fate of sludge-borne nitrogen was derived from short-time laboratory incubation, soil column, and greenhouse studies, which may have some pertinence to one-time, low-rate applications of sludge. However, very little is known about nitrogen transformations and losses from long-term field projects where soil physical and chemical properties have been drastically changed by sludge applications. Further field studies should be initiated to evaluate the various parameters affecting the fate of sludge-borne nitrogen on areas that have received sludge applications for a sufficient length of time to establish an equilibrium between soil organic matter additions and losses.

Trace Elements. — Based on their toxicity, frequency of occurrence, persistence, and rate of increasing use, the U.S. Environmental Protection Agency (Jenkins, 1981) has identified antimony, arsenic, beryllium, boron, cadmium, chromium, cobalt, copper, lead, mercury, nickel, selenium, tin, and vanadium as posing continuously increasing threats to plant, animal, and human health. By order of importance, lead, cadmium, mercury, arsenic, and nickel were thought to offer the greatest potential threats to human health. Sewage sludges, and especially those from large, industrialized cities, contain these elements at concentrations several-fold higher than those generally found in soils. Therefore, their concentrations in soils are expected to increase as applications of sludge continue on the same site. Recent results from long-term studies where digested sludge from Chicago was applied to soil showed that after several years of annual application, losses of trace elements approached the amounts that were applied each year. The disappearance of trace elements from the complete soil profile could not be accounted for by amounts measured in runoff and drainage water or as constituents of grain removed from field plots. Research is needed to determine the mechanism(s) which establish the fate of heavy metals in sludge-amended solids so that the impacts of land spreading can be properly assessed and recommendations made for reducing those practices that affect the environment adversely. We need to know: 1) if there is some critical soil concentration at which heavy metals are lost from soil; 2) if losses vary with soil type, methods and rates of sludge application; 3) the importance of organic matter additions to metal losses; 4) the effect of gradual increases in heavy metal concentrations on amounts lost from soils; 5) the duration of heavy metal losses from soils after sludge applications are terminated; and 6) the effect that different cropping systems have on heavy metal losses from soils.

Prevention of increased levels of cadmium in human food-chains could be attained by growing varieties and cultivars of field crops with an inherited capacity to exclude cadmium uptake beyond that found in similar plants growing on uncontaminated soils. However, the development of adapted varieties of self-pollinated field crops to exclude cadmium from accumulating in plant tissues will be more difficult. It will first involve the screening of world-wide sources of germ plasm to identify gene pools that could be incorporated into adapted varieties of crop plants to regulate cadmium uptake to normal levels without loss of quality and yield potentials. This breeding work could be considerably reduced if genetic mechanisms controlling the uptake of heavy metals by plants were known.

Disease Organisms. — Pathogenic organisms are concentrated in sewage sludges and survive most sludge treatment processes, persist during long periods of sludge storage, and remain viable for various periods of time on the surface of vegetation and in soils. Nevertheless, there is a paucity of reports linking sludge application to disease outbreaks in either man or animals. It appears that health risks from disease-causing organisms present in stabilized sludges spread on land are low, but further studies are especially needed to evaluate risks to the health of animals grazing pasturelands that have been treated with repeated applications of sewage sludge.

Refractory Organics. — Presuming that sewage sludges will not be used to grow vegetable crops, refractory organics present a threat to human health mainly through food-chains involving animals. Thus, further research should be devoted to assessing health risks to animals grazing pasturelands which have received long-term annual applications of digested sewage sludges.

Land Reclamation. — Because of its high organic matter content, sewage sludge could play a major role in the reclamation of 0.8 million hectares of lands that have been severely disturbed by surface mining. A major applied research effort is needed to develop methods for utilizing municipal sludges as an organic amendment to reclaim these lands to a high state of agricultural productivity without creating or intensifying problems of erosion and water contamination during the reclamation process.

Landfilling. — While beneficial use of sludges is a worthy goal, it is to be anticipated that landfilling—particularly, of sludges with adverse properties—will continue to be a means of sludge disposal for the foreseeable future. Chemical, physical, and biological transformations that can occur under conditions that exist in landfills need to be further clarified. Interactions of constituents of leachate from landfills with soils and lining materials need to be explored.

Ocean Disposal

During much of the past decade, a federal policy banning ocean disposal of sludges existed. Consequently, the amount of fundamental research on effects of ocean discharge of sludges was limited. Fundamental research is needed to guide sludge management policy and to allow ocean disposal to be conducted in the fashion which minimizes adverse environmental effects. Such research is complicated by the fact that sludge is but one of many discharges to the ocean.

Fate of sludge particles in oceans needs to be better understood. Mechanisms such as resuspension, solubilization, and biological uptake which may take place when sludge particles become a part of ocean sediment should be explored. The significance of typical sludge constituents with regard to chronic toxicity in marine systems must be explored. There are indications that mechanisms of pollutant assimilation in the deep ocean are unusual, and these must be explored if sludges are to be discharged to the deep ocean.

PUBLIC RELATIONS

Public acceptance is necessary to implement a successful program of sludge management. Indeed, it is likely that more schemes have failed because of lack of public acceptance than from lack of technical feasibility. While fundamental research concerning public acceptability is quite different than other research needs suggested here, it seems warranted. It is necessary to learn how information on alternative sludge management schemes can best be presented to the public.

As in other areas of environmental quality control, research on risk assessment associated with sludge management is justified. The existence of risk assessment procedures that would allow rational decisions to be made from among alternatives might have eliminated much of the sludge related controversy of the previous decade.

PROCESS INTEGRATION

Traditionally, each wastewater treatment and sludge management process has been designed essentially independent from other processes with which it interacts. Ultimately, trade-offs between processes must be considered in order that overall optimum design and operation of wastewater and sludge management processes can be achieved. The ability to achieve overall optimum design and operation awaits development of fundamental understanding of the performance of each process involved in wastewater and sludge treatment.

While improvement in available information on the cost of constructing and operating alternative sludge management facilities may not be basic research, such improvement is needed in order to attempt cost optimization.

With acceptable performance and cost models of sludge management processes, sensitivity analyses should be conducted to explore the influence of individual process selection and the structuring of systems for sludge management. Optimal means for integrating sludge management and wastewater treatment processes also must be explored. Similar techniques should be used to develop means for evaluating the feasibility of regional systems for sludge management.

SUMMARY

The major fundamental sludge management research need is to acquire an understanding of the basic chemical, physical, and biological properties of sludges. This understanding—coupled with results of research on basic factors influencing performance of processes by which sludges are produced, treated, reclaimed, and disposed—would allow sludge management schemes to be selected, designed and operated more rationally, economically and effectively.

Examples of sludge management needs that could be met by successful conduct of this fundamental research include control of the quality of sludge produced by wastewater treatment, alteration of sludge properties to permit efficient release of water, effective control of putrescible and pathogenic constituents of sludge, selective removal or destruction of toxic constituents, economical and environmentally acceptable combustion, safe disposal, and, ultimately, reclamation of sludge constituents.

REFERENCES

Jenkins, D.W., "Biological Monitoring of Toxic Trace Elements: A Project Summary," U.S. Environmental Protection Agency, 600/S3-80-090 (1981).

Water Pollution Control Federation Subcommittee on Sludge Research Needs, "An Analysis of Research Needs Concerning the Treatment, Utilization and Disposal of Wastewater Treatment Plant Sludges," Water Pollution Control Federation, Washington, D.C., 111 pp. (1982).

SOME THOUGHTS ON PHYSICAL-CHEMICAL ASPECTS OF WASTEWATER MANAGEMENT

James W. Patterson, Ph.D.
Pritzker Department of Environmental Engineering
Illinois Institute of Technology
Chicago, IL

In preparing for this paper, I began a list of all pollutants, domestic and industrial, which are today "managed" to some extent by physical and/or chemical (P/C) technologies. My list, which also included appropriate P/C technologies for each pollutant, quickly became unwieldy, as demonstrated in Table 1 for water supply and domestic wastewater pollutants only. Indeed, I had hardly scratched the list of 129 federally regulated industrial wastewater toxicants, nor began to consider the many spent concentrated industrial process solutions regulated as hazardous under the Resource Conservation and Recovery Act (RCRA). Further, it occurred to me that, with the exception of a very few true chemical conversion/destruction processes, the bulk of the P/C technologies available today simply involve a phase change leading to a concentration (eg. precipitation) or dilution (eg. volatization) effect (Table 2).

This perception led me to consider the consequences of the latter two general categories of technology-concentration and dilution, and ask the following question. Is it really good environmental engineering practice to effectively treat a water or wastewater, but in the process generate a secondary waste of potential major environmental concern such as a hazardous sludge or an air pollutant? It doesn't seem so to me, and I thus propose that we, the researchers and sponsors of research, should primarily emphasize those research areas which involve either destruction, detoxification, recovery or avoidance, rather than those technologies which simply lead to a phase conversion. Phase conversion may be an intermediate, but should not be perceived as the final step in water pollution control. One caveat involves concentration for resource recovery and reuse, as in the application of ultra-filtration or coalescence for oil concentration and recovery.

Further it is my perception, and I suspect a perception shared by many, that much of the historical and current wastewater research in our discipline has decidedly "black box" characteristics. In other instances, the researcher uses synthetic and vastly simplified test solutions for which results rarely translate to real wastes and full scale systems. Yet, the most challenging and stimulating research activities are associated with real, not simple synthesized process liquids. In essence, much of the P/C research literature on wastewaters describes either black box optimization or unreal system experimentation. Although I suspect that there are many

Table 1. Physical-Chemical Technologies for Water Supply and Domestic Wastewater Pollutants.

Pollutant	P/C Technologies
Suspended Solids	Coagulation Clarification Flotation Filtration
Organic Compounds	Sorption Volatilization Chemical Conversion
Inorganic "Nutrients" a) Phosphorus b) Nitrogen	Precipitation/Coagulation Ion Exchange Volatilization Chemical Conversion
Trace Metals	Bioaccumulation Coprecipitation
Infectious Organisms	Chemical Deactivation

Table 2. Classifications of Physical-Chemical Technologies

Category	Example Technologies
Detoxification	Chemical Conversion Thermal Conversion
Concentration	Precipitation Sorption
Dilution	Volatilization Deep Well Injection

reasons for this, one perhaps obvious one is that grantmanship often appears to demand that research be "successful" (i.e., with results that are positive and promising), and thus discriminates against fundamental research which is by its very nature high risk.

Today, a critical need exists for research on existing, new and innovative technologies to cope with an expanding array of nontraditional industrial pollutants, and to control these pollutants to the low levels mandated by public concern, current legislation, and long-term environmental risks.

During the past decade, a considerable amount of my professional effort has been spent in tracking and evaluating pollution control technologies for industrial constituents such as cyanides, heavy metals, phenolics, oily wastes, and the like. Over this same ten year period, industry has invested enormous sums of money in pollution control under the impetus of federal, state, and local legislation. During the past three years alone, industry's investment in water pollution control facilities exceeds $4.7 billion, according to a recent issue of Chemical and Engineering News.

Given these conditions of tightening environmental regulations and major capital investment requirements, one might, in 1970, have anticipated a flood of improved, new and innovative technologies yielding significant performance or economic advantages. My experience, for the two dozen industrial pollutants which I have most closely monitored during this time span, is that there have in fact been fewer innovative technologies implemented than I have fingers on one hand, and the picture for near-term innovative technologies still in the developmental stage is equally bleak.

Perhaps my criteria for innovative technologies are too strict, but I think this is not the case. My criteria exclude relatively minor design or operational modifications to existing processes having a long history of previous application, as well as new processes which incorporate serious adverse side effects such as excessive energy consumption of excessive generation or hazardous treatment residues. To achieve innovative status, I suggest that a technology innovation must incorporate the characteristics of both technological and economic potential as well as achieving an added degree of environmental protection.

I do believe that there are a number of areas of opportunity for research and innovation in the control of industrial wastewater pollutants. Four example areas are described below:

1. Innovative new physical, chemical or biochemical processes for pollutant reduction, elimination, or recovery.
2. Innovative new hardware, which enhance the application for pollution abatement of known environmental control or other industrial processes.
3. Innovative application of existing industrial processes and/or hardware not currently utilized in pollution control.
4. Innovative operational techniques which enhance the reliability of overall performance of established processes.

I maintain however that, although a knowledgeable environmental engineer might cite one or two recent technological innovations within each of these example areas, there have in fact been very few significant breakthroughs in industrial pollution control technology within the past two decades.

Further, our attention has been almost wholly focused upon end-of-pipe treatment, usually of composited, self-diluting waste streams gathered from across a whole industrial facility. It is likely that this "end-of-pipe" technological and regulatory mentality has reinforced the prolification of black box and synthetic waste related research mentioned above. I suggest that the most fruitful opportunities for innovation, particularly with regard to nontraditional pollutants, lie not at the end-of-pipe, but within the manufacturing facility itself, at the very point of pollutant generation and where it is in its most concentrated and consistent form, and isolated circumstance. If we blend one isolated process stream with others of dissimilar nature for end-of-pipe treatment, the opportunity for innovative control is lost, and replaced by the necessity for brute non-selective control procedures.

It is within the manufacturing plant that *manufacturing process modifications*, such as raw feed stock prepurification, or changes in reactor conditions must take place. It is within the plant that innovative *clean manufacturing process* must be introduced, to reduce or eliminate the occurrence of unwanted side reactants or loss of valuable raw materials or product. And it is within the manufacturing plant, at the point of waste generation that the opportunity for pollutant capture is greatest and where we must initiate our research programs in *recovery, recycle and reuse*. I do not exclude the opportunities for innovation in end-of-pipe technology, but I do believe that opportunities for in-plant innovation for industrial pollution abatement at least match, and perhaps exceed these associated with end-of-pipe treatment.

Let me mention some example opportunities which I believe exist within the plant for pollution abatement. Within an industrial context, a pollutant might fall into one of three categories:

1. The pollutant originates as a raw material impurity, or is a product of reaction of that impurity to

yield a secondary undesirable constitute. In this instance, a logical strategy of control might involve prepurification of that raw material, to prevent the entry of that impurity into the reactor.

2. The pollutant may result as either a side reactant, or an intermediate product of the manufacturing process. That is, the pollutant is neither a raw material nor a desired product, but represents a loss of product yield through side or incomplete reaction. Two alternative innovative opportunities which might be available are (a) reactor process modification to enhance the desired product yield and thereby reduce or eliminate the undesired side reactants or intermediates, or (b) the substitution of innovative and new "clean" reaction processes which avoid or reduce the generation of side reaction products or the incomplete conversion or raw material to product.
3. The third category represents the instance when the pollutant actually represents the loss of a valuable raw material or product from the manufacturing step. Here, the opportunities for recovery, recycle and reuse are maximum, since the industry gains both by recovery of a valuable material, and avoidance of the cost of treatment of that material as a pollutant.

Such opportunities for pollutant avoidance, elimination, reduction or recovery might not exist for every pollutant or industrial process. However, even when objectives such as these cannot be achieved, I suggest that capture and treatment of the pollutant at the immediate source point can provide significant benefits for innovative control, through the application of highly specific detoxification/destruction techniques appropriate for the concentrated and isolated process waste stream.

In preparing this paper, I have attempted to resist the impulse to prepare a laundry list of recommended research topics, recognizing two factors. First, such a list would primarily reflect my own selfish interests, and second the list would undoubtedly omit many significant and exciting research areas worthy of our efforts. Within the context of my emphasis today on opportunities for destruction, detoxification, recovery or avoidance however, I would like to close by suggesting a few phenomena with high promise for our discipline. These are summarized in Table 3.

Table 3. Research Areas of Potential High Promise

Area	Comments
Catalysis	Chemical Conversion
Coprecipitation/Sorption	Supression/Enhancement
Extraction, incl. Liquid-Liquid and Distillation	Selective Separation
Metals Speciation and Separation	Selective Separation
Nondestructive Sorption and Desorption	Selective Separation

Catalysis is a well-established and documented technology within chemical engineering practice. Advantages include improved reaction rates, reduction in activation energies, and increased product yield. Catalysis has been essentially ignored in environmental pollution control, however. Coprecipitation, a phenomenon which appears dominated by surface sorption behavior within environmental technology applications, is generally considered as an incidental phenomenon. However, an increased understanding of coprecipitation behavior together with increased ability to either suppress or enhance the phenomenon, may offer unique opportunities.

Extraction, involving liquid-liquid, liquid membrane systems, and distillation among others, is already beginning to have application in some aspects of hazardous waste resource recovery. In my opinion, such technologies represent potentially powerful tools for concentration and recovery from wastestreams, particularly where such wastestreams are treated at their point of generation.

Metals are among the most pervasive industrial pollutants, occurring in wastewaters of widely varying characteristics. Environmental engineering folklore holds that the technologies of metal control are well established. In fact, and with the exception of simple thermodynamic equilibria models which rarely describe real wastewater systems, the exact reverse is true. The majority of metal treatment systems are designed from trial and error bench-scale treatability studies or historical precedent, and operated as black boxes. Little research has been reported on kinetics of soluble and solid phase reactions, side reactions, nucleation phenomena, precipitate solids management, and so forth. These fields of research are wide open, with significant opportunities for potential gains in metals control and recovery.

Our technical literature is replete with sorption research results, but little of it relates to nondestructive sorption followed by desorption for recovery. Both carbon and resins have potential application here. We are only now beginning to see some research related to this area, although the potential is there, and the economics of recovered product may well off-set any added costs.

These are only a few research areas which I believe deserve our consideration. However, I suggest that they all offer the opportunity to enhance environmental control, through the application of innovative research programs.

DISCUSSION

Dr. W. Weber, Jr.
(University of Michigan)

Comment for Patterson:

I would take issue, Jim, with the comment that research is irrelevant if it employs the "black box" approach or is performed with synthetic systems. I do agree that neither approach provides a very satisfactory end in itself. However, the modeling of an ill defined system as a black box may often provide an entry point from which further sensitivity analyses and model refinements can be accomplished to approach development of a conceptual model. The use of synthetic systems, on the other hand, is frequently the only means by which critical variables may be evaluated and quantified under comparable and reproducible conditions. The irrelevance of research with synthetic system relates only to those cases in which no attempt is made to extend and verify the conclusions of such research with real systems.

Patterson's reply:

"Black box" research can only be justified as screening exercise, to identify the general behavior of a system or phenomena. The results of "black box" research can define the trends of a system but except through intuitive guesswork cannot identify or elucidate fundamental mechanisms. My reaction to "synthetic" waste research studies is similar to that of "black box" studies. Synthetic waste studies may provide indicative results, but are equally likely to provide misleading information. Under no circumstances should the results of synthetic studies be extrapolated directly to real systems.

Dr. L. Young
(New York University)

For Grady:

Should environmental engineers be taught more microbiology, or should microbial physiologists enter the environmental arena?

Grady's reply:

Although it may sound flippant to say so, I would have to answer: "Both, particularly for research. "For our typical MS students a good fundamental course in biological process principles is probably adequate. For persons planning a research career, however, and particularly for PhD students specializing in biological processes, it is essential that their education include advanced courses in microbial physiology, ecology, and biochemistry. The nature of the problems facing environmental engineers today are such that "sludge in a bucket" research will no longer suffice. To paraphrase Mark Twain, the easy questions have already been asked and answered. For example, to understand how anthropogenic compounds are degraded in the environment and in treatment systems we must have a much better knowledge of mechanisms. This not only requires us to understand the principles of microbiology but also to acquire the requisite laboratory skills. Consequently, those of us engaged in research, and our students, must be more highly trained. At the same time, some of the required skills are so complex that they can best be provided by a specialist such as a microbial physiologist. Consequently, there will also be many times when we must enlist their assistance in team research to understand what is happening in our systems.

Dr. P. Jones
(University of Toronto)

Comment:

I am always grateful to come to these meetings and see that you have the same kinds of funding problems that we face in Canada. However, it seems to me we are walking around the point. We should be asking ourselves: what are toxics, what are they toxic to, and what is their dose-response relationship? We need an interface between health effects and environmental engineering. We should be concerned about the significance of low concentrations of substances, rather than discussing analytical techniques for obtaining results in the parts-per-trillion range. John Andrews spoke about risk. Risk assessment is necessary if only finite funds are available. We should put various risks into perspective so society can decide. Risk analysis is a critical decision-making tool. Defining environmental risks is the level of fundamental research environmental engineers should do. This, however, means a changed emphasis and more interdisciplinary studies.

Dr. Grady

Comment:

I would just like to say that at the Water Chlorination Conference held in California last year there was a large amount of time devoted to exactly these topics; so we are involved in them.

Dr. C. Haas
(Illinois Institute of Technology)

For Grady:

In your paper you stated that we lack a mechanism to do a critical literature review as opposed to a synoptic review like the Federation does. Would you want to expand on that?

Grady's reply:

In my own work I find that much of my "continuing education" comes from reading good critical reviews such as in the *Annual Review of Microbiology*. In them the authors have culled out the questionable work and have synthesized the remainder into a coherent package from which the novice can learn much. In my view, one thing that has slowed research progress in our field is the lack of such a vehicle. To start one in these economic times would not be easy, but it probably could be done. What we need is a really dedicated individual who is willing to invest the large amount of energy required to initiate and edit such a volume on an annual basis. The topics would be different from year to year and so the main job of the editor would be to establish an editorial board to determine the topics to be covered and then to find experts willing to write them.

Dr. E. Bryan
(National Science Foundation)

For Grady:

Why is the Federation reluctant to do critical reviews?

Grady's reply:

There are two major types of literature reviews: comprehensive and critical. The first is a very useful means of keeping a broad spectrum of readers up to date on the material available in the published literature. The second is useful mainly as an educational tool and will generally be read only by a few people who are experts in a given area and who want to sharpen their skills. Although I have argued in my presentation for more critical reviews, I am not certain that the annual reivew prepared by the Water Pollution Control Federation is the proper place for them to be published. The Federation has a very broad spectrum of members. The reason they started their review was to make available to their members a compilation of literature on a broad range of topics which the readers may pursue in greater detail if they wish. Since the background and needs of the members are so broad, it is unlikely that a critical review would be useful to more than a very small percentage of them. By publishing a comprehensive review, on the other hand, they bring a valuable service to a large number of members, most of whom need no more information than a citation to a specific article which they can read in detail. Thus, the Federation is merely trying to maximize its service to its readers. If they felt that there was sufficient demand for critical reviews they might well respond to it, but via a different publication.

Dr. F. M. Saunders
(Georgia Institute of Technology)

Comment:

I would like to note that at least two of the papers presented here have been based in part on publications from the WPCF Research Committee. There are two WPCF reports on research needs: one on Toxics and the other on Sludge Management.

Dr. J. FitzPatrick
(Northwestern University)

For Gossett:

What kind of interdisciplinary teams are required to make the kind of progress you have been talking about in sludge handling and management?

Gosset's reply:

I hope you won't think me flippant when I say that virtually *all* disciplines are required in the comprehensive investigation of sludge handling and management problems. Ultimate disposal, for example, should involve social science concerns such as risk assessment and public acceptance, as well as investigation of physical, chemical, and biological mechanisms responsible for transformation and transport of sludge constituents.

Dr. J. FitzPatrick
(Northwestern University)

For Gossett:

What about just to look at mechanical dewatering, thickening and so on, to give a narrow focus; do you have a clear number of disciplines which should be involved?

Gossett's reply:

Taking your one example of mechanical dewatering, I would propose that its investigation at the fundamental level might involve physical and surface chemists, applied and fluid mechanicians, and—of course—environmental engineers! There are many challenging, fundamental problems of surface chemistry to be found in the study of sludges, but few of our aquatic chemist colleagues have elected to concentrate on this medium.

Dr. A. Vesilind
(Duke University)

Comment (on FitzPatrick's question):

Let me try to respond to Joe. One of the exciting things about research in sludge is that everyone gets involved. In the jargon of the economists, it's a field with no externalities: you go all the way from surface chemistry to sociology and they are all important.

Dr. A. Vesilind
(Duke University)

Comment (on Lue-Hing's paper):

I am not as confident as Cecil that there will be no significant breakthroughs in the wastewater treatment field during the next 20 years. It is often difficult to predict such changes. For example, I suspect that the manufacturers of slide rules neither anticipated nor were prepared to cope with the sudden availability of electronic calculators, and none of these companies got into the calculator business. I hope that when the breakthroughs in treatment technology occur (and they will!), our profession will recognize these changes and embrace the new technology, and not leave the area of waste management to a perhaps new and as yet unknown brand of professionals.

Dr. B. Lewis
(Northwestern University)

For Patterson:

I want to comment about the "black-box" approach. What we have is the tail wagging the dog. No one mentions the time element. Fundamental research needs more than 6 months or a year, yet many of the grants are at most two years. I wonder how much the factor of time comes into the selection of a black box vs. a fundamental approach.

Patterson's reply:

I agree completely with your comment. Fundamental research requires some freedom from the actual or perceived time scale of current project funding cycles. This problem is aggravated by the need of the researcher to rapidly generate results that can be used to justify renewal proposals, even where the initial grant is supposedly approved for a multi-year effort.

Dr. E. Bryan
(National Science Foundation)

Comment:

With regard to the question asked of Dr. Patterson, you will note from the Lists of Awards made available to participants at this conference, the NSF programs in water resources and environmental engineering made a total of 102 awards in FY 1982, of which one third were continuing grants for more than one year of support and another dozen or so were initiation grants for two years of support. There were no actions taken with any of the 102 awards made that altered the time-frame proposed by the investigators. As a general rule, applicants to NSF are permitted to request up to three years of support and may request renewal of support beyond that initial period by submitting a proposal for renewed support up to a total of five years. Requests for renewed support are, however, evaluated in competition with all other pending proposals. The support provided by NSF clearly recognizes the characteristics of fundamental research identified in the question asked with regard to providing the investigator sufficient time with assured support to meet project needs consistent with reasonable constraints within which a public agency must exhibit responsible management of funds derived from public sources.

Dr. F. DiGiano
(University of North Carolina)

For Grady and Gossett:

I have a question relating to the use and misuse of multiple regression analysis in attacking new areas and trying to understand the relationship between the dependent and independent variables. I'm thinking of Jim's presentation and Les's as well, where you have a number of variables involved and you are not sure what the "black-box" contains. We have heard recently at the WPCF conference, papers on the analysis of performance of final clarifiers using regression techniques. I'm curious— where would you say the use of multiple regression analysis fits in the whole sphere of fundamental research?

Grady's reply:

When we start investigating a new problem we often have little real appreciation for the relative importance of the independent variables. One way to overcome that is to perform first-generation experiments which cover a broad range of values of those variables. Statistical techniques can then be used to evaluate their relative importance, thereby allowing the second-generation experiments to be finer tuned. I am a firm believer in the use of modeling as an integral part of experimental research.

Iteration between modeling and the lab-bench helps to eliminate those experiments of marginal value before they are run, thereby saving valuable resources for the truly significant experiments. It should be remembered, however, that statistical significance only tells us whether we can discern an effect due to the independent variable within the constraints of our ability to analyze for the dependent variable. It may happen that an independent variable really does influence the dependent one, but we just can't discern it with our techniques. Consequently advances in analytical techniques require us to reevaluate past conclusions.

Gossett's reply:

Multiple regression analysis can play a very beneficial role in helping to identify the important variables warranting further investigation. In this role, it can be a useful first step leading to a more fundamental mode of investigation. However, results can be misleading if misinterpreted. For example, if multiple regression analysis indicates the lack of a statistically-significant effect of one variable upon process performance, it *could* be because the experiments were designed or conducted so poorly that a large observed variance precluded observation of significance. In other words, the finding of a statistically-significant effect invariably warrants a meaningful conclusion; lack of statistical significance may not.

Dr. F. M. Saunders

Comment:

I would like to comment regarding what Jim Gossett said. In the area of characterization of sludges, Jim's comments were primarily focused on municipal sludges. If we look at industrial sludges, and I know you didn't ignore them, they are in the Dark Ages in terms of characterization. There is a tremendous need to characterize suspensions that are formed in industry. I'm looking principally at the metal finishing area for example. But those people know very little, in many instances, about the fundamental characteristics. The things that we call right now our best available data, they have very little of. I think there is a tremendous need for research in that area. We produce sludges in those particular areas, but the conditions under which they are produced are very often simply left to chance, and the biggest emphasis is simply neutralization. There needs to be a lot of research focused in that area, and all too frequently the fundamental research I have seen is done on domestic sludges and not focused in industrial areas.

Dr. B. Rittmann
(University of Illinois)

Comment:

Professor Grady's paper was an excellent overview of reseach needs and exciting new possibilities in what can be called the "high technology" aspect of biological processes. I can hardly add to his thorough review. However, another critical area that deserves support is development of processes that achieve conventional goals (e.g., BOD removal) with simpler, less expensive, and more reliable methods. Use of anaerobic processes for sewage treatment, use of denitrification/nitrification systems, and use of robust fixed-film systems are examples of processes that can potentially provide reliable and lower cost treatment. A much better basic understanding of the limits and possibilities of these and other processes is necessary before innovations can be applied.

FUNDAMENTAL RESEARCH IN FATE OF POLLUTANTS

Conference on Fundamental Research Needs
for Water and Wastewater Systems

FUNDAMENTAL RESEARCH NEEDS IN FATE OF POLLUTANTS—SURFACE WATER

G. T. Orlob
Department of Civil Engineering
University of California
Davis, CA

INTRODUCTION

Over the past several decades significant progress has been made in the identification of a myriad of pollutants of our natural water and atmospheric systems. Today we number in the thousands all sorts of chemical and biochemical substances that we can justifiably call "pollutants," i.e. man-created or man-induced products that have either intentionally or inadvertantly been admitted into our limited water and air resources.

Of course, knowing *what* we are dealing with is a first step in pollutant control, hopefully a step that will lead to prevention of pollution—at the source. Yet, what if control at the source is not effective and the substance actually becomes a "pollutant"; what then? What will be its fate in the environment? Will it endanger human welfare or the natural ecosystem? Will it diminish beneficial uses of our water resources? Will it persist in the environment and accumulate; will it degrade to an innocuous state or will it be transformed into other products, equally or even more detrimental to the welfare of the ecosystem?

These are questions for which answers are not easily found, at least not for the great abundance of materials that are admitted to our surface (and ground-) waters. Research is needed—both fundamental and applied—to allow us to predict the fate of a pollutant in the aquatic environment. In this paper I will explore—albeit briefly—a few of the areas where research effort may be profitably expended to give us greater predictive capability—and reliability. Because of my own predilections, I will give particular attention to the tools of prediction, e.g. mathematical models. However, I will try also to identify for those disinclined to model, where they, too, can contribute in the most fundamental ways to a better understanding of the fate of pollutants. I will restrict my comments to surface waters—streams, lakes, and estuaries—leaving to my colleagues the equally, if not more, difficult areas of the oceans, the atmosphere and groundwaters.

As I view needs for research in fate of pollutants in surface waters, they can be divided generally into four groups, dealing with

- Transport processes,
- Kinetics of reactions,
- Transformations of state, and
- Hydrosphere—atmosphere—biosphere interactions

In addition to these—and relating more specifically to the techniques of prediction—are needs for research in the structure of models and data systems to support them. My comments on each of the above topics are as follows.

TRANSPORT PROCESSES

The phrase "fate of a pollutant" implies a need for knowledge of a pollutant's position in time and space. It is fundamental to prediction of "fate" that we have a capability to describe the movement of the medium by which the pollutants are transported. In this case we refer specifically to hydrodynamic behavior, embracing the mechanisms of mass, momentum, and energy transfer. It is seldom sufficient, I submit, to treat the receiving water body as a "continuous stirred tank reactor" (CSTR) or to consider it to be at "steady state"; it is *never* either of these. While there is some justification for such assumptions as engineering convenience, their indiscriminate use leaves unanswered important questions concerned with both temporal and spatial extremes. Of what use is an estuarial model that predicts only a steady state DO of 4 mg/L if the diurnal swing due to tidal excursion is plus or minus 4 mg/L?

There is a need, I believe, to improve further our abilities to describe reliably the following hydromechanical properties of real aquatic systems:

1. Unsteady, nonuniform advective flow in well-mixed streams and shallow, vertically mixed estuaries
2. Lateral and longitudinal mixing due to bottom friction in such systems
3. Wind induced mixing
4. Unsteady, nonuniform flow in density stratified water bodies, e.g. lakes, reservoirs, and estuaries
5. Internal mixing, as represented by traditionally measured parameters describing advective flow, particularly as it may be modified by density stratification.
6. Propagation of hydraulic transients identified with pollutant emissions, e.g. accidental spills.

Over the past several decades substantial progress has been made in developing these capabilities, yet the present state-of-the-art in applying transport models to predict the fate of pollutants does not yet provide an adequate description of mixing processes. Turbulence models like those of Jones and Launder (1973), Launder (1976) and Spalding and Svensson (1976) hold real promise, but as yet the mesh sizes needed to model the important subranges of eddy sizes are so small that computer costs are exhorbitant, even with today's efficient machines and reduced rates. Bedford (1981) has proposed simplified models that preserve the dominant statistical characteristics of turbulence, but as yet these have not been fully tested nor implemented. Practical methods of simplication, allowing the essence of turbulence phenomena to be represented in transport models are in need of development.

KINETICS OF REACTIONS

Process reactions used in modeling the fate of pollutants have most often been described by first order kinetics, with rate coefficients derived either from laboratory experiments or by simulation of the behavior of monitored systems. The literature is replete with coefficients, mostly applicable to specific cases. While these provide some guidance to other researchers or modelers there exists an understandable reluctance to accept such coefficients as applicable to other cases under consideration. There exists a need to collate these experiences from both laboratory and field sources, and to subject them to scrutiny collectively. Benefits of such a review could be

1. Guidance to researchers as to the range of expected values from similar experiments
2. Identification of knowledge gaps
3. Determination of interdependency on other environmental factors
4. Appropriateness of first-order, or higher-order, kinetics
5. Determination of whether or not any universality exists in the values of coefficients.

A good start on organizing coefficients, rate constants, etc. used in modeling is exemplified by the work of Zison, et al. (1978).

TRANSFORMATIONS OF STATE

All too often the dynamic character of chemical transformations of state is ignored on the grounds that it is relatively unimportant at the time scales considered. This may be so, if one is only concerned with steady states and equilibrium conditions. However, when dealing with transient phenomena, e.g. accidental spills, changes occurring at small time scales may be the most important. An obvious example is the transformation of a heavy metal from the acutely toxic ionic state to a much less toxic precipitable form. Yet, one may inquire, as well, whether the new compound is not the more toxic.

Our understanding of reaction dynamics at small time scales (e.g. minutes) is presently very limited. Perhaps this is not surprising, because of the difficulties inherent in developing and applying continuous monitoring techniques capable of following such processes accurately. An example from my own limited experience may serve to illustrate.

In a "shake-down" test of the Diablo Canyon Power Plant on the coast of California the first flush of the condenser system released a 45 kg slug of ionic copper to the marine environment. Concentrations were observed to reach 7700 μg/L within the first quarter hour after discharge commenced, averaging about 600 μg/L for a period of about one hour. Even though copper in the ionic state is usually assumed not to persist at toxic levels in marine waters, being adsorbed or complexed with marine sediments, concentrations in this case were apparently sufficiently high to cause high acute mortalities in abalone near the outfall. This occurred even though ionic copper as such was undetectable in the receiving water. The fact that the affected shellfish are filter feeders suggests the possibility that ingestion of adsorbed or complexed copper could have caused death, but the dynamics of the event pointed strongly to the much more toxic ionic copper as the the culprit. Whatever the case, there existed a need to know the temporal distribution of copper between at least three identifiable-state—ionic, sorbed and complexed.

A review of the literature was unsuccessful in revealing any description of the kinetics of copper transformations needed to model such an episode (the accidental occurrence of which was considered a likely possibility at other power installations). Controlled laboratory experiments, using Cu^{64} as a tracer indicated that the first order rate for transformation of labile ionic copper to the sorbed state on marine sediments was about 0.7 $hour^{1}$ at 25°C. Nevertheless, because of obvious technical limitations in characterizing transformations at these time scales, results such as these must be regarded as tentative. (Orlob, et al., 1980)

Some other cases of kinetic transformations of particular concern in environmental quality management and for which knowledge of rates is insufficient are:

1. Solubilization of iron and manganese from bottom sediments in deep reservoirs (dependent upon anoxic conditions and redox potential)
2. Sorption—desorption of heavy metals on suspended organic sediment in riverine, estuarine and marine surface waters
3. Formation of insoluble precipitates as a result of changes in pH, redox, dissolved oxygen, etc.
4. Liberation of methyl mercury from bottom sediments under anoxic conditions.

In fact, the whole area of chemical, biochemical and biological reactions at the sediment-water interface is, I believe, a priority area for fundamental research on the fate of pollutants in surface waters. This brings me to my final topic area for needed research.

HYDROSPHERE—ATMOSPHERE—BIOSPHERE INTERACTIONS

One has only to trace the potential pathways of a particular pollutant through our environment to appreciate that we cannot characterize its fate in the hydrosphere without describing interactions with the atmosphere and, of course, with life forms. Consider, for example, the fate of a heavy metal emitted as a product of combustion. It may initially be volatilized or introduced in particulate form. Subsequently, it will be transported and dispersed by atmospheric circulation, it may be reacted photochemically, scavenged by precipitation or deposited in dry form, transported with surface runoff, transferred across the air—water interface, sorbed on suspended particles, complexed with organics, deposited in quiescent waters, resuspended, solubilized at low pH's, consumed (in particulate form) by filter feeders and bioaccumulated by higher trophic levels, eventually by man. Even a crude conceptual model of this intricate web of flows, reservoirs and processes will reveal how deficient is our capability to predict the fate of the pollutant. Nevertheless, such an exercise will help us to focus on promising topics for fundamental research.

A few of the hydrosphere—atmosphere—biosphere interactions that I believe should draw our attention are:

1. Atmospheric scavenging and deposition
2. Heat energy transfer at the air—water interface
3. Mass transfer at the air—water interface
4. Nutrient uptake, storage and release by biota
5. Toxicant uptake and excretion by aquatic biota
6. Degradation of toxicants in the hydrosphere
7. Bioaccumulation by filter feeders and higher trophic levels
8. Benthic biochemical transformation and release

FUTURE PROSPECTS

It is tempting to predict that we will make important strides in some of these areas of research in the very near future. However, the realities of waning financial support and shifting priorities in government suggest that we have a bumpy road ahead. In the absence of adequate direct support for fundamental research we will have to look to subsidizing (perhaps this is not the best word) research effort through programs that are oriented to application and operation. In the context of practical engineering problem solving it is always possible to develop new ideas and to find opportunities for meaningful research. We need to bring these from practice into the classroom and laboratory. Instead of soliciting the customary funding sources, let us look more to industrial clientele, state and local agencies—those who have real problems to solve and give our students the opportunities to make their research really relevant to environmental management. I have found, as I am sure you have, that this can be the most rewarding of research experiences.

REFERENCES

Bedford, K. W. (1981) Spectra preservation capabilities of Great Lakes transport models. *Predictive Ability of Surface Water Flow and Transport Models*, H. B. Fischer, ed., Academic Press, NY.

Jones, W. P. and B. E. Launder (1973) The calculation of low Reynolds number phenomena with a two-equation model of turbulence. *International Journal of Heat and Mass Transfer*. 16(6)

Launder, B. E. (1976) Comments on improved form of the low Reynolds number k-*R* turbulence models, by G. H. Hoffman, *Physics of Fluids* 19(5)

Orlob, G. T., D. Hrovat and F. Harrison (1980) Mathematical model for simulation of the fate of copper in a marine environment. *Advances in Chemistry Series No. 189, Particulates in Water*, M. C. Kavanaugh and J. O. Leckie, eds. American Chemical Society, pp. 195-212

Spalding, D. B. and V. Svensson (1976) The development and erosion of the thermocline. *Heat Transfer and Turbulent Buoyant Convection*. Hemisphere, Washington, D.C. pp. 113-122

Zison, S. W., W. B. Mills, D. Deimer and C. W. Chen (1978) Rates, constants, and kinetic formulations in surface water quality modeling. Report to Environmental Protection Agency by Tetra Tech, Inc., TC-3689, 317 p.

CHEMICAL ASPECTS OF FATE OF POLLUTANTS IN SURFACE AND GROUND WATERS

James J. Morgan
Environmental Engineering Sciences
California Institute of Technology
Pasadena, CA

INTRODUCTION

Our present-day notion of the environmental fate, or ultimate disposition, of a pollutant is imbedded in a generalized model or paradigm which views the environmental system as made up of phases, compartments or reservoirs, e.g., surface waters, atmosphere, suspended solids, biota, sediments, soil, ground water, between all or at least some of which the pollutant can be distributed. The concepts of *mass flux* of pollutant between and through the various environmental phases of a system and of material *balance* for compounds and elements in the total system are essential features in relating pollutant emissions to fate and exposure levels in water, the biota and the atmosphere.

Another essential concept for understanding the fates and exposure levels of emitted pollutants is that of transformation leading to *chemical speciation*, i.e., different physical-chemical forms which result from chemical reaction and phase transformations in specific environmental situations. Inorganic pollutants can exist in various chemical forms, e.g., oxidized, reduced, dissolved, precipitated, complexed, free, adsorbed, etc. Organic pollutants can exist in dissolved, precipitated, adsorbed, or complexed forms. Rates of transformations of organic pollutants depend strongly on their chemical structure (reactivity) and upon critical environmental factors (light, temperature, oxidation-reduction conditions, pH, and nutrients). Among the important transformation processes for organic pollutants are hydrolysis, photolysis, oxidation and biodegradation. Inorganic pollutant transformations may include oxidation and reduction, precipitation and dissolution, complexation, photochemical processes, and biologically-mediated processes, e.g., formation of metal-organic compounds. An important pattern that is emerging from recent investigations is that, for a number of pollutants, transformations in water, air, biota, sediments, and soil need to be considered to understand environmental fate.

MODELS

Models for predicting pollutant fate and exposure may be classified as closed-system or open-system models. Either may be treated as stationary-state or dynamic systems. Models are useful for seeing the interelationships among the several environmental phases or reservoirs and for describing the interplay of pollutant mass flows and transformations. Open-system models are essential for an understanding of the relationship between pollutant sources and fate, exposure and persistence of pollutant and transformation products in the *total* environment.

For each chemical species in a compartment or phase of the environment the chemical mass balance may be expressed as

Accumulation = Inputs from sources and other phases − Outputs to other phases and by advection ∓ Losses and gains via chemical reactions

For an environmental system of several phases the total accumulation is simply the summation over all phases. More concisely, for *each* species in each phase j

$$\frac{dM_j}{dt} = T_j \times R_j$$

with M_j the mass accumulation ($V_j \cdot c_j$), T_j the transport fluxes, and R_j the total reaction flux, $R_j = \Sigma r_{jq}$, for q different reactions. By the *fate* of a pollutant is meant: (i) the distribution of ΣM_j over all the environmental phases under specified conditions, e.g., steady inputs and steady-state, and (ii) the relative contributions of the various reaction fluxes, r_{jq}, and advection from the system, to disappearance of input pollutant. By *exposure* is meant the concentration, c_j, in each phase.

A steady-state model of an environmental system ($\Sigma dM_j/dt = 0$) requires: the input flux, the inter-phase partition coefficient vector (equilibrium constants for air-water, water-solid, and solid-biota partitioning), the reaction kinetics matrix (appropriate rate expressions for each phase), and the advection vector (flows to and from the various phases). When all reversible reactions within a phase are rapid (characteristic times of reaction << residence time), equilibrium constant information suffices. Steady-state models for organic pollutants, which combine interphase distributions and kinetic descriptions of processes in water, air, sediments, biota, etc., can prove very useful in predicting general patterns for fates and exposures. Heuristic or *evaluative* models can help prepare the way for simulations of specific systems. However, *simulations* may need to be dynamic models. Then, interphase (diffusive) transport may need a kinetic rather than equilibrium description.

FATE VERSUS CYCLES

The "fates" model is perhaps most useful for assessing environmental behavior and exposure levels for synthetic substances, i.e., those not normally encountered in natural systems. Many synthetic substances are slow to degrade or transform. As a result, they tend to partition in several phases. Naturally-occurring pollutants, e.g., most metals and inorganics, take part in elemental biogeochemical cycles. For these pollutants, the slow or fast "turning of the wheel" from land, to water, to sediment, to atmosphere and so on, driven by different chemical transformations (precipitation, oxidation, complexation, adsorption) is a more apt metaphor than "fate." But the distinction is a matter of time scales. A perfectly non-degradable synthetic compound behaves, in the limit, like an element. Total regional or global distribution would then be determined by transport and physical-chemical state changes. A degradable synthet ic compound eventually enters the global cycles of carbon, nitrogen, sulfur, phosphorus and other elements. For both the *fate* of a synthetic pollutant and the *fate* $\rightarrow$ *cycle* of a naturally-occurring pollutant, the key features to be understood are: the source strengths and physical-chemical forms of *inputs*, the equilibrium and kinetic properties of *interphase transport processes*, the kinetic and equilibrium properties of *reactions* in each phase, and the *outputs*.

NEED FOR RESEARCH

Within this framework of fates and/or cycles of pollutants, their transport and their transformation to products within a multiphase environmental system, we can identify fundamental research needed to improve our understanding of the fate of pollutants. The primary aim of new research should be to improve our physical, chemical and biological understanding of pollutant transport and transformation processes in water and in those other environmental phases which strongly affect the fate of a pollutant. My sense is that this aim involves an emphasis on: (a) kinetic study of organic, inorganic, and combined chemical degradation processes in water and air; (b) field investigations of water-to-air, land-to-air, and air-to-water pollutant fluxes; (c) transport process dynamics for air-to-water, water-to-air, sediment-to-water and water-to-sediment intermedia transport; (d) accurate evaluation of Henry's law equilibrium constants for volatile species as a function of temperature; (e) speciation of organic compounds through binding to dissolved as well as particulate organic carbon in surface waters; (f) photochemistry of organic-metal associations in the water column; (h) integrated investigations of surface waters or ground waters in which the fates of several synthetic organic and inorganic pollutants are studied with respect to *interactions* with one another and with *cycles* of other pollutants, such as sulfur, nitrogen, and carbon.

Major gaps in our knowledge are revealed in attempting to model the non-point source inputs to specific bodies of water from land and from the atmosphere. The "T_j" or transport terms for individual pollutants moving to and from water need greater attention. Most important, perhaps, are the magnitudes of dry deposition and wet deposition fluxes of synthetic organics, metal pollutants and acidity, which can alter natural geochemical cycles in water and sediments through effects on pH. Definition of the volatilization process in dynamic or equilibrium terms requires reliable information on air-water partitioning and volatile substance speciation in water.

Among the chemical processes which may prove important for fate of pollutants in surface water are photocatalyzed oxidations of organics and oxidation-reduction processes between organics and higher oxidation states of metals. Surface-catalyzed redox processes in the water column and at interfaces could link natural geochemical cycles with pollutant fate pathways. Kinetics need to be studied under concentration conditions representative of the actual water-atmosphere-soil systems of interest.

A simplifying assumption in modeling fates of pollutants is that interphase transport processes, e.g., water-air or water-particle, are *fast* compared to chemical reactions and compared to the rates of water movement. For ground water systems this seems reasonable. It appears to merit investigation in surface waters, however. Diffusion-controlled or chemically-controlled transport processes might influence significantly the rate of pollutant transfer to or from water. The appropriateness of a steady-state model rests, in part, on the characteristic times of inter-phase transfer, as well as on the characteristic times of oxidation, hydrolysis, photolysis, biodegradation, and other transformation.

A long-term goal of fates research in water is to understand the important terms in the general model: $dM_{iw}/dt = T_{iw} + R_{iw}$ for each of the i different species of interest in the water phase, w, as well as in the other relevant phases. Achieving the goal in a systematic way demands an organized attack on the transport and chemical terms. The difficulty, and the challenge, will come in revealing the *interactions* among all of the important processes, physical, chemical and biological.

GROUNDWATER HYDROLOGY— RESEARCH NEEDS FOR THE NEXT DECADE

George F. Pinder
Department of Civil Engineering
Princeton University
Princeton, New Jersey

A perspective on the research needs of the next decade requires that one reflect for a moment on past activities. Over the last fifteen years, there has been a virtual revolution in our ability to describe quantitatively the physical behavior of subsurface phenomena. By borrowing some of the established theoretical tools of continuum mechanics and developing several new ones, scientists and engineers interested in groundwater flow and related processes found they are capable of formulating the governing equations describing very complicated porous-flow problems. They found that not only did it become possible to translate these problems into mathematical formulae but, due to the advent of modern computers, the resulting equations could be solved numerically for rather general physical situations.

Just as the ability to simulate subsurface phenomena changed rapidly during this period, so also did the problems of practical importance. The concern for energy resource evaluation generated interest in the simulation of geothermal reservoirs. This led to the development of multiphase non-isothermal models of considerable physical and numerical complexity. At about the same time there arose a general awareness of the growing problem of groundwater contamination. Of particular concern was the migration of trace amounts of organic chemicals to both private and public water supply wells. Some of these chemicals are recognized for their toxicity as carcinogens and mutagens.

Although diminishing quantities of groundwater will present significant and difficult problems, particularly in the arid parts of the nation, groundwater quality will probably be of primary concern for the next decade. It is likely that the problem we face today is only the tip of the iceberg. Because organic compounds are often recognized as toxic in concentrations of parts per billion, or even parts per trillion, very small quantities of the pure compound can contaminate vast groundwater resources. Thus, wherever industrial activity exists, groundwater supplies have been and continue to be potentially jeopardized.

Because the contamination of groundwater by organic compounds is a recently recognized phenomenon, there is a general lack of scientific understanding regarding its occurrence, detection, and evolution. Even if one puts aside the questions concerning laboratory procedures, detection limits and costs, there still remain problems with effective field sampling procedures and a lack of understanding of the fundamental porous media physics involved.

Let us consider first the question of field sampling procedures. Experience indicates that in order to deal with most of the pressing groundwater contamination problems, one must unravel the micro-hydrology of the area. Local variations in stratigraphy, reservoir properties, and fluid potential gradients can profoundly affect the direction and rate of movement of a contaminant plume. In the next decade there should be a concerted effort into developing reasonably priced technology to detect contaminants in the field, to measure groundwater velocities and to determine reservoir parameters. While these needs may be rather apparent, there is a less obvious yet equally important problem associated with optimal sampling strategy. Enormous sums of money are currently being spent to drill holes in order to sample formation fluids and investigate reservoir lithology. Yet, there is no established protocol for conducting these investigations. Today most field programs are designed and executed only through a combination of experience, ingenuity and insight.

Questions associated with how much data are needed and an optimal strategy for their collection are very important and require attention by the scientific and and engineering community.

Consider now the question of reconstructing and forecasting contaminant movement. These questions arise in establishing liability and designing remedial measures for containment and rehabilitation of the reservoir. To establish contaminant movement one must understand the basic physical processes at work in the subsurface. It is probably fair to say that we do not understand very well how highly volatile organic compounds enter the subsurface and remain suspended for decades in the unsaturated zone during which time they contaminate infiltrating precipitation. The ability to simulate the movement of the separate organic phase through the unsaturated zone to the water table eludes us not only because of the inability to write the governing equations, but also because experimental information necessary to expose the fundamental physical processes is needed and lacking. Experiments, preferably coupled to analysis, should be conducted on porous medium columns to determine the mechanics of migration including the saturation-pressure profiles and relative permeabilities of each phase, the thermodynamic relationships and the miscible transport parameters.

The simpler simulation problem of miscible transport in the saturated zone is technically within our grasp provided a complete description of the field problem is available. The field information, however, is expensive to obtain at the level of detail required for reliable forecasting. Moreover, some parameters such as dispersivity, are very difficult to measure, irrespective of cost. It would be very valuable to have new field procedures to obtain transport parameters in a cost effective way.

While it is technically possible to simulate contaminant transport in three space dimensions, an accurate yet efficient algorithm is not available. Unfortunately most field simulations require a transient three space dimensional capability. Thus, we should continue to pursue more efficient numerical schemes, particularly those suited to large scale simulations on mini-computers.

Whereas considerable progress has been made in the description and simulation of conservative porous medium transport and transport with adsorption, very little has been accomplished in describing transport coupled with chemical reactions. The difficulties are both experimental and analytical. Experiments are needed to establish the chemical behavior at the pore level and the associated reaction kinetics. To use this information effectively, there must be a parallel effort in equation formulation and model development. Because the resulting equations are almost certainly going to be coupled and non-linear, it is now time to establish a strategy for their efficient and accurate solution.

Two longstanding and important research areas remain to be considered. Consider first the problem of fractured porous-medium flow and transport. The porous blocks tend to provide the storage in the system, whether it be mass or energy, while the fractures act as the principal conduits to flow. A reservoir of this kind is currently treated using either a continuum approach wherein the geometry and orientation of the individual pores are disregarded or by considering each discrete fracture as a separate entity. Recently efforts have been made to hybridize these two theoretical viewpoints. The ultimate formulation, however, has yet to be developed. More importantly, perhaps, there are few effective methods for obtaining the field parameters required to employ either the separate approach or the hypothesized hybrid. Because the fractured porous medium problem is common to several disciplines, more research is needed in this area.

The second area we have failed to address is simulation under uncertainty. The significance of this problem lies in the inherent uncertainty associated with the estimation of any field parameter. There are two aspects of the problem that require additional effort. The first is the simulation methodology. An efficient, generally applicable modelling capability does not currently exist. Moreover, there appears to be no mathematical apparatus available to provide the theoretical underpinnings for such a capability. The second concern in this general area is the identification of the uncertain parameters. Given suitable field techniques, however, one can in this instance at least conceive of methods to attack the problem. There is little doubt that simulation under uncertainty is a fundamental element of porous medium modelling and deserves careful consideration.

Herein I have provided but one perspective on groundwater research needs for the next decade. The point of view is, of course, flavored by the background and interests of the author. Hopefully, those working in other areas of groundwater hydrology will find neither major omissions in this tabulation nor an inappropriate allocation of priorities.

DISCUSSION

Dr. D. O'Connor
(Manhatten College)

Comment:

I would like to reinforce the need for field investigations. Although they are an order of magnitude more costly than laboratory studies, it is essential that we ultimately undertake large scale investigations. Just as "the proper study of mankind is man," the proper study of the fate of pollutants is in the natural system, itself. It is impossible in many instances to reproduce natural systems and related phenoma in the laboratory, particularly with respect to air-water water-bed interaction. Air-water exchange experiments in laboratories have been somewhat more successful in reproducing the natural environment. Neither of these processes, among many others, will be fully understood until efforts are directed to well-planned and coordinated field studies. Furthermore, environmental disturbances (e.g. PCB in the Hudson and Kepone in the James which resulted in closing of the fisheries) provide an ideal case study for the fate of such substances. Such disasters should be capitalized on by organized and concerted field studies of the distribution of the substances as well as the processes which effect their ultimate fate. Many notable efforts were initiated in this respect, but rarely have they been sustained to the degree necessary. This issue, in my opinion, is one of major deficiencies in our research programs. This approach is essential in checking, validating and improving the existing model structures of dissolved oxygen, eutrophication and toxic substances.

Anonymous

For Pinder:

It is not true that first order methods can be used to solve problems in uncertainty?

Pinder's reply:

It is my opinion that an accurate first-order method capable of accommodating large variances in a transient groundwater simulator does not currently exist.

Prof. M. Small
(Carnegie-Mellon University)

For Morgan and NSF representative:

In the presentation on acid rain effects on water chemistry, it was mentioned that while we have a very active research program in atmospheric modelling, we are doing very little in the way of aquatic modelling. I want to ask two types of questions: The first to the panel, and the second to a representative from NSF.

1. There are quite a few people looking at aquatic effects. These people have not been environmental engineers, but primarily limnologists and fisheries people. Very active monitoring programs are going on, but very little modelling. I am wondering if that is an area that we have missed and should be getting into more.

2. If we are going to get into that, is the NSF the appropriate agency? There are other agencies sponsoring quite a bit of acid rain research. Is NSF essentially deferring to these groups and would they be a more appropriate source to look to for funding?

Morgan's reply:

Aquatic system modeling of atmospheric inputs of various chemicals, among these acids, is in its early stages. I believe that careful field observations are essential. The intelligent design of field programs can be greatly aided by models incorporating transport and chemical transformations in water, soils and sediments. I consider the integrated watershed-water-sediments acidification models of Jerry Schnoor and his colleagues at Iowa to be important examples of a comprehensive approach. The input-output chemical balances of Harry Hermond at MIT on Thorean's Bog and of Peter Kilham of Michigan on Weber Lake are also very valuable. Engineers, chemists and biologists can all contribute to integrated analysis and modeling programs in this area.

Dr. E. Bryan
(National Science Foundation)

Reply:

With reference to the 12th Annual Report of the Council on Environmental Quality (1981), "the Acid Precipitation Act of 1980 (Title VII of the Energy Security Act of 1980-P.L. 96-294) established an Interagency Task Force charged with planning and implementing a comprehensive research program to clarify the causes and effects of acid precipitation. The Task Force is chaired jointly by the Department of Agriculture, the Environmental Protection Agency, and the National Oceanic and Atmospheric Administration. It includes nine other federal entities concerned with acid precipitation research: the Council on Environmental Quality; the Departments of the Interior, Commerce, Energy, State, and Health and Human Services; the National Aeronautics and Space Administration; the National Science Foundation; and the Tennessee Valley Authority. The Task Force also includes the directors of the Argonne, Brookhaven, Oak Ridge, and Pacific Northwest National Laboratories."

NSF is represented on this Task Force by Dr. Francis S. Johnson, Assistant Director, Astronomical, Atmospheric, Earth and Ocean Sciences. Although NSF does not have a separately targeted program related to "acid precipitation," proposals for support of research on acid precipitation may be submitted to any NSF program to which the research is relevant. Information regarding NSF-support of reserach on this topic is coordinated through its membership on the Task Force and its subgroups.

Dr. N. Clesceri
(Rensselaer Polytechnic Institute)

Comment:

The Electric Power Research Institute has put a considerable amount of funding into a study in Upstate New York called the "Integrated Lake Watershed Acidification Study. "It is comprised of many universities, and Tetratech. They are doing some modelling (which is piggy-backing on the hydrologic understanding where there is a better definition of what is occurring) on some of the chemical reactions that are taking place in the soil column interacting with the forest canopy and passing down through the lakes. Because of study priorities, the impact on biota and the incorporation into mathematical models that deal with the lake biota and the impacts of acidity has been put off to a second phase. You are right in saying that the problem has not really been investigated to a great extent, but there is a considerable effort on behalf of it sponsored by the utility industry.

Dr. B. Rittmann
(University of Illinois)

Comment:

In groundwater modeling and much of surface water modeling, the activity of surface-attached microorganisms needs emphasis. Present models are incapable of describing systems with high surface area-to-volume ratios. Effects on kinetics and speciation are important aspects of surface growth.

Dr. R. Luthy
(Carnegie-Mellon University)

Comment:

In general, the discussions at this conference have focused on problems of transport and fate of pollutants in the environment as well as issues relating to processes for water treatment and water pollution control. Each of these subjects has particular problems worthy of continued and aggressive research. I believe, however, that the issues discussed largely reflect the research priorities of the conference attendees. While I do not wish to denigrate the importance of these issues, I would like to address a different subject which certainly will have great impact on the future of our profession.

The problem concerns the physical condition and financing of the Nation's infrastructure elements, particularly water treatment plants, water delivery networks, and wastewater collection and pollution abatement systems. In many parts of the east, for example, large segments of the water supply networks and wastewater collection system have been in service since the turn of the century. As we approach the end of the century, these elements will be in need of substantial renovation and replacement.

This situation poses a unique set of problems and opportunities for environmental engineers. Solution of these problems will entail massive investments, and as a profession we have a responsibility to ensure that resources applied to these problems are employed as effectively as possible for the betterment of the community which we serve. The important need for providing adequate supply of wholesome water and the need for insuring effective means for wastewater removal and treatment requires that close attention and creative thinking be invoked for rehabilitating water and wastewater infrastructure elements. The very magnitude of the problem may necessitate creative solutions.

There exists an opportunity at this early juncture to assess the desirability of the way things have been done in the past and to search for innovative strategies for solving future problems concerning water and wastewater infrastructure elements. An example of an innovative strategy is provided by a colleague who proposes to examine the desirability of ownership alternatives for water and wastewater facilities. He reasons that while a private organization might not qualify for direct federal subsidies, investment tax credits and depreciation allowances would be available to reduce the after tax costs of the facility. A treatment plant might be constructed more quickly and at an earlier and more desirable time since the community need not wait for the availability of federal funds. Integrated planning/design/construction might be undertaken to reduce costs. In sum, the change in ownership from a municipality to a private organization may have distinct advantages.

Research on broad issues relating to rehabilitation of water supply and wastewater systems is probably outside the current research agenda for many who have attended this conference. Nonetheless, as environmental engineers, we will surely be drawn into both technical and policy debates on the subject. For certain individuals it will be appropriate to become actively involved on research problems pertaining to rehabilitation of infrastructure elements. For others there is an obligation to remain informed and knowledgeable on the subject. It is worthwhile to recall that many of the great names in environmental (sanitary) engineering research and practice were active participants in the design and establishment of the present day infrastructure. It is incumbent on our profession to take a lead role in the rehabilitation of these elements.

CONFERENCE CLOSING REMARKS

CONFERENCE CLOSING REMARKS

David Jenkins
Department of Civil Engineering
University of California
Berkeley, CA

I was asked to close the conference with remarks that would focus and summarize the important points made by the speakers in the discussion; and also to point out any areas, that in my opinion, had been given short shrift.

The conduct of fundamental long range research utilizing Federal and other government support is difficult because of the general "crisis" approach to technological problems and goals in this country. We often wait until it is necessary to devise a crash program to solve a problem (i.e., the space *race*, the *war* on cancer, the energy *crisis*, *zero* discharge) rather than working steadily over the long range to provide amelioration instead of quick fixes. Because of this atmosphere, Federal agencies granting money for fundamental research must justify such expenditures in terms of their short-range mission-oriented goals. One of the objectives of this conference, although not explicitly stated, must be to provide such justification and delineation for the particular fundamental research that we in the environmental academe and practice desire to conduct. We should be delighted that we have been asked and should be generous to the agencies for providing us the means to express our opinions.

The system referred to, and supportive conferences such as this one, have their obvious benefits—fundamental research gets supported. Dangers exist also because non-conforming, low priority research may be excluded. We should be cautious that this does not happen to us, especially in an area such as environmental engineering with its diversity of contributing disciplines.

In addition to the above reasons of "context" this conference was convened at a time of shifting national priorities away from concern with the environment and at a time of reduced government spending. Its function, of resetting directions, stimulating ideas and presenting views is doubly important.

In this summary I do not intend to give a blow-by-blow account of each point made by the speakers. Rather I feel that a more productive role would be to convey my perception of the important common themes that ran through the conference. These were:

a. *Basic Properties of Systems.* Almost to a man(woman) the speakers stressed the importance of understanding the physical, chemical, biological and ecological properties of, and interactions in waters, wastewaters, receiving waters and treatment systems. One had the feeling that the days of the empirical collective parameters (BOD, COD, etc.) were numbered and that the "in black box—out black box" measurement and prediction of process efficiency is going the way of the do-do.
b. *Behavior At and Across Interfaces.* Most water and wastewater treatment processes and much of importance in the fate of pollutants involve phase transfers; also we should not isolate our view of treatment and pollution control in water from the surrounding media—air and land (solid). The equilibria, kinetics, reaction mechanisms and transport of species at and across all types of surface, inanimate and biological, is of primary concern.
c. *Dynamic State Rather than Steady State and Heterogeneous Systems (poorly-mixed) rather than Homogeneous (well-mixed) Systems.* In practice and natural systems steady-state and complete mixing are the exception rather than the rule. These conditions, convenient to laboratory study and theoretical analysis, should be extended to the dynamic state and heterogeneous system more representative of "real-life."
d. *Particles.* This general topic might well be considered as a special topic in the category of interfaces. I have given it special attention because of its critical importance in all aspects of water-related environmental engineering. Removal of particles, conversion of dissolved materials to particulates followed by removal, reactions at interfaces, processes in floc and film particles, water quality standards (turbidity), waste water standards (suspended sol-

ids) constitute a very short and incomplete list that conveys the importance of particles.

e. *Genetic Modification of Microorganisms.* Because genetic modification usually requires a knowledge of the biochemistry of the target function to be enhanced, the application to waste treatment processes will probably be in the long-term. Promising areas of application are the ability to bioflocculate and to nitrify at high-growth rates, and the enhancement of biological phosphorus removal.

f. *Modelling.* In addition to the need to generate non-steady state, multi-phase and multi-media models, basic practically-applicable hydrodynamic data and rate coefficients need to be determined.

Some recurring "non-technical" points were made:

a. *Involvement of Pure Scientists.* The concern was expressed that, since advances would require work in basic science areas, it would be necessary to develop joint research efforts with pure scientists of relevant disciplines.

b. *Communication of Results.* Because of the previously-mentioned atmosphere in which fundamental research is conducted it is necessary to inform the body politic, the publics and the practitioners of the results of our research. Communication of results in these ways will help justify the conduct of fundamental research by demonstrating its tangible benefits to society.

The research needs stated in the conference papers and abstracted and generalized here are met by the conduct of fundamental research work. In our present context, such work is supported by the NSF Environmental and Water Quality Engineering Division and the EPA Exploratory Research Grants Program. I have summarized the dollar support for "water-related" research activity in these two programs (Tables 1 and 2). Table 3 is the total of the EPA and NSF programs. Several interesting facts emerge from these data:

a. The NSF program is weighted heavily in the waste treatment area

TABLE 1. NSF ENVIRONMENTAL AND WATER QUALITY ENGINEERING FY82 AWARDS

	$ x 10^3 /year	% of Total
Waste Treatment	977	43
Water Treatment	348	15
Residuals (Sludge)	217	10
Receiving Waters, Pollutant Fate	613	27
Misc. Workshops, Travel, Equipment	106	5

TABLE 2. EPA PEER-REVIEWED RESEARCH GRANTS, AWARDS FY81

	$ x 10^3 /year	% of Total
Waste Treatment	602	26
Water Treatment	552	24
Residuals (Sludge)	71	3
Receiving Waters, Pollutant Fate	1076	47

TABLE 3. COMBINED EPA/NSF AWARDS (Tables 1 and 2 Combined)

	$ x 10^3 /year	% of Total
Waste Treatment	1579	35
Water Treatment	901	20
Residuals (Sludge)	228	6
Receiving Waters, Pollutant Fate	1689	37

b. The EPA program is weighted heavily in the receiving water/pollutant fate area

c. The combined research support for residuals (sludge research) is small.

These distributions of funding support for various research areas probably reflect the response of proposers to the description/announcement of the research programs published; the NSF announcement tends to emphasize treatment more than the EPA announcement. The importance of these documents in selecting research areas is illustrated by this analysis. They represent the mission-orientation of fundamental research.

The small amount of money spent on fundamental research on sludge is a reflection of the off-hand way in which sludge treatment/disposal has been treated in design; it also says much about the lack of knowledge of fundamental properties of sludges that makes sludge processing results unpredictable without pilot-plant testing. It is an area that needs much attention.

In closing, it is a satisfying retrospect that we have a growing fundamental research support mechanism in the Federal establishment. Five years ago at the time of the first AEEP/NSF Research Needs Conference there was no mechanism for peer-reviewed fundamental research to be supported by EPA. The NSF environmental engineering/water resources program was in its infancy. Now we have both and the hope for their continuation and growth. It is up to us in the engineering-science profession to support these programs by the submission of quality proposals, by acting as fair yet critical reviewers, by assisting their administrators in their efforts to promote their programs within their bureaucracies and by communicating our results in a timely and understandable fashion to our publics.

LIST OF PARTICIPANTS

ADRIAN, Donald Dean
Dept. of Civil Engineering
Colorado State University
Ft. Collins, CO 80523

AHLERT, Robert C.
College of Engineering
Rutgers University, Box 909
Piscataway, NJ 08854

ALLEMAN, James E.
Environmental Engineering Group
School of Civil Engineering
Purdue University
West Lafayette, IN 47907

AMY, Gary L.
Dept. of Civil Engineering
University of Arizona
Tucson, AR 85721

ANDREWS, John F.
Dept. of Environmental Science and Engineering
Rice University
Houston, TX 77001

ARBUCKLE, William Brian
Dept. of Civil Engineering
University of Akron
Akron, OH 44325

AULENBACH, Donald B.
Dept. of Environmental Engineering
Rensselaer Polytechnic Institute
Troy, NY 12181

BELL, Bruce A.
School of Engineering and Applied Science
George Washington University
Washington, DC 20052

BENSON, Ronald E.
Dept. of Civil Engineering
Citadel Military College of S.C.
Charleston, SC 29409

BHAGAT, Surinder K.
Dept. of Civil and Environmental Engineering
Washington State University
Pullman, WA 99164

BISHOP, Paul L.
Dept. of Civil Engineering
University of New Hampshire
Durham, NH 03824

BOENING, Paul H.
Production Engineer
Finch Building
Andrews Knitting Mills, Inc.
St. Paul, MN 55101

BRYAN, Edward H.
Water Res. and Environmental Engineering
National Science Foundation
1800 G. Street N.W.
Washington, DC 20550

BULKLEY, J.W.
Dept. of Civil Engineering
University of Michigan
Ann Arbor, MI 48104

BURGOON, Peter S.
New Alchemy Institute
233 Hatchville Rd.
E. Falmouth, MA 02536

BUSCH, A.W.
Graduate Program in Environmental Sciences
University of Texas, Dallas
Richardson, TX 75080

CARDENAS, Paul R., Jr.
Dept. of Civil Engineering
Polytechnic Institute of New York
Brooklyn, NY 11201

CASSELL, E. Alan, Dir.
Vermont Water Res. Research Ctr.
Acken Center
University of Vermont
Burlington, VT 05401

CHAMBERLIN, Charles E.
Dept. of Geography and Environmental Engineering
Johns Hopkins University
Baltimore, MD 21218

CHAPRA, Steven C.
Environmental Engineering
Texas A & M University
College Station, TX 77843

CHRISTENSEN, Erik R.
Dept. of Civil Engineering
University of Wisconsin Milwaukee
Milwaukee, WI 53201

CHRISTENSEN, G. Lee
Dept. of Civil and Environmental Engineering
Cornell University
Ithaca, NY 14853

CHU, T. Julian
Environmental Quality Specialist
Room 106
803 Locust Street
Tennessee Valley Authority
Knoxville, TN 37902

CHUDYK, Wayne
Dept. of Civil Engineering
Tufts University
Medford, MA 02155

CLARK, Mark
Dept. of Geography and Environmental Engineering
Johns Hopkins University
Baltimore, MD 21218

CLESCERI, Nicholas L.
Dept. of Chemical and Environmental Engineering
Rensselaer Polytechnic Institute
Troy, NY 21281

CLIFFT, Ricky C.
Dept. of Civil and Environmental Engineering
P.O. Drawer 1080
Arkansas State University
State University, AR 72467

COLLINS, Anthony G.
Dept. of Civil and Environmental Engineering
Clarkson College
Potsdam, NY 13676

COLLINS, Michael A.
Dept. of Civil Engineering
School of Engineering
Southern Methodist University
Dallas, TX 75275

COLUMBO, Deborah
New Alchemy Institute
233 Hatchville Rd.
E. Falmouth, MA 02536

CORNWELL, David A.
Project Manager
Malcolm Pirnie, Inc.
301 Hiden Boulevard
Newport News, VA 23606

COWAN, Peter A.
Dept. of Civil Engineering
California State University
Long Beach, CA 90840

CRITTENDEN, John C.
Dept. of Civil Engineering
Michigan Technological University
Houghton, MI 49931

DEB, Arun K.
Project Director
Roy F. Weston, Inc.
Weston Way
West Chester, PA 19380

DELUCIA, Russell
MetaSystems, Inc.
10 Holworthy Street
Cambridge, MA 02138

DICK, Marshall
RD 681
Environmental Protection Agency
401 M Street
Washington, DC 20460

DIGIANO, Francis A.
Dept. of Environmental Sciences and Engineering
University of North Carolina
Chapel Hill, NC 27514

ECHELBERGER, Wayne F., Jr.
School of Public and Environmental Affairs
Indiana University
Indianapolis, IN 46223

EDZWALD, James K.
Dept. of Geography and Environmental Engineering
Johns Hopkins University
Baltimore, MD 21218

ELZERMAN, Alan W.
Dept. of Environmental Systems Engineering
Clemson University
Clemson, SC 29631

EWING, Ben B., Director
Institute for Environmental Studies
University of Illinois
Urbana, IL 61801

EZRA, Arthur A.
Water Res. and Environmental Engineering
National Science Foundation
1800 G Street N.W.
Washington, DC 20550

FERNANDES, Joseph B.
Louisiana Tech. University
P.O. Box 10348
Ruston, LA 71270

DEL FERRO, John
Dept. of Geography and Environmental Engineering
Johns Hopkins University
Baltimore, MD 21218

FITZPATRICK, Joseph A.
Dept. of Civil Engineering
Northwestern University
Evanston, IL 60201

FREEMAN, Mark P.
Dorr Oliver Incorporated
77 Havemeyer Lane
Stamford, CT 02904

GHOSH, Mriganka M.
Dept. of Civil Engineering
University of Missouri
Columbia, MO 65211

GLASER, Harold T.
James M. Montgomery Consulting Engineers, Inc.
11800 Sunrise Valley Drive
Reston, VA 22091

GLAZE, William H.
Graduate Program in Environmental Sciences
University of Texas, Dallas
P.O. Box 688
Richardson, TX 75080

GOSSETT, James M.
Dept. of Civil and Environmental Engineering
Cornell University
Ithaca, NY 14853

GRADY, C.P. Leslie, Jr.
Dept. of Environmental Systems Engineering
Clemson University
Clemson, SC 29631

GUROL, Mirat D.
Dept. of Civil Engineering
Drexel University
Philadelphia, PA 19104

HAAS, Charles N.
Dept. of Environmental Engineering
Illinois Institute of Technology
Chicago, IL 60616

HAO, Oliver J.
Dept. of Civil Engineering
University of Maryland
College Park, MD 20742

HARLIN, Curtis
Chief, Drinking Water Staff
U.S. EPA
1111 Army Navy Drive, A-106
Arlington, VA 22202

HASIT, Yakir
Environmental Studies Institute
Drexel University
Philadelphia, PA 19104

HENRY, Glynn
Dept. of Civil Engineering
University of Toronto
Toronto, Ontario
Canada, M5S 1A4

HINESLY, Thomas D.
Dept. of Agronomy
University of Illinois
Urbana, IL 61801

HOVEY, Wendell H.
Dept. of Civil Engineering
South Dakota School of Mines and Technology
Rapid City, SD 57701

HUANG, C.P.
Dept. of Civil Engineering
University of Delaware
Newark, DE 19711

HUANG, Jerry Y.C.
Dept. of Civil Engineering
University of Wisconsin
Milwaukee, WI 53201

HUCK, Peter M.
Dept. of Civil Engineering
University of Alberta
Edmonton, Alberta
Canada T6G 2G7

HUNDT, Thomas R.
Dept. of Geography and Environmental Engineering
Johns Hopkins University
Baltimore, MD 21218

HUNG, Yung-Tse
Dept. of Civil Engineering
Cleveland State University
Cleveland, OH 44115

JACQUEZ, Ricardo B.
Dept. of Civil Engineering
New Mexico State University
Las Cruces, NM 88003

JENKINS, David
112 RFS
1301 South 46th Street
University of California
Richmond, CA 94804

JONES, P.H.
Dept. of Civil Engineering
University of Toronto
Toronto, Ontario
Canada M5S 1A4

JOSEPHSON, Julian
Associate Editor
ES&T Journal
American Chemical Society
1155 Sixteenth Street, N.W.
Washington, DC 20036

KAGEL, Ronald
Dow Chemical, USA
2030 Dow Center
Midland, MI 48640

KAPUSCINSKI, Richard B.
Dept. of Civil Engineering
Engineering Building 1A
University of Michigan
Ann Arbor, MI 48109

KATZ, William J.
William J. Katz Assoc., Ltd.
220 W. Cherokee Circle
Milwaukee, WI 53217

KAVANAUGH, Michael C.
J.M. Montgomery Consulting Eng.
5020 Overlook Avenue, S.W.
Washington, DC 20032

KEINATH, Thomas M.
Dept. of Environmental Systems Engineering
Clemson University
Clemson, SC 29631

KHARARJIAN, Hraj A.
Apartment 31432
Walnut Hall Court
Memphis, TN 38138

KUGELMAN, Irwin Jay
Center for Marine and Environmental Studies
Lehigh University
Bethlehem, PA 18015

LAGREGA, Michael
Dept. of Civil Engineering
Bucknell University
Lewisburg, PA 17837

LAURIA, Donald T.
Dept. of Environmental Sciences and Engineering
University of North Carolina
Chapel Hill, NC 27514

LAWLER, John P.
Lawler, Matusky and Skelley Engineers
One Blue Hill Plaza
Pearl River, NY 10965

LECKIE, James O.
Dept. of Civil Engineering
Stanford University
Stanford, CA 94305

LEE, G. Fred
G. Fred Lee and Associates
Environmental Quality
3 Stump Lane
Slaton, TX 79364

LETTERMAN, Raymond D.
Dept. of Civil Engineering
Syracuse University
Syracuse, NY 13210

LEWIS, Barbara-Ann G.
Dept. of Civil Engineering
Northwestern University
Evanston, IL 60201

LION, Leonard W.
Dept. of Civil and Environmental Engineering
Cornell University
Ithaca, NY 14853

LONG, David A.
Dept. of Civil Engineering
Pennsylvania State University
University Park, PA 16802

LUE-HING, Cecil
Metropolitan Sanitary District of Greater Chicago
100 E. Erie
Chicago, IL 60611

LUTHY, Richard G.
Dept. of Civil Engineering
Carnegie-Mellon University
Pittsburgh, PA 15213

LUTKENHOFF, Steven D.
Environmental Criteria and Assessment Office
U.S. EPA
26 West St. Clair Street
Cincinnati, OH 45268

MALE, James W.
Dept. of Civil Engineering
University of Massachusetts
Amherst, MA 01003

MALONE, Ronald F.
Dept. of Civil Engineering
Louisiana State University
Baton Rouge, LA 70803

MANES, Milton
Dept. of Chemistry
Kent State University
Kent, OH 44242

MCCARTY, Perry L.
Dept. of Civil Engineering
Stanford University
Stanford, CA 94305

MCDONNELL, Archie J.
Institute for Research on Land and Water Resources
Pennsylvania State University
State College, PA 16802

MCGUIRE, Michael J.
Metropolitan Water District of Southern California
700 N. Moreno Avenue
LaVerne, CA 91750

MITCHELL, Gayle F.
Dept. of Civil Engineering
Mississippi State University
Mississippi State, MS 39762

MOLOF, Alan H.
Dept. of Civil and Environmental Engineering
Polytechnical Institute of New York
Brooklyn, NY 11201

MORAND, James M.
Dept. of Civil Engineering
University of Cincinnati
Cincinnati, OH 45221

MORGAN, James J.
California Institute of Technology
Environmental Engineering Sciences
W.M. Keck Laboratory
Pasadena, CA 91115

MUELLER, James A.
Environmental Engineering and Sciences
Manhattan College
Bronx, NY 10471

NEUHAUSER, Edward F.
Dept. of Agricultural Engineering
Cornell University
Ithaca, NY 14853

NOSS, Richard
Dept. of Civil Engineering
University of Massachusetts
Amherst, MA 01003

NOVOTNY, Vladimir
Dept. of Civil Engineering
Marquette University
1515 West Wisconsin Avenue
Milwaukee, WI 53233

O'CONNOR, Donald J.
Dept. of Civil Engineering
Manhattan College
New York, NY 10471

OLIVIERI, Vincent P.
School of Public Health
Division of Env Health Engineering
Johns Hopkins University
Baltimore, MD 21205

O'MELIA, Charles R.
Dept. of Geography and Environmental Engineering
Johns Hopkins University
Baltimore, MD 21218

ORLOB, Gerald T.
Dept. of Civil Engineering
University of California
Davis, CA 95616

OSSENBRUGGEN, Paul J.
Dept. of Civil Engineering
University of New Hampshire
Durham, NH 03824

PAGORIA, Philip S.
University of South Carolina
College of Engineering
Columbia, SC 29208

PARKIN, Gene F.
Dept. of Civil Engineering
Drexel University
Philadelphia, PA 19104

PARRAMORE, Reid S.
Envir. Tech.
Genex Corporation
6110 Ex. Boulevard
Rockville, MD 20852

PATTERSON, James W.
Dept. of Environmental Engineering
Illinois Institute of Technology
Chicago, IL 60616

PINDER, George F.
Dept. of Civil Engineering
Princeton University
Princeton, NJ 08544

PIPES, Wesley O.
Environmental Studies Institute
Drexel University
Philadelphia, PA 19104

QUINNEY, John C.
Research Director
The New Alchemy Institute
237 Hatchville Road
East Falmouth, MA 02536

RAM, Neil M.
Dept. of Civil Engineering
University of Massachusetts
Amherst, MA 01003

RANDALL, Clifford W.
Dept. of Civil Engineering
Virginia Polytechnic Institute and State University
Blacksburg, VA 24061

RAO, Harish G.
Dept. of Civil Engineering
Old Dominion University
Norfolk, VA 23508

REGAN, Raymond W.
Dept. of Civil Engineering
Pennsylvania State University
University Park, PA 16802

RIORDAN, Courtney
Acting Asst. Admin. for Research and Development
Office of Research and Development
U.S. EPA
401 M Street
Washington, DC 20460

RITTMANN, Bruce E.
Dept. of Civil Engineering
University of Illinois
Urbana, IL 61801

ROBERTS, Paul
Dept. of Civil Engineering
Stanford University
Stanford, CA 94305

ROCK, Chet A.
Dept. of Civil Engineering
University of Maine
Orono, ME 04469

ROSENKRANZ, William A.
Waste Management Division (RD-681)
U.S. EPA
401 M Street
Washington, DC 20460

ROSS, Berinda J.
Asst. Manager, Technical Services
Water Pollution Control Fed.
2626 Pennsylvania Avenue, N.W.
Washington, DC 20460

RUDY, Susan
Dept. of Geography and Environmental Engineering
Johns Hopkins University
Baltimore, MD 21218

RUSSELL, Clifford
Resources for the Future
1755 Massachusetts Avenue, N.W.
Washington, DC 20036

SANDERS, Thomas G.
Dept. of Civil Engineering
Colorado State University
Fort Collins, CO 80523

SANDERSON, Jack T.
Asst. Director of Engineering
National Science Foundation
1800 G Street, N.W.
Washington, DC 20550

SAUNDERS, F. Michael
Environmental Engineering
Daniel Laboratory
Georgia Institute of Technology
Atlanta, GA 30332

SCHERFIG, Jan
Dept. of Civil Engineering
University of California, Irvine
Irvine, CA 92717

SHERRARD, Joseph H.
Dept. of Civil Engineering
VPA & SU
Blacksburg, VA 24061

SIMS, Judith L.
Utah Water Research Laboratory
UMC-82
Utah State University
Logan, UT 84321

SIMS, Ronald C.
Utah Water Research Laboratory
UMC-82
Utah State University
Logan, UT 84321

SINGER, Philip C.
Dept. of Environmental Sciences and Engineering
University of North Carolina
Chapel Hill, NC 27514

SMALL, Mitchell J.
Civil Engineering/Engineering and Public Policy
Carnegie-Mellon University
Pittsburgh, PA 15213

SNODGRASS, William J.
Dept. of Geography and Environmental Engineering
Johns Hopkins University
Baltimore, MD 21218

SNOEYINK, Vernon L.
Dept. of Civil Engineering
University of Illinois
Urbana, IL 61801

SPEECE, Richard E.
Dept. of Civil Engineering
Drexel University
Philadelphia, PA 19104

SPEITEL, Gerald E., Jr.
Dept. of Environmental Sciences and Engineering
University of North Carolina
Chapel Hill, NC 27514

STENSTROM, Michael K.
Dept. of Civil Engineering
University of California at Los Angeles
Los Angeles, CA 90024

STINSON, Mary K.
Chemical Engineer
U.S. EPA
Edison, NJ 08837

SWITZENBAUM, Michael
Dept. of Civil Engineering
University of Massachusetts
Amherst, MA 01003

THOMANN, Robert V.
Dept. of Civil Engineering
Manhattan College
Bronx, NY 10471

TITTLEBAUM, Marty
Dept. of Civil Engineering
Louisiana State University
Baton Rouge, LA 70303

UCHRIN, Christopher
Dept. of Environmental Science
Georges Road Laboratories
Rutgers University
New Brunswick, NJ 08903

UNZ, Richard F.
Dept. of Civil Engineering
Pennsylvania State University
University Park, PA 16802

VALENTINE, Richard L.
Dept. of Civil Engineering
University of Iowa
Iowa City, IA 52242

VESILIND, P. Aarne
Dept. of Civil Engineering
Duke University
Durham, NC 27706

WALTERS, Richard W.
Dept. of Civil Engineering
University of Maryland
College Park, MD 20742

WANG, Zhishi
Dept. of Geography and Environmental Engineering
Johns Hopkins University
Baltimore, MD 21218

WEBER, Walter J., Jr.
Dept. of Civil Engineering
University of Michigan
Ann Arbor, MI 48107

WETZEL, Edward D.
Dept. of Civil Engineering
Lafayette College
Easton, PA 18042

WIESNER, M.R.
Dept. of Geography and Environmental Engineering
Johns Hopkins University
Baltimore, MD 21218

WINKLEHAUS, Charles
P.O. Box 4164
Chevy Chase, MD 20815

WOLF, Harold W.
Office of Res. Grants and Ctr.
U.S. EPA ORD (RD675)
401 M Street, N.W.
Washington, DC 20024

YOUNG, James C.
Dept. of Civil Engineering
University of Arkansas
Fayetteville, AR 72701

YOUNG, Lily Y.
Dept. of Environmental Med. & Microbiology
Medical Ctr., New York University
New York, NY 10016