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ABSTRACT

This publication contains the proceedings of a workshop held in Ann Arbor, Michigan to identify the priority Great Lakes environmental research initiatives. The five major objectives of the workshop include the determination of research initiatives, opportunities for university research communities to discuss and recommend future research initiatives, consideration of United States-Canadian joint research initiatives, identification of logical follow-up research, and exploration of priority needs of major users. Topics included in the discussions are water movements; simulations of aquatic ecology; Great Lakes dynamics, scale, and water levels and flows; water quality; and lake-atmosphere boundary layer processes. Two appendices at the end of the publication include listings of the work group membership and of workshop attendees. (MA)

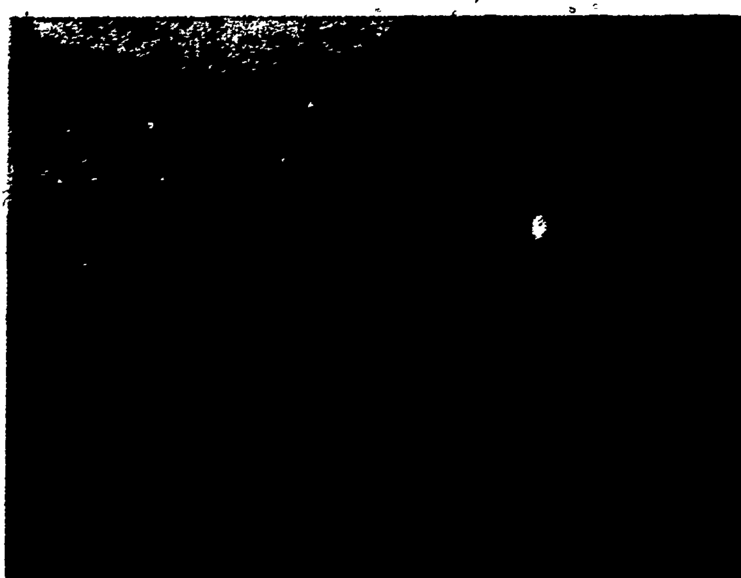
PROCEEDINGS OF WORKSHOP ON

PRIORITY
GREAT LAKES ENVIRONMENTAL RESEARCH
INITIATIVES

HELD OCTOBER 10 11 1974
AT THE
GREAT LAKES
ENVIRONMENTAL RESEARCH LABORATORY

Ann Arbor, Michigan

Dr. Eugene J. Aubert, Program Chairman
Dr. Arthur P. Pinsak, Proceedings Editor



U.S. DEPARTMENT OF COMMERCE
National Oceanic and Atmospheric Administration
Environmental Research Laboratories

APRIL 1975

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FOREWORD

A workshop was held on October 10-11, 1974, at the Great Lakes Environmental Research Laboratory (GLERL) of the National Oceanic and Atmospheric Administration (NOAA) in Ann Arbor, Michigan, to identify future priority Great Lakes environmental research initiatives for GLERL. The International Field Year for the Great Lakes (IFYGL), an ongoing major multidisciplinary research initiative for which NOAA has lead agency responsibility, has completed the data collection phase; research, analysis, and simulation will continue until approximately June 1977. The following question thus becomes timely: What Great Lakes environmental research should be pursued by GLERL as a follow-on initiative to IFYGL on an in-house and contract basis, through multiagency coordination, and with a possibility for joint United States-Canadian participation?

Central to the GLERL mission is the development of improved methods of environmental simulation and prediction in its broadest sense. Several suggestions have been made for future GLERL research initiatives which invoke data collection, analysis, and modeling of nearshore environmental dynamics, nearshore and lake-scale water movements, aquatic ecology, environmental dynamics of Lake Michigan, Great Lakes water levels, flows in connecting rivers, and flooding. Deliberation needs to be given to these and other subjects to arrive at the most pertinent research priorities both from the scientific viewpoint and from the aspect of environmental information to support Great Lakes research management.

The workshop was convened for the following purposes:

(1) To identify future Great Lakes environmental research initiatives (i.e., major research programs of the multimillion dollar, 3- to 5-year duration variety--beyond the normal GLERL resource) required to provide a satisfactory state-of-the-art in environmental simulation and prediction to support the decision process for Great Lakes activities.

(2) To provide the university research community an opportunity to discuss and recommend future Great Lakes environmental research initiatives.

(3) To consider possible United States-Canadian joint research initiatives.

(4) To identify logical research follow-ons to IFYGL.

(5) To provide background for subsequent development of a preliminary research plan by the GLERL staff. This plan will be coordinated with other agencies as appropriate to consider joint research initiatives. The ensuing GLERL program document will be submitted through NOAA channels for support in the FY 77 budget.

The workshop format included a plenary session with perspectives and structured response in eight fields followed by five work group sessions and a final plenary session. The principal speakers in the plenary opening session took stock of the accomplishments and deficiencies of Great Lakes environmental research in IFYGL as well as in other research programs or projects in order to set the stage for the workshop discussion sessions. Of concern is what has been learned and what are the proper scientific questions that should be asked, what research objectives are now logical, and what achievable products can be defined to meet these objectives. Of concern also are the user needs for environmental information. The responders either presented their views on the topic to expand the perspective given by the principal speaker and/or reviewed major points raised by the principal speaker. With the background provided in the plenary session, the individual work groups discussed and identified research initiatives in terms of the following guidelines:

(1) ~~Discuss the state-of-the~~-art of simulation and prediction and identify the research required to further it.

(2) Discuss IFYGL and other research programs/projects in terms of research accomplishments, deficiencies, and the logical next research step.

(3) Identify scientific questions, objectives, and products.

(4) Identify user needs for improved environmental information.

(5) Generally consider methods of approach related to the research sequence (data collection, analysis, modeling, evaluation).

Each work group developed a priority listing of recommendations which was presented at the final plenary session for discussion and reached consensus on a coordinated listing of priority research initiatives.

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INTRODUCTION - E. J. Aubert

Welcome to this workshop at the Great Lakes Environmental Research Laboratory (GLERL). We are the newest research component of the National Oceanic and Atmospheric Administration (NOAA) Environmental Research Laboratories. We plan to have a workshop proceedings; that is why a tape recorder is being used. We felt it might not be reasonable to ask all the speakers to come with manuscripts, so our plan is to record and transcribe the workshop. Proceedings will then be published for limited distribution. The proceedings will be primarily for the attendees and are therefore viewed as a working document. I would like to express appreciation to several people in GLERL who have participated in organizing ~~this~~ workshop up to this point. I would like to acknowledge Art Pinsak, who has been my deputy on this; Bob Bramlet, my Administrative Officer, who has solved many logistics problems; Marlene Hein and Jean Grasso, who helped register you; and Dave Norton and Steve Bermick, who are assisting with projection and recording. Many others are also involved.

I have identified five objectives for this workshop. The first objective is to determine future Great Lakes environmental research initiatives. Many of us have been involved in the International Field Year for the Great Lakes (IFYGL), and although IFYGL is not yet completed, it is desirable at this point to take stock of what we have accomplished both in IFYGL and in other Great Lakes research investigations that we expect to complete in the near term. Then we can look at where to go from here. I frequently use the word simulation in the program outline. The word is used in a broad sense to represent the end product of a sequence of research endeavors involving both field and laboratory observations, analysis to organize the information and to better understand the process and phenomena, and simulation to organize this information in a predictive model. A predictive or simulation model may be theoretical or numerical. A feedback mechanism exists in this research sequence and requires many feedback loops. Modeling is part of the learning process, and as I view it, the simulation or prediction precision represents the state-of-the-art for the environmental science involved. If we simulate or predict poorly, we do not understand very well.

The second objective is to provide the university research community an opportunity to discuss and recommend future Great Lakes environmental research initiatives. We do plan, as a result of this workshop, to prepare an initiative for the budget process through this Laboratory.

The third objective is to consider possible United States-Canadian joint research initiatives. Recognizing that the international boundary runs down the middle of four of the Great Lakes, one must, if one plans to undertake a lake-scale investigation, recognize that advantages may exist for a joint United States-Canadian research program. All initiatives, however, need not be lake-scale. Likewise, one major lake is in the United States.

The fourth objective is to identify logical follow-on research to IFYGL, and the fifth objective is to explore priority research needs of some of the major NOAA users, such as Sea Grant, Office of Coastal Zone Management, and the National Weather Service. In other words, as I see the scope of this workshop and the scope of this Laboratory, some of our environmental research is to support NOAA users. It is postulated that a suitable joint research effort between GLERL and these users would improve the total NOAA environmental product. We visualize the program of GLERL as a joint in-house and grant or contract venture, rather than solely an in-house effort.

The attendees at this meeting fall into two groups: people from NOAA and Great Lakes researchers from universities and private institutions. GLERL has a large representation at this workshop. The following NOAA units are also represented: Environmental Monitoring and Prediction, Marine Resources, Sea Grant, Coastal Zone Management, the National Weather Service, and the Environmental Research Laboratories.

The workshop will start off in plenary session, which will continue through this afternoon. Work group sessions will convene this evening and will continue in the morning. We will then reconvene in plenary session tomorrow afternoon to hear the major points and recommendations of each workshop group. Workshop sessions will not be recorded on tape. The summary of the recommendations presented in plenary session and the discussions which follow will be recorded.

The first plenary session has been structured to give perspective to the workshop. If you look closely at the program, you will see that all the topics in the plenary session today line up with the work group sessions. Item 1.1, the View of IFYGL Research, and item 1.7, Simulation of Environmental Dynamics of the Great Lakes, both are background for the Environmental Dynamics work group session. Two topics also back up the Water Movements work group session: items 1.2 and 1.4, Simulation of Lake Scale and Nearshore Circulation, respectively. Likewise, two topics provide background for the Aquatic Ecology and Water Quality work group; they are designated under those titles. Simulation is being used in the broad sense I described earlier. One plenary session topic lines up with each of the work group sessions on Lake-Atmosphere Interactions and on Water Levels and Flows. I view simulation modeling as the research process which organizes all the knowledge gained from observation and analysis into a predictive framework. Simulation is the end product, although the ability to simulate requires the complete research process.

1. PERSPECTIVES OF GREAT LAKES RESEARCH

1.1 View of IFYGL Research - C. H. Mortimer

What have we learned and where do we go from here? It is really too early to say what we have learned from IFYGL. We are just beginning to look at some of the results, but planning must go on and budgetary planning must be done several years ahead of time; this meeting is therefore timely. But, in order to cut the speeches and get to the debate, I have prepared a handout (table 1) which is by no means comprehensive. There are others here who can, of course, go back into the history of IFYGL when it was a gleam in the eyes of the founding fathers of the steering committee. Dr. Chandler is in a much better position than I to tell you about that. I joined at a much later stage, with the Water Movements working group.

The IFYGL program was in many ways unique in that it was the first large-scale attempt to study the physical limnology of the Great Lakes. The major institutions on both sides of the border took part; six research vessels, a number of smaller craft, and over 600 scientists and technicians from both sides of the border were involved. So, on the Great Lakes scale at least, it was "big science." It started as a component of the International Hydrological Decade program and therefore hydrology had an important role. Meteorologists came into the program early and played a great part in it. I am not competent to speak of the results in that field, but I am sure others will do so.

Then, at a fairly late stage, resulting from proposals some of us made at a meeting of the International Association for Great Lakes Research, biology and chemistry were added. Of course, IFYGL, like many such large scientific projects, had the task of selling the programs to governments. The presence of a water quality component helped, but it was also significant that this was an important attempt to weld the program in physical limnology to biology and chemistry and to bring the principal users into the picture at the beginning of planning.

Table 1 lists a number of themes. It is not complete, and I am sure others will be added.

How are we going to measure the progress of IFYGL? I believe progress will be measurable mainly in three main categories listed as columns I, II, and III. As this workshop continues, I hope you will be able to fill in some of these columns--they are left blank at the moment. It is a game you can play as the workshop proceeds. Column I lists progress in estimating known effects or better estimates of things we have known already but need to know with a greater precision. Column II relates to new discoveries or improved understanding of operating mechanisms, and column III, already referred to by Gene Aubert, relates to predictive modeling capability which environmental management needs and is willing to pay for.

Table 1. Progress in Accomplishment of IFYGL Objectives

IFYGL THEMES	PROGRESS IN*: I. Estimating known effects	II. Discovery or improved understanding of mechanisms	III. Predictive modeling capability
Water Quality: (a) in basin; inflow, outflow; evaporation--methods compared (b) in air mass			
Energy fluxes in and between air and water. Lake heat budget			
Air motions: air/water interactions, **M			
Water motions: (a) surface waves - short - long, **M (b) internal waves - short - long, **M (c) currents--whole-basin circulation patterns, **M; nearshore patterns (d) diffusion and dispersal			
Sediment distribution and inputs			
Water quality: inputs and exchanges of dissolved materials - nutrients, **M - toxins			
Biological studies: (a) surveys (b) dynamics, **M			

* on a variety of space and time scales.
**M, substantial modeling effort attempted.

The first theme is Water Quantity, i.e., basin inflow, outflow, and evaporation estimated by various methods. What was attempted was a large-scale Lake Hefner experiment. In the classic experiment on Lake Hefner, the water budget and energy budget methods were used to provide estimates of evaporation, that important but usually ill-defined term in the water balance equation. It will be interesting to see how much further progress has been made as a result of the IFYGL work. My guess is that we can put a small plus in column I there. Others will be talking about the attempt to estimate water quantity in the air mass.

A lot of effort was put into another theme, energy fluxes in and between air and water. Most of the ship time was taken up in measuring thermal structure of the lake and its changes with time. It will be interesting to see how much closer those estimates are and how much further we have proceeded beyond Sweers' (1969)[1] summary of knowledge of Lake Ontario heat budgets, published before IFYGL started. My own guess here is that we shall be able to put a small plus under column I, a query in column II, and a small plus under column III because an improved estimate, of course, gives improved predictive modeling capability. My strategy in making such sweeping and certainly debatable statements is to generate discussion. If I may insert a conclusion from later remarks on "Where Do We Go From Here," I believe that future investigation should concentrate on the dynamics of key mechanisms, rather than repeating the use of research vessels for large-scale surveying of quantities that we know already to a fair degree of accuracy.

I will touch briefly on the remaining headings in table 1. Where substantial modeling efforts were attempted, I have inserted **M.

Water motions fall into various classes depending on the space and time scales that were considered; there were programs on short surface waves, long surface waves, and seiches and associated modeling efforts which were quite successful. There have been a number of notable advances, for example, of D. B. Rao's normal mode analyses and Paul Hamblin's treatment. A good set of water level measurements is available for verification, and new results concerning both the gravitational and rotational modes of Lake Ontario have been obtained. A large set of observations of internal waves, both short and long, was made using several instruments, particularly Farrell Boyce's thermistor chain and the undulating transducers that we towed from the research vessels. Analysis has only begun, but some of the patterns are beginning to emerge. I believe these will be focused more clearly when we recognize the episodic nature of the forcing functions. We can already put a small plus in column II as a result of the discovery of internal surges on the upwelling fronts.

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[1] Sweers, H. E. (1969), Structure, dynamics and chemistry of Lake Ontario, Marine Sciences Branch, Department of Energy, Mines and Resources, Ottawa, Canada, *Manuscript Report Series* No. 10, 227 pp.

Whole-basin circulation patterns have been successfully modeled (Bennett, Simons), warranting large pluses in columns II and III. Another strong feature of the IFYGL program was the attention paid to coastal circulation and coastal currents through the setting up of coastal chains both on the Canadian side and on the United States side under the respective leadership of Profs. Csanady and Scott. They will present some of the initial results, so little needs to be said here. I predict that we shall be able to log some large pluses in columns II and III.

The work done on diffusion and dispersal by Murthy, Kollenberg, Csanady and colleagues has provided much improved estimates of the horizontal and vertical dispersion and diffusion coefficients. Therefore I believe that all three columns will register pluses.

Deep lake sediments were not studied in detail under the IFYGL program, although there was some work, particularly at Canada Centre for Inland Waters (CCIW), in sediment distribution and inputs. Water quality and biological studies were added to the program at a later stage, and these will be reviewed later in this workshop. For these I believe we can insert pluses in column I now, perhaps a plus in column II later, and in due course a plus in column III.

"Where to From Here" is the main theme of this workshop and I have made a few suggestions of my own on table 2. You may wish to scan first the material at the top and then go to the "Preparatory Work in Advance of New Field Programs."

If I may expand a little on some of the points summarized above, I make a strong plea for thorough analysis of the IFYGL findings to exploit fully the investment. We all know of examples where this was not done because funds dried up after the field work was completed. For example, consider a $13 million program on the Great Lakes just over 10 years ago. Few results have been published; others are still in limbo; great efforts have largely been dissipated. That, I sincerely hope, will not happen to IFYGL. Therefore, we should strongly press for thorough digestion and exploitation of the present findings, if only because we can take steps forward on their foundation.

My second plea is for "prior modeling," i.e., modeling <u>before</u> the experiment is designed. We had this in mind during IFYGL planning, but we did not have time or funds to do it.

My third plea is for prior instrument development and reliability testing. IFYGL has had its successes and its failures. Perhaps we do not want to dwell on the failures, except to learn from them. On the U.S. side, the planning of water movements instrumentation left a great deal to be desired. Instrumentation requirements were considered by the water movements panel, but the design plans were not. The scientists should have made a recommendation as to whether the adopted real-time telemetry was worth the additional cost. In the end,

Table 2. Selected Future Lines of Research Including Modeling

1. Vertical motions and structure--air/water interactions; vertical fluxes of momentum and heat (buoyancy), two- and three-dimensional modeling of thermocline history.
2. Horizontal motions and dispersal--scales and mechanisms; horizontal shear effects (see 3, which follows).
3. Inshore/offshore exchanges and partition of energy--mechanics of upwelling and subsequent whole-basin responses; shore-trapped long waves; generation and decay of nearshore currents.
4. Assembly and critical review of all available chemical and biological data for the purpose of model testing, model development, and design of effective long-term monitoring strategies.

Preparatory Work in Advance of New Field Programs

1. Thorough analysis of present IFYGL findings to exploit fully previous investment.
2. Prior modeling to focus on key questions and to improve design of experimental programs along the selected research lines.
3. Prior instrument development and extensive reliability testing, under rigorous field conditions, designed to provide answers to key questions identified under 2, above.
4. Encourage interagency and interinstitutional planning to optimize use of research platforms and funds.

little use was made of this feature. Also, as is so often the case, what appear to be small details of design and seamanship can largely determine success or failure. For example, during IFYGL, it was not possible to change the gas cylinders on the Texas Instrument buoys in rough weather because the buoy casing was awash. Breakdowns, coupled with a 30-hour limit in the backup tape, led to considerable loss of data during particularly interesting episodes. Standard, well-tried, self-contained instruments of conventional design used by CCIW were more successful.

My final plea is for interagency and interinstitutional planning of the kind we are starting today. We must recognize that a number of agencies, on the U.S. side at least, are developing plans for Great Lakes research on rather a large scale. Although they have different aims and missions, each agency is looking to a similar type of lake research to answer particular questions. Therefore, in order to obtain maximum benefit from expenditure of the federal dollar, coordination is called for.

1.1.1 Response - D. C. Chandler

In an overview of the IFYGL program, it seems to me there are two categories of benefits derived from that experience: (1) direct and concrete gains in the form of scientific data and technological advancements and, (2) indirect and intangible benefits in the form of attitudes, viewpoint, and general philosophy about the Great Lakes.

The first category has been summarized by Dr. Mortimer and the remainder of the day will be given over to a discussion of specific and detailed scientific gains. Therefore, I will confine my brief remarks to the second category—indirect and intangible benefits.

I feel that the IFYGL program influenced the attitudes and viewpoints of Great Lakes investigators in many ways, but I will comment on only four aspects for the sake of brevity:

(1) It was the first successful attempt at a multidisciplinary study of a Great Lake with special emphasis on the total system (biological, chemical, and physical processes and phenomena of the lake water and the interactions of the water with its atmospheric and geologic boundaries). The components or elements of the program were not necessarily original or imaginative, but rather they consisted of current procedures, methods, and technology. However, it demonstrated without question the advantages of this approach over the results of individual or small group effort.

(2) It demonstrated that the Great Lakes are mesoscale aquatic systems, requiring the application of oceanographic methods, equipment, design of field study, management procedures, and level of funding in the conduct of research. It further demonstrated the feasibility and desirability of multiple ship synoptic coverage of the lakes.

(3) It also emphasized the importance of an international cooperative effort involving government agencies, industry, and academic institutions. No single organization possess the total capabilities to study adequately the Great Lakes as a total system.

(4) It created an opportunity for many interested scientists to become involved in a way totally closed to individuals or small groups.

As one who has been involved in Great Lakes research for more than three decades, I am greatly impressed by the present-day wide acceptance, among Great Lakes researchers, of the multidisciplinary, cooperative approach to Great Lakes investigations. Prior to the field year, the predominant attitude among the academic scientific community was one of emphasis on individual effort with complete freedom to pursue a specific problem which required low levels of funding. The field year program afforded an opportunity for the first time for academic scientists to participate in a cooperative effort involving their specific interests and with higher levels of funding. I believe these scientists are in a favorable mood to continue this kind of involvement, and I sincerely hope that in the near future another multidisciplinary cooperative Great Lakes program will be launched. It is to be hoped that such a program would build on the experience of the field year by avoiding the mistakes of that effort and strengthening the areas of success.

1.1.2 Response - F. Aubert

I will give a brief overview of IFYGL. The point has been made that a lot of analysis must still be accomplished. The IFYGL schedule includes plans to continue the IFYGL analysis phase until 1977. It is desirable to give perspective to the research accomplishments and deficiencies, although I recognize that my perspective is incomplete. IFYGL is so broad that I doubt anyone, Professor Mortimer excepted, can adequately define all the IFYGL major accomplishments and deficiencies. I suggest three questions or objectives for future research. These objectives are limited. Participants will have future research ideas to suggest for consideration and discussion in this workshop.

IFYGL addressed Lake Ontario and the Ontario Basin (fig. 1). At the time of this workshop, near the end of 1974, we have completed the first four scheduled activities (table 3) and the data management-archive generation is nearing completion. A large data base is being generated in both the United States and Canada. Several years remain in the analysis phase. We anticipate many more results from the research analysis phase than what we have accomplished to this point.

3. IFYGL Schedule

ACTIVITY	1971	1972	1973	1974	1975	1976	1977
Develop technical plan	—	—					
Prepare for field program	—	—					
Field year operations		—	—				
Engineering, tests, and data system comparisons	—	—	—				
Data management-archive	—	—	—	—	—		
Analysis	—	—	—	—	—	—	—

Table 4 is an overview of the IFYGL scientific objectives and projects. Eight different major projects address these three objectives.

Table 4. IFYGL Projects and Objectives

SCIENTIFIC OBJECTIVES	PROJECTS
. To determine large-scale processes	Atmospheric water balance Lake heat balance Terrestrial water balance Evaporation synthesis Materials balance
. To determine small-scale distribution, variability, processes	Atmospheric boundary layer Water chemistry and biology
. To model limnological, hydrological, and meteorological properties	Atmospheric boundary layer Terrestrial water balance Water chemistry and biology Water movement

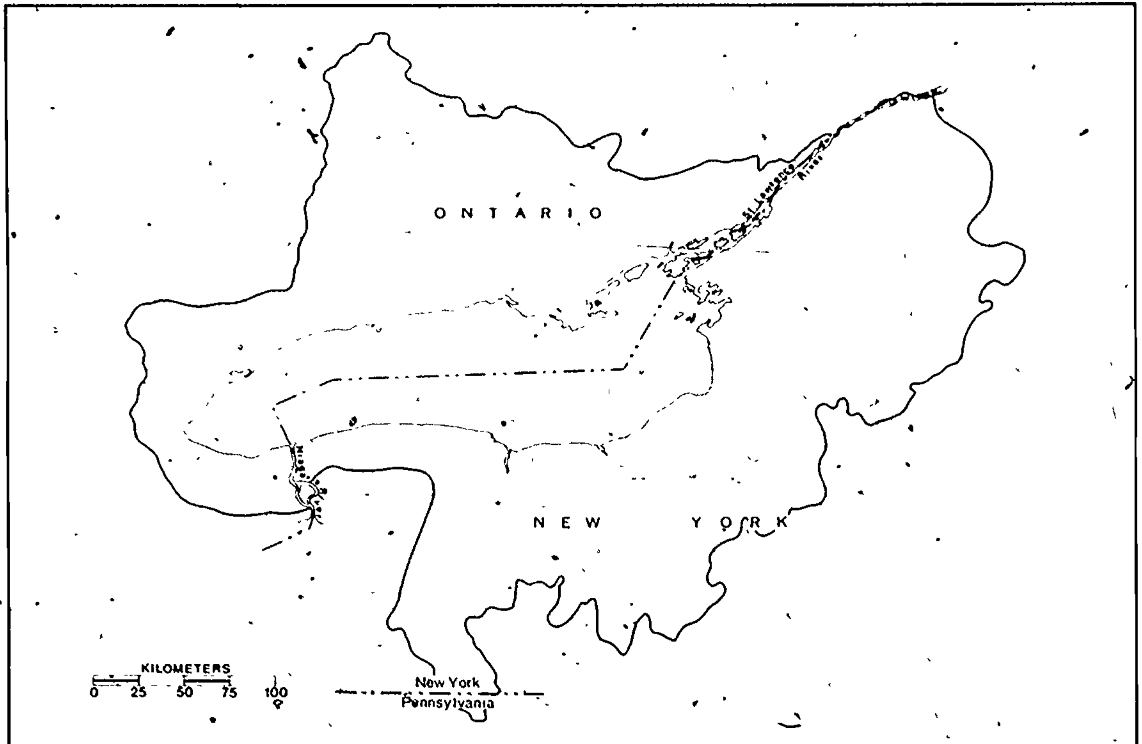


Figure 1. Lake Ontario drainage basin.

Table 5, shows the major planned scientific-technical products.

Table 5. Major Planned Scientific-Technical Products.

- Analysis of budgets--lake, basin, atmosphere
  - Water, heat, materials
- Analysis of natural distribution and variability--what and why
  - Physical quantities, chemical concentrations, biological properties
- Develop and test models for analysis, diagnosis, prediction and simulation of interdependent physical, chemical, and biological properties
- Reports
  - IFYGL scientific reports, articles and agency scientific reports, technical reports on data acquisition systems

Only a few of these products have been achieved at this time; all are anticipated by 1977. Eight major IFYGL international Summary scientific reports are planned for completion during 1975, 1976, and 1977.

Table 6 lists the major IFYGL accomplishments and deficiencies as I see them at this time. The data collection phase is completed; we had some successes and some failures. A large data archive will result. The natural distribution and variability (NDV) analyses include budgets and small-scale distributions for the various projects listed. Little variability analysis will result from the chemical and biological program since no suitable data were collected for this purpose. Likewise, little variability analysis will result for mean and eddy transports of chemical constituents. With respect to model research, several significant efforts are underway and significant success has been achieved. I am not aware of any predictive modeling research that is underway in the nearshore at this time, but research plans may be initiated within the next year or so. There is little test and evaluation of these models due to the fact that they are relatively new. Joe Simons's model development of the physical circulation was perhaps one of the first, and his model is at the most 2 years old. His first model has had several versions.

With regard to publications, we prepared a proceedings of the IFYGL symposium held at the American Geophysical Union (AGU) meeting in April 1974. In April 1973, there were about 20 papers presented at the Great Lakes Conference and published in the International Association for Great Lakes Research (IAGLR) Proceedings. In August 1974, there were 54 IFYGL papers presented at the Great Lakes Conference, and I would expect that next year there may be even more.

Table 6. IFYGL Accomplishments and Deficiencies to Date

- Data collections--complete
- Some successes, some failures
- Large data archive nearly complete
- Analysis--natural distribution and variability
- Budgets
  - Terrestrial water budget, atmospheric water budget, lake budget, mass balance
    - Preliminary analyses complete
- Small-Scale
  - Water movements and boundary layer
    - Variability analysis partly complete
    - Some episode analysis
  - Chemical budget
    - Status of lake surveys nearly complete
    - Little variability analysis
  - Transport
    - Little variability analysis
- Models, simulation
- Water movements
  - Lake-scale circulation
    - Several developed; limited testing and evaluation
  - Nearshore circulation
    - None developed
- Boundary layer
  - Mesoscale phenomena and processes
    - Several developed, limited testing and evaluation
- Chemical budget
  - Water quality
    - Several under development, no testing and evaluation
  - Ecology
    - Under development, no testing and evaluation
- Publications
  - Proceedings of American Geophysical Union Symposium
  - Papers for International Association for Great Lakes Research Conference (54)

Some ideas for questions or objectives for future research are contained in table 7. There ought to be better evaluation of lake-scale circulation models. A second item refers to the nearshore--there have been interesting studies in IFYGL and interesting results of nearshore phenomena. We do not fully understand, at this point, the mechanisms of the nearshore jet and the transport and exchange processes. Hypotheses have been postulated, but they cannot be adequately tested with the data collection in IFYGL. The chemistry and biology research was a late entry in IFYGL. A lot of research is underway, and significant accomplishments are anticipated. The chemistry and biology data collections are, however, of insufficient intensity to support variability analyses and detailed ecological model development. The third item on table 7 recommends more intensive chemical and biological experimental and model research focusing on typical nearshore regions.

Table 7. Questions and Objectives for Future Research

- To evaluate lake-scale circulation models,
  - Determine uncertainity, refine models
  - Apply models to management questions
- To determine variability of nearshore circulation and materials transport,
  - Improve understanding of processes
  - Develop and test simulation model
  - Apply models to management questions
- To determine variability of C and B properties in a typical nearshore region,
  - Develop and test models (1-D, 2-D, 3-D) to simulate observed variability
  - Apply models to management questions

1.2 WATER MOVEMENTS: SIMULATION OF LAKE-SCALE CIRCULATION - J. Bennett

Prof. Mortimer's outline had three boxes: estimation of known effects, progress in discovery or improved understanding of mechanisms, and predictive ~~modeling~~ capability. While Prof. Mortimer is inclined to give pluses in most of these boxes, I am more skeptical. I think we have just barely scratched the surface in modeling. Most progress so far has been in understanding the models, which is a far cry from understanding the lakes. I think we have elucidated quite a few of the physical mechanisms that should theoretically happen in the lake. We had a fairly foggy view of some of them before, for example, propagation of low frequency waves, and I think they have been shown theoretically and observationally to be important. As far as quantitative prediction of these processes, I think we are still a long way off. The mechanisms in the model are probably just being understood now, and it took a long time to do that.

To give just a brief review of the modeling that did take place in the IFYGL program, there were seven numerical models of Lake Ontario. That says something already. Some people consider it undesirable to have this many; others, myself included, consider this healthy competition in most cases. The models have been my own, Joe Simon's, Nobuyoshi Baba's, a student at Princeton (the model was for his thesis). The thesis was very interesting, and I think it will turn out to be one of the cheapest contributions to the IFYGL program since it was done without any support from the program. It is the only model I have seen run for the whole navigation season. It is a 17-level three-dimensional model which runs for 8 months at a time using typical winds. Another model is that developed by Pandolfo and Jacobs, an air-sea interaction model. Bonham-Carter and Thomas of the University of Rochester have developed a model of the Rochester Bay area. This approach has a lot of potential and I think it may be what we will be looking for in the future. Most of the applications on the Lakes involve smaller scale shore-based operations, and the University of Rochester model is the first step in matching a small-scale shore-based model to a large-scale lake model. Another model developed in the last couple of years was David Paskausky's. The other numerical model, to make it complete, was D. B. Rao's two-layer model. He is using it to understand internal waves and seiches in lakes.

There are this number of models because a lot of models of natural bodies of water can be applied to Lake Ontario.

There is a lot of activity in numerical modeling, but I do not think most of the progress has been made in that area. Many analytical studies have had much more effect on the design of the IFYGL experiments. The simple two-layer model of Csanady, for instance, had more effect on the design of the program than any of the numerical models. One of the weaknesses of the IFYGL program was that there was no thought given to using models to design the program. Compare that to the Mid-Ocean Dynamics Experiment. A workshop was held a year or two before they designed the program; a group of people got together at the National Center for Atmospheric Research and played with numerical and analytical models to design the right observing network for the program and to try to guess what they would measure. In any physical problem, it is always a good idea to try to guess what the results are going to be, even if it does not turn out that way.

I have a couple of fairly concrete suggestions for designing field programs. First, I do not think the Great Lakes research community should devote much effort to developing numerical techniques or put too much emphasis on the technical details involved in modeling. I am not saying that the Great Lakes community cannot afford to fund projects that are simply concerned with the technical details of modeling. There is a huge literature in numerical modeling and mathematical modeling in other fields which can be used and we just cannot add much. There are a lot of well-known techniques in numerical weather prediction (such as matching small-scale models to large-scale models) that are not being applied very well to the Great Lakes.

Technical details will not be dwelt on because I assume anybody who can do modeling can read those himself. We can take for granted that most people in this room could generate a numerical model, and I do not consider that a big feat anymore. The main difficulty with numerical modeling is lack of insight into the Great Lakes in order to apply them, and this turns out to be a very difficult problem.

To give an example where I think numerical models have some definite weaknesses, I am using Bob Pickett's slide of July temperatures during the IFYGL program (fig. 2). The basic features of this temperature distribution have been understood for a long time. We do not need IFYGL to tell us, for instance, that in July there is a residual pool of cold water at the bottom of the lake, or that the south shore is warmer due to downwelling and perhaps the inflow of the Niagara River is quite warm this time of year, or that there is upwelling on the north side.

Figure 3 shows the resultant current field in July at 15 m depth, and the dynamic height patterns can be calculated from the temperature field. What is interesting about them is that they seem to be internally consistent. There seems to be a big cyclonic circulation of the lake. The dynamic height method seems to work in estimating the currents. If one did not try to predict these currents with a model, the explanation of a big geostrophic gyre would be quite satisfactory. Unfortunately, none of the models give this. I think there is something fundamental going on here that we really do not understand. If we have an understanding of the mechanics of the models, then they can help us understand basic phenomena like this. It is easy in any model to get eastward flow on the south shore, but the trouble is getting the flow to turn around and go back west on the north shore. As you know, the prevailing wind is from the west and tends to drive the flow in shallow water toward the east.

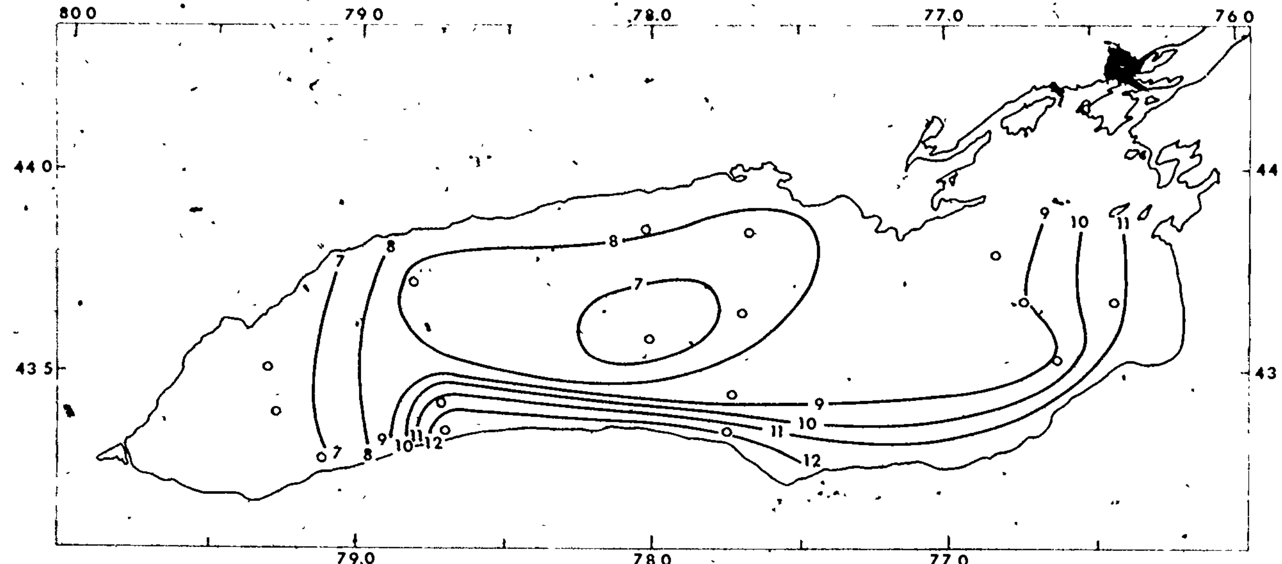


Figure 2. Lake Ontario water temperature (°C) at -15m, July 1972.

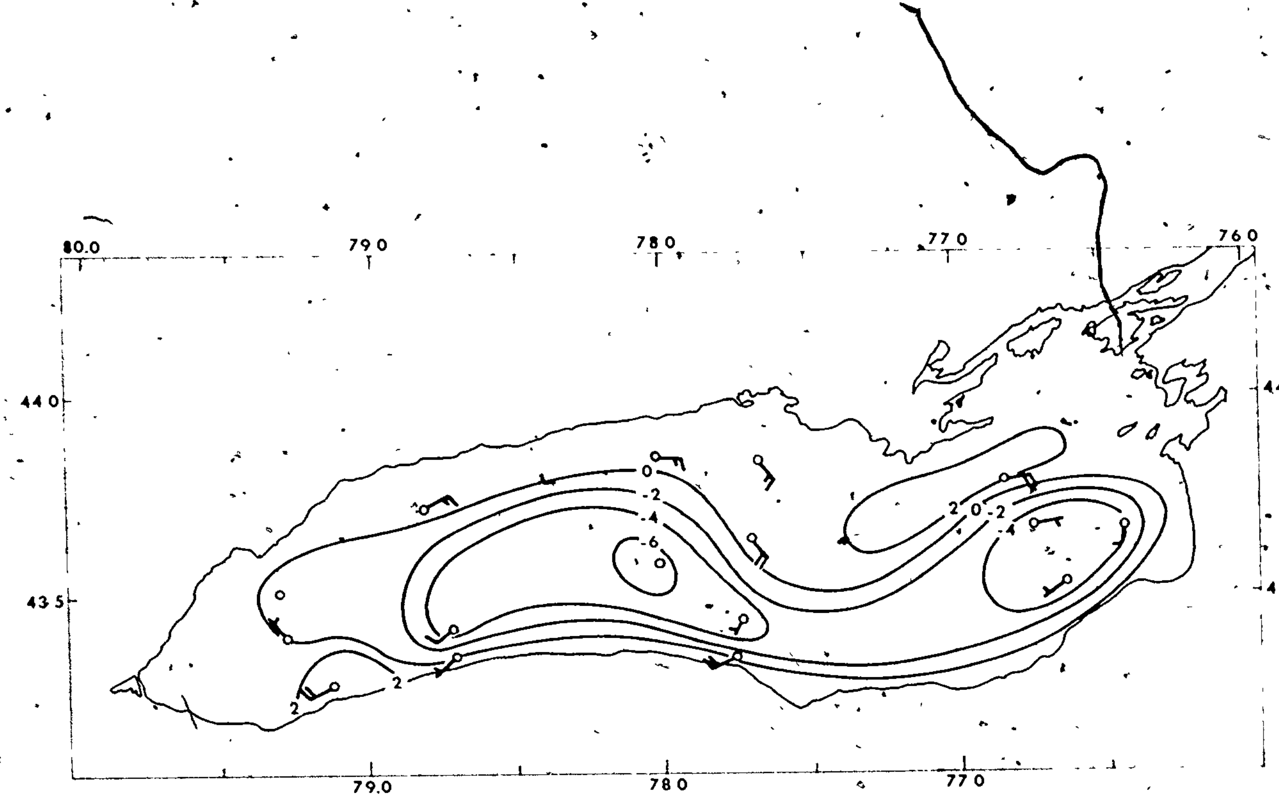


Figure 3. Lake Ontario current (full barb is 1 cm sec^{-1}) at -15 m and dynamic height (mm), July 1972.

The question is, why does the wind not have a bigger effect on the current? If you did not know better, you would say the temperature pattern, generated by heating and wind, produces currents that are essentially in geostrophic equilibrium with no other wind effect at all.

Figure 4 shows a temperature field from my model for July. It is supposed to compare with the first slide. It is not too bad a fit. There may be some weaknesses in it, but I see no big problem; warm water is present on the south shore, the big cold pool is in the center, and upwelling occurs along the northwest shore. The basic features are roughly correct. Almost any model can be tuned to give a pattern that looks more or less like this.

Figure 5 is the vertically averaged flow. This is stream-function in units of $10^8 cm^3 sec^{-1}$. Instead of having the one big gyre that the observed current has, it has a relatively large cyclonic gyre and also a smaller anticyclonic gyre in the northwest.

Figure 6 is a graph of the eastward component of the current. The most glaring error is near the north shore. It shows all the shore water flowing toward the east, and those measurements by Bob Pickett all show the current flowing toward the west. It is not a matter of the observations either because other people have measured current even closer to the north shore at other times of the year, and they also say that the current on the north shore of Lake Ontario is to the west. This is something we fundamentally do not understand. However, I still have hope that all these problems can be ironed out with the IFYGL data. I hope that the modeling expertise we have built up in the last couple of years will eventually explain most of the IFYGL current measurements. This knowledge can be used to design a new field program that will be better.

Another suggestion is that serious thought should be given to running at least one model operationally during any field experiments. This is a suggestion from Joe Simon. His argument is that numerical weather prediction improved when meteorologists had to make a forecast every day. They found out they had blunders which had to be corrected. It is not just a matter of saying "We predict a Kelvin wave over in that cove; see if you can find it." Operational modeling would provide discipline for the modelers and a means for continual testing of the model. After the field work, modeling can still play an integral role in interpretation of the results.

I suggest that all the modeling take place at institutions where data analysis is going on; it is helpful to have modeling work hand-in-hand with analysis of the observations.

I would also suggest that the modeling program should retain versatility. It should not rely on just one numerical model, but it should keep many people involved in modeling and the analytical and numerical models should be used in conjunction with analysis of observations.

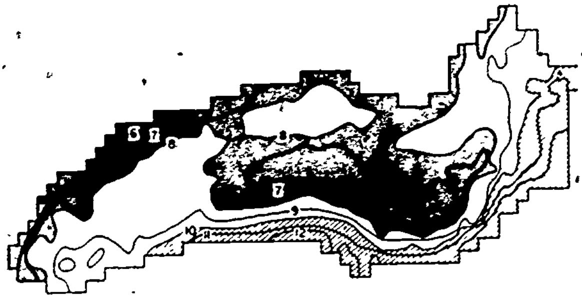


Figure 4. Lake Ontario simulated temperature field, July 19

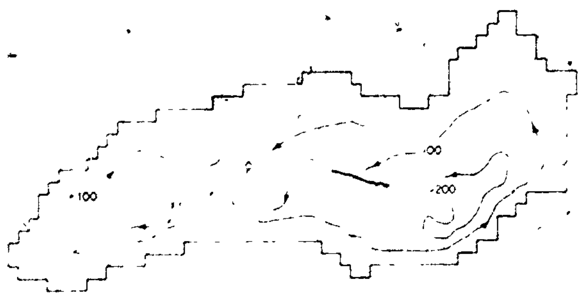


Figure 5. Lake Ontario vertically averaged flow, July 1972.

Figure 6. Eastward component of Lake Ontario current, July 1972.

1.2.1 Response - D. B. Rao

John Bennett has preempted me on practically everything I wanted to say. I basically have to agree with everything he said.

In terms of modeling, the Great Lakes community could conveniently use mathematical techniques derived from numerical meteorologists over the last three decades instead of expending efforts on developing new techniques.

When we look at numerical models of the Lakes (John Bennett has described seven models or so), they consist basically of two categories: the barotropic homogeneous numerical lake models and the baroclinic numerical models where baroclinicity is either at fixed levels or between movable interfaces. The barotropic models have been successfully used over short-time scales in hindcasting studies of storm surges primarily on the Great Lakes by George Platzman. They have been fairly successful in reproducing what is observed, and perhaps one might say they are ready for operational use. On long time scales, barotropic models can be used to look at seasonal circulation patterns and steady-state dynamics.

The two most elaborate baroclinic-multilevel models are those of Joe Simons of CCIW and John Bennett here. They have been integrated over long time periods. Perhaps one can start using them operationally, although I guess you cannot put forth the information for public use like a weather forecast.

The models can be run with the idea of understanding. First of all, how close does the model simulate phenomena that have been found in observations? Or maybe new phenomena can be discovered. These applications should be looked at in terms of analyzing the dynamics of the models, rather than comparing with observations to see how faithfully currents and temperatures at a given point and time can be reproduced. Even though the numerical model may not exactly reproduce some observed features, it still gives information for analyzing processes in the lake. Examples are coastal upwellings, nearshore processes, or generation of internal waves. Also, things like the importance of nonlinear dynamical processes and interaction between coastal and interior zones of the lake can be understood.

The coastal zone is important, of course, from both the biological as well as the waste dispersal point of view. Wastes are injected into the coastal areas, and those areas have to be modeled fairly well. Large-scale numerical models might give some information on how strong the interaction is between the coastal zone and open lake.

As Dr. Aubert said, "How does one go about developing limited-area numerical models which probably do not exist at the moment?" In meteorology there are limited-area models which use large-scale model information. These techniques are available.

Finally, I feel a government institution like GLERL or perhaps CCIW cannot only run models on an operational basis, but can see to it that, when these models are put together, they are sufficiently general so that anybody can make specific experiments by getting access to these models. This is what a general circulation model is supposed to be.

1.2.2 Response - R. Pickett

I want to comment on a point John Bennett made about using a numerical model in essentially an operational mode in a son-of-IFYGL experiment. Predictions would be made and checked during the field program. One of the problems in the past was that several years were required for data processing. If one looks over the lake-scale studies that Prof. Mortimer mentioned, in each case somewhere in the range of 4 to 6 years were required before the data were put in a useable form and before a data report was produced. Prof. Mortimer also mentioned that data from studies of Huron and Superior were never published. So, at least to my knowledge, these studies are still unedited recordings on tape somewhere.

John Bennett also cited the IFYGL data. This is 1974 and those data were taken in 1972. We are just now reaching the stage where we are integrating and editing both countries' data and analyzing the results. That seems to me to be too long. Certainly if future field operations are planned, more thought should be put into how the data can be made available in a shorter time period.

What are some of the things that can be done? Certainly, we can take only the data we need. For example, we took 6- and 10-minute observations in IFYGL and calculated hourly averages. I think with present technology hourly averages can be calculated at the transducers and just the results transmitted. We should be able to get the computer in the process sooner. We could then do some high-speed editing so that observations are immediately verified or thrown out. We could display the data over the whole lake in the manner of the illustrations by John Bennett. They were done by using a computer-coupled cathode ray tube. We could also, if we stage another field year, test our analysis and editing procedures before the field work is begun to make sure they are tuned up and working well. That way we would not go through a year of development to come up with procedures to handle the data after the field work.

Finally, there seems to be a trend to put most of the effort into field work; then people and resources drift away so that few are left to crank through the long-term processing. I think we would do well in planning future field work to keep enough effort in data processing to squeeze it down to the shortest time period. Until we do, there is no way of getting the kind of feedback we need. As we bump into questionable data now in IFYGL, we cannot find out what happened. Was the current increasing or was the sensor drifting at this particular level? What is the most probable explanation? The field people have forgotten or gone.

Someone once said, "Data is like fresh meat--it spoils very quickly." We have to compress the period of years that it now takes to process data. Otherwise we will never be able to use numerical models in anything approaching operational modes.

1.3 DISCUSSION

Aubert. We are running a little ahead of schedule. Even though discussion was not planned in this plenary session, we could entertain discussion for about 10 minutes. Are there comments on this plenary session up to this point? Do I hear any controversial topics?

Holland. There were two contradictory recommendations made, I think. There was the cost of this real-time business. It would be large, but telemetry is necessary. Prof. Mortimer suggested that telemetry was unnecessary in IFYGL because there was no real-time use made of the data. This is, in essence, as I understand his comments.

Mortimer. In the water movement program, yes.

Holland. If one were to go into real-time prediction, of course, telemetry would be essential. Also the preparation of the system would be essential, the shaking down of all software, all data processing, editing, and analysis. Despite automatic procedures, the staffing would be heavy. During the course of the thing, you would have to have a team handling the data, so the cost would certainly peak during the operations phase. The cost of data processing and analysis would peak very heavily during the operations itself, rather than being distributed over time as they are in IFYGL. I think some of these things are essential in order to assure the success of the field program itself. Part of the problem with the IFYGL data is that we did not have test data and processing procedures in advance of the program and had to develop these after the observations were taken. Then we face these unknowns. We find that we do not have enough information to know exactly what we are doing. There is considerable merit in the suggestion, just in the interest of guaranteeing a successful data collection effort. But I think it has to be understood that it is very costly and it introduces a lot of technological uncertainties. Maintenance is an example. A 30-hour or a 30-day backup recorder cannot be relied on to serve this purpose. If the communications go out, you have had it, so more reliability is needed. Faster trouble shooting is needed and it gets to be a much more expensive project.

Bennett. When I suggested that a model be run operationally, I did not imply that all the observations would be real-time. One can run a small-scale numerical model real-time or he can hindcast. Every weekend you could run the previous week with the observed winds, keep track of your prediction model through the field year, and get verification data anywhere you wanted to whenever you had an opportunity to keep the model in tune. It is a long way between that and real-time collection and processing of data.

Holland. J. Bennett is probably right. The real-time system may be quite expensive and not worthwhile, but there could be a lot of data collected within a week or so. If the model were in one big lab like this, the data could be used to tune the model on an unofficial basis.

Aubert. Does anybody else have comments on this point? J. Holland mentioned several points which in IFYGL added to this 2-year lag between observation time and data availability which R. Pickett referred to. That is, the data processing procedures were not available, developed, and operational prior to the start of the field year. Part of the reason for this was that the equipment was not fully developed and tested before the operational period started. No developmental data base existed on which to develop the data processing procedures. The development and testing of data acquisition systems take a lot of lead time and this did not exist for some of the data systems. The systems did not exist at the time they were needed for an orderly development. This resulted in the lag, and the data processing procedures were developed after the fact.

J. Bennett, you mentioned the desirability of more intensive testing. I guess you are adding the dimension of testing on a routine basis. By routine operations, you do not necessarily mean that a new integration would be started every day or every 12 hours. Once a week might be enough.

Bennett. Yes, even every 2 weeks could be useful. You would not want too many iterations. It would be an intellectual exercise more than anything. You could send out the model results for comment to the field investigators every month or so and ask whether it agrees with what they saw in the lake or not.

Birchfield. I think that would be particularly useful to view what goes on in a lake. Or it could be looked at from an episodic point of view because, if a large storm goes by, then some data will hopefully be coming in from some aspects of the storm that will reflect its passage over the lake.

Holland. J. Bennett was right. Even if a 2-week time scale is used for simulation, data must be coming in currently, but this would be a less expensive alternative. It certainly costs less to do a 2-week type real-time operation than it would to do a day-by-day real-time operation. But it would still be a sterile exercise. You would run your models every week or two and you would not know whether they had any correspondence to the real world or not. This would not be very interesting. So data must be coming in. It certainly would be possible to design a scaled down system to decide what parameters you want, what averaging time you want, how quickly you have to get them, and how to size the thing to fit your pocketbook. It might be a month instead of a week that you could afford to handle it in a real-time sense. Then you could go out in boats, pick up the tapes and process them, and check your simulations on that kind of time base.

Baer. Could I ask a very simple-minded question? I find all the discussion on lake-scale circulation, assuming it will be a son-of-IFYGL as I heard it called, of equivalent scale, magnitude, and major activities. Are there not things that need to be done that are not so big?

Aubert. Does somebody want to rise to that question?

Mortimer. I think that will be answered by the end of today or tomorrow.

Aubert. Yes. It is not the general point of view that there should be a son-of-IFYGL. If you still have that question at the end of the plenary session, it should be brought up at a workshop. It is to be hoped that it will be answered by later presentations.

Mortimer. I want to make one point is a kind of footnote. I am not against real-time telemetry, and I think some instruments might be designed that way. What I urge is that the instrumentation debate and decisions should sit right in the heart of the scientific program planning. They should not be dealt with by some distant agency in Washington. Success in the end depends on reliable instruments and on seamanship. For example, in IFYGL there was little money to service the Texas Instrument buoys. I believe there were only two small motor boats based in Rochester, N.Y. They could not operate in rough weather; therefore they could not get the propane cylinders onto the buoys unless the weather was calm because of the buoy design. If the buoy broke down, the weather was rough, and the 30-hour backup tape had run out, there was nothing that could be done about it. Of course, Murphy's Law being what it is, the most active episodes often occurred when the most interesting recorders had broken down. There may be justification for real-time telemetry on perhaps a limited number of instruments, but the information cost is much less with self-contained recorders of proven design. Now, having said that, I also say that the instrument contractors deserve considerable praise. Theirs was a new entry to the oceanographic instrumentation field; when they saw the difficulties, they pulled out all the stops to make things work.

Pinsak. I would like to make one point to clarify our perspective. Is the intent to test the model or to test the data? From the trend of discussion, a presumption seems to be that the data are all good as they come in from the observing system. This, of course, is not true. If the data do not fit the model, there would then be a question as to whether to adjust the model or to adjust the data.

Aubert. I think we had better cut off the discussion at this point. The next item on the agenda is Water Movements: Nearshore Circulation. The lead talk will be given by Dr. G. Csanady of Woods Hole Oceanographic Institution, followed by responses from Prof. J. Scott of State University of New York, Albany, Prof. G. E. Birchfield of Northwestern University, and Prof. T. Green of the University of Wisconsin, Madison.

1.4 WATER MOVEMENTS: NEARSHORE CIRCULATION - G. Csanady

In the language of dynamical oceanography, the "coastal boundary layer" (CBL) may be defined as that band of water within which any Ekman drift perpendicular to shore reduces to zero. Given the presence of stratification, frictionless fluid theory shows the width of the CBL to be of the order of the internal radius of deformation, the magnitude of which is in most cases between 5 and 50 km. In the absence of stratification, the width of the CBL may be expected to be determined by frictional effects.

The only serious observational studies of the CBL appear to have been carried out in Lake Ontario in connection with IFYGL. We should note here that the CBL extends much further from shore than the littoral drift zone (LDZ) which has been studied extensively by civil engineers, geologists, and others. Within the LDZ, the momentum of incoming surface waves is rectified by dissipative processes and causes a longshore current. The width of this zone is from wave breaking depth to shore, or typically a few hundred meters. In what follows, we shall be concerned with the bulk of the CBL which lies outside the LDZ.

The IFYGL-related observations have firmly established a qualitative difference between the current regimes of the CBL and those of the deeper, mid-lake region. Within the CBL, observed water movements are mostly shore-parallel and "current-like"; outside the CBL, they vary in direction in a periodic manner, being more nearly "wave-like." The IFYGL observations supplied a detailed description of nearshore currents and leave no doubt about the distinct identity of a CBL. In Lake Ontario, the width of the CBL is of the order of 10 km.

A particularly important consequence of the distinct flow regime in the CBL is that pollutants discharged nearshore remain trapped within it for prolonged periods. This has been often noted in connection with effluent and river plumes which generally turn shore-parallel after discharge and has been documented by specific dye diffusion experiments in Lake Huron.

Linear theoretical models of wind-driven flow in a stratified fluid have yielded the concepts of coastal jet and Kelvin wave. Considering the simplicity of these models, they have been remarkably successful in providing an intellectural framework for the interpretation of CBL observations. I have recently compiled a more detailed review of the achievements of linear dynamics.

An aspect of linear dynamics not completely resolved yet is the precise influence of depth variations on depth-integrated flow or "transport." In shallow water, transport is downwind, while in deep water, return transport occurs--this much is generally agreed upon. However, the precise effects of friction and of the earth's rotation on a flow pattern left over from a wind impulse remain unclear. Although the CBL usually lies well within the downwind leg of the topographically controlled transport gyres, a rotation of this flow pattern, or its rapid spin-down by friction, is an important determinant of CBL behavior.

Some especially interesting nearshore phenomena occur early in the heating season during the so-called thermal bar period. IFYGL data provided much greater detail on the nearshore flow structure than available earlier, and these could be interpreted in terms of dynamical concepts with some conclusiveness.

Given the distinct identity of the CBL, it is a legitimate conceptual model to speak of mass transfer between one black box called "CBL" and another black box called "mid-lake." From a practical point of view, this particular exchange process is of evident importance. Existing evidence shows that under stratified conditions the flushing of the CBL is associated with onshore-offshore movements of a thermal front which is the nearshore upwelled or downwelled end of the seasonal thermocline. The structure and behavior of this front is known in a gross way and qualitatively, but is poorly understood. Figures 7 and 8 show an example of nearshore upwelling and associated coastal jet observed during IFYGL. Linear dynamics indicate large isotherm movements nearshore, but the theory is so far incapable of describing anything but "small" displacements, small that is, compared to equilibrium thermocline depth. A particularly difficult feature of this problem is that the lake bottom slopes away from shore gently, but quite significantly in the sense that the water depth can easily double or triple over the nearshore slanted portion of an upwelled or downwelled thermocline.

In connection with upwelled or downwelled fronts, one would like to be able to answer questions relating to their generation and decay, the factors which determine how far from shore an upwelled front (say) stabilizes, or how long it takes for such an upwelled front to relax to its horizontal equilibrium position. Also, one would like to know the magnitude of the mass exchange between the CBL and mid-lake associated with the development of an upwelling, its local disappearance upon the passage of a wave-like front, or its eventual frictional decay. These questions involve the effects and parameterization of turbulent friction in a strongly statified shear zone, time-dependent inertial adjustment to a state of equilibrium characterized by the presence of an inclined front, and finite displacements of fluid columns over a sloping beach, problems all well outside the scope of existing linear theory.

One considerable contribution the Great Lakes community could make to oceanography would be a thorough documentation and understanding of upwelled thermal fronts. In the Great Lakes, these fronts are not much more than 5 km from shore, and the logistics of their study is comparatively simple, certainly in comparison with oceanic fronts more than 160 km from shore near the eastern seaboard of North America or their counterparts in the southern ocean thousands of kilometers from major oceanographic facilities. The understanding of the generation and maintenance of such fronts is a key outstanding problem in oceanography.

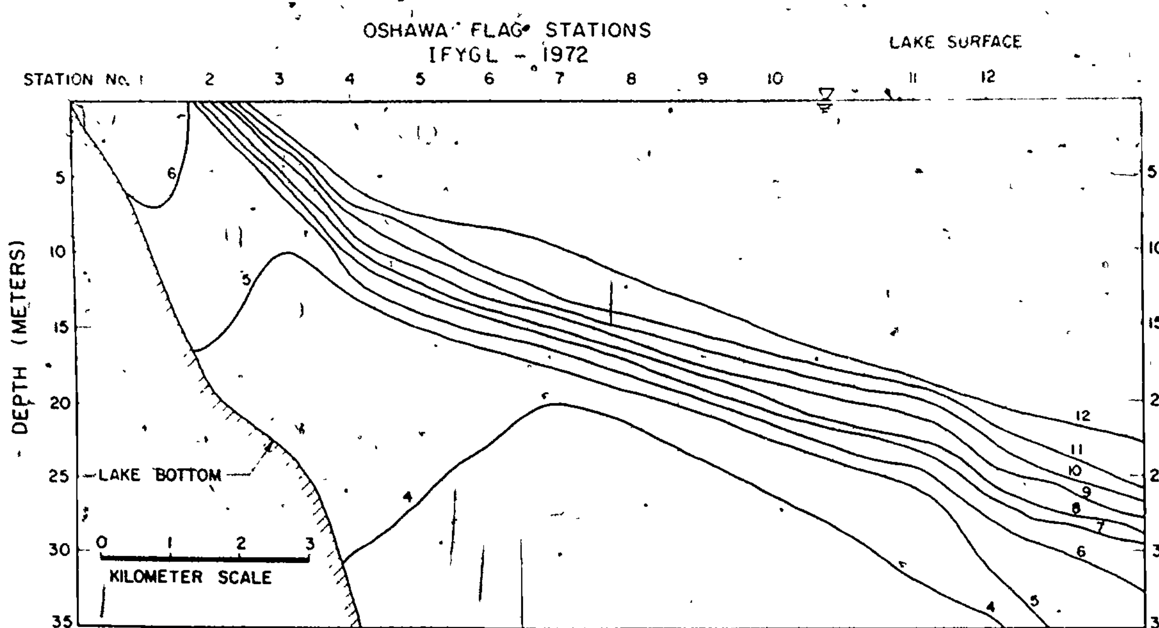
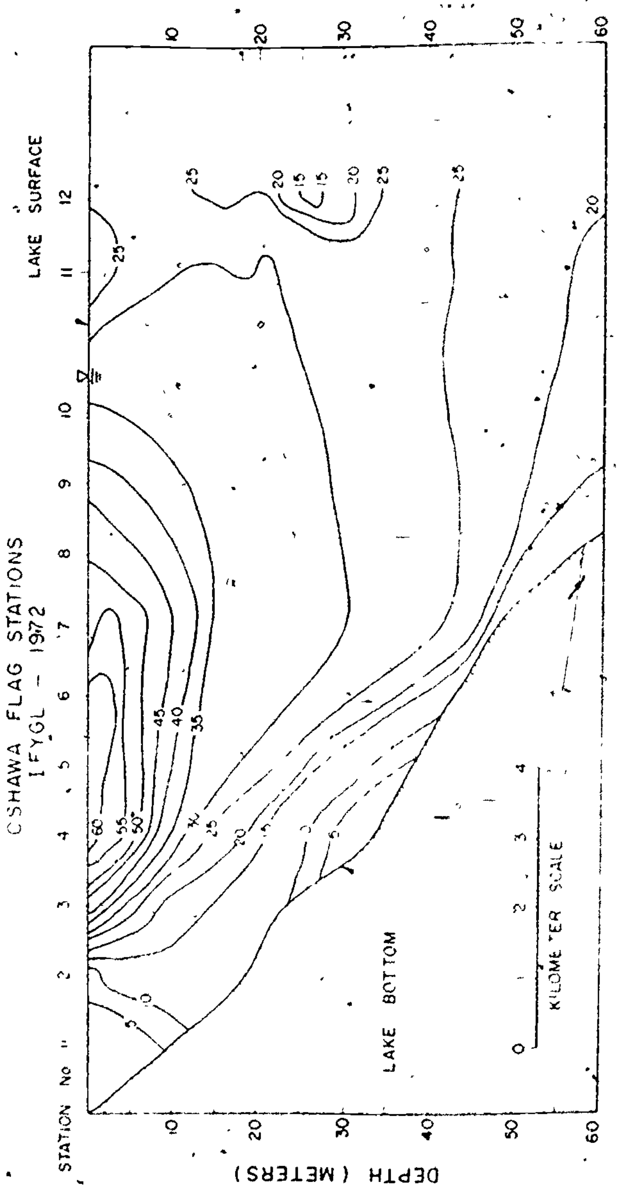


Figure 2.4 Isotherm contours (°C) at Oshawa, Ontario, flag stations, October 10, 1972.



OSHAWA FLAG STATIONS
IFYGL - 1972
LAKE SURFACE
LAKE BOTTOM
KILOMETER SCALE
STATION No.
DEPTH (METERS)

It should also be pointed out that the current state of understanding of turbulent friction in the CBL is unsatisfactory. It is not clear, for example, how far momentum advection by Ekman drift or horizontal momentum transfer by turbulent eddies are dynamically important in the CBL. One's best current guess is that both of these are similar in magnitude, and both are much less important than momentum transfer to the bottom by turbulent friction. Without further detailed studies, one cannot say whether this is so or under what conditions it ceases to be so.

A probably related problem is the explanation of the observed asymmetry of "right-hand" and "left-hand" coastal jets (looking downwind). Observations during IFYGL have shown right-hand jets produced by either westerly or easterly wind impulses to be stronger than left-hand ones. As a corollary, time-averaged flow or lake "circulation" have been observed to be cyclonic. Right now there are four proposed mechanisms on the market purporting to explain this phenomenon, but none is completely convincing. All explanations invoke some aspect of turbulent friction or of nonlinear momentum advection, although in quite different ways. This is a relatively happy situation for planning further research: We know a phenomenon exists and we have some tentative ideas why, the task being to decide between rival theories.

Theoretical studies of nearshore frictional effects have suggested the probable existence of a kind of "secondary flow" in a vertical plane normal to the coast, onshore flow within the top layer being compensated for by offshore flow below, or vice versa. Such secondary flow, superimposed on coastal jets, could turn out to be of great practical importance in connection with pollutant dispersal. However, we have not so far been able to relate such theoretical models very well to observation, mainly because of the relative crudeness of observations, or more specifically, the poor accuracy with which onshore-offshore components of nearshore currents can be determined. Thus, we do not know the magnitude of the parameters that would realistically represent turbulent friction. In at least some of the extant theoretical work, specific assumptions are made regarding the magnitude of the frictional parameters, mostly to the effect that "horizontal" eddy viscosities are quite large. What little we know about this problem in the Great Lakes does not agree with such an assumption. A crude analysis of frictional effects, based on empirical information on friction in a mixed layer, leads one to very different conclusions from what some of the friction dominated theories would predict. What we clearly need is further fundamental knowledge on turbulent friction in the nearshore zone which would enable us to assess the probable importance of the kind of secondary circulation mentioned above.

Another point in connection with the long-term average lake circulation problem already referred to above is that in pollution dispersal problems we are concerned with Lagrangian properties of the flow. In simple terms, how does a given water mass get from Niagara to Toronto, or vice versa. It is not certain that averaged data from fixed current meters tell anything at all about the Lagrangian circulation. No long-term experiments have so far been carried out to determine the relationship of Eulerian and Lagrangian average velocities, and we cannot really predict where a batch of pollutants released nearshore would end up in a few days.

In the design of sewage outfalls or water intakes, it is necessary to model the dispersal of effluents in the nearshore zone. We have some information on diffusion parameters, but these have so far not been related to the specific flow structure of the coastal zone. The strong coastal jets illustrated in figures 7 and 8 above are certain to influence nearshore diffusion in important ways: When one part of a diffusing batch goes faster than another, the batch becomes elongated and its dispersal may be expected to speed up. There is no systematic quantitative information on similar effects.

Further reflection on a number of the above topics leads one to the conclusion that our greatest current need is for fundamental understanding of various key physical processes operating in the coastal zone. The IFYGL observations were designed essentially to elucidate the large-scale, lake-wide response of Lake Ontario to such forcing events as a major storm. The results of these observations have led to a satisfactory understanding of the first-order flow pattern. The next step is not more large-scale observation, but well-focused experiments aimed at such fundamental problems as density fronts and turbulent friction. We need a great deal of thought and depth in our next approach, rather than breadth and extensive coverage.

1.4.1 Response - J. T. Scott

Gabe Csanady mentioned experiments in separate scales, and I completely agree. Future experiments should be designed along the lines of his final remarks. That is, we should concentrate on well-focused experiments on different phenomena.

We do not have all the results in from IFYGL, so we cannot really determine all of the interactions between scales. But I have a feeling that scales are going to be very difficult to separate for certain phenomena. For example, Csanady showed that large-scale features and waves very definitely affected the nearshore circulation. This is the problem in powerplant siting studies where, by law, you have to go out and put up a current meter to obtain a year or so of data. You have no idea what is going on in the rest of the lake. The same sorts of remarks apply to what he said about upwelling and downwelling. These are also to some extent governed by features of the large-scale circulation.

I want to comment on what some of the others have said. A lot of those comments fit together. First, Cliff Mortimer's comments about prior instrument development and reliability testing go back to the Texas Instrument experience. They were late in entering the IFYGL program, and there were difficulties in instrument development. This Laboratory (GLERL) could be the mechanism for early instrument package development. Perhaps GLERL could develop a set of basic instrumentation that could be utilized in different programs much like CCIW does now. CCIW had a lot of experience with their instrument systems before IFYGL, whereas we did not. However, I am not disappointed with some of the data from IFYGL.

Another comment relates to what Bob Pickett and John Bennett said on gyres. I have ideas which I think are slightly different from theirs. My ideas are that large shore-bounded waves migrate cyclonically around the lake. They decay and a new event starts. This results in that mean cyclonic gyre basically because the short-term fluctuations are sometimes larger than the mean.

If people are talking to each other, a lot of these differences can be ironed out; but when we are separated, we all develop our own ideas. Bennett did say that when modelers work together with the data people, better results can be achieved. I agree with him and think that another role of this Laboratory could be that of getting people together like this more often.

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I want to go back to something Clifford Mortimer said when using his table; he gave "pluses" for "nearshore circulation" in column II. Perhaps eventually we will get one in column III when we have more interaction with the modelers. I am surprised he left us out of column I because IFYGL did "reinvent the wheel" in many cases. The nearshore circulation and coastal jets that Csanady predicted in several different theories were rediscovered. In the future we may "rediscover" the long waves that both Mortimer and Csanady have been writing about for many years. Perhaps we put a "tire" on the "wheel" of their earlier work.

Bob Pickett made an interesting point, starting some discussion which will probably continue. It was on the operational aspects of real-time data. This interests me very much. I put myself on the side of "real-time" information so that we can get a quick look at results. That was one of the problems of the Texas Instrument system. They were attempting to build a real-time system in a short amount of development time. We did not achieve the desired result, but as Pickett pointed out, we got the data processing time down from several years to about two. The Canadians did better with the older, more fully tested techniques, but real-time capability has more possibilities for the future. Being able to get the data for early spot analysis can be valuable if only for instrument checking, redesign of experiments, and help in modeling. Another thing this Laboratory can probably do pretty well would be to develop this capability.

My last point relates to what Dave Chandler said on whether to undertake small or big experiments. I lean toward the large cooperative programs though not necessarily as large as IFYGL. I saw many indirect benefits coming out of the IFYGL program, both from a planning point of view and from the point of view of the impact in the scientific community.

1.4.2 Response - G. E. Birchfield

First, I want to make a general comment. It seems to me that a significant event in environmental scientific research has taken place recently. That is the establishment of this Laboratory. It is a relatively unusual event in that it is a scientific Laboratory. It has, as part of its program, scientific objectives. In the present orientation of the Federal Government this should be given all possible encouragement and support.

For a second general comment, I want to refer to history for a brief moment. Prof. Mortimer can perhaps sharpen my numbers. With regard to the IFYGL program, one of the first activities in the environmental area which had similarities with the IFYGL objectives was the voyage of the H.M.S. *Challenger*. That started out in 1872, or about 100 years ago. My reason for mentioning this is that I think the *Challenger* was one ship at sea for 3 or 4 years involved in collecting the first genuine and valuable oceanographic data set. It collected physical, biological, meteorological, and I think, some geological information. I believe it was at least a good 20 years before the final volumes of analysis of the H.M.S. *Challenger's* data were published. In that view, the red line extending to 1977 that Dr. Aubert showed us is for 6 ships and 600 scientists. We have an enormous amount of data here, and we are planning to analyze it in very quick order. I then ask the question, is the digital computer enough to compensate for the difference in time and scale, to cope with the amount of data we have, and to get what is valuable out of the data?

Going to the specific area, I would first like to say a few words, with tongue in cheek, about John Bennett's comments. It seems to me that all our questions should be solved by numerical models, such as Joe Simon's and John Bennett's. They take the full equations of motion without dropping any terms. They put a lot of resolution in the vertical and a lot of resolution in the horizontal, and integrate with good initial conditions. Why is not that the end of the story?

As Bennett pointed out, there seems to be something they do not understand. The purpose of repeating that comment is to point out that one does not have to think of modeling in terms of numerical models in which you throw in everything including the kitchen sink and the scouring pads too. Are we not going to be able to formulate large-scale, semiquantitative models of the major processes that are going on in the lake due to particular kinds of forcing? Or qualitative models? For example, if you look at meteorology, you have synoptic-scale models of the Bjerknes school for fronts. Can we construct a model of the response of a lake to surface waves, to the barotrophic response, and to the currents and thermoclinal response in a qualitative manner that can be understood? Is the problem so difficult that we cannot use a simple qualitative model of the response of a stratified lake to a particular typical wind forcing? I would say we can and that such models would be of value.

We are moving toward developing such semiqualitative models by trying to identify the processes that are important for these models. I would assign these models a name, something like synoptic lake models--synoptic in the sense that it is a qualitative physical model of circulation, including the coastal zone.

Speaking of the coastal zone, I would like to express some really serious reservations about treating the coastal zone as a black box. There are processes that appear to occur in the coastal zone, but that are intimately related to what is going on in the lake as a whole. We really know very little about the exchange processes between the coastal zone and the deep water. The theories of coastal flow that Csanady developed--the baroclinic and barotrophic flows--are, as he said, a first approximation to the response. However, because of the character of the observations we have at our disposal, we are in a rather serious state of ignorance. One could ask, are the coastal flows dynamically stable? Are they baroclinically or barotropically unstable? If they are, what is the growth time of disturbances? If the growth time is sufficient, does one really have an exchange of mass with the coastal zone through such instability? Are these jets or coastal flows stable over a long time compared to the average period in which cyclones go by? If the time scales on coastal currents are long compared to that period, can we guess that exchange processes have only an episodic character when there is a reversal and interruption of the so-called coastal flow?

I guess what I am really saying is that we do not have a very good three-dimensional picture of flow in the coastal zone. We have some good and informative cross sections of the flow. In the field year we had about five scattered around a 3,200-km coastline. To really investigate the coastal zone, which is undeniably very important from the practical point of view, some effort is needed to get a three-dimensional picture of the coastal zone. This is particularly true in regard to mixing or exchange of water between the coastal zone and deep water.

The last thing I want to say is in a somewhat different area that is not appropriate under "coastal water movements"; but, in looking over the agenda, I find no emphasis on other kinds of coastal processes. In particular, I am thinking of the transport of sand in the shore zone. Very closely related to that is erosion of the coast. IFYGL was not really involved in that area of research, and it might be something to consider further.

1.4.3 Response - T. Green

I find myself, in the position of third responder, agreeing with those who agree. Thus, I have relatively little new to say here, but will check off a few points. In the first place, I have essentially no IFYGL experience. You can look upon this as either a fresh opinion or naivete. However, the University of Wisconsin has been studying coastal currents on Lake Superior for a number of years, using ships, aircraft, and moored instruments; I speak from experience gathered there. Primarily I have been, and my bias is toward, providing building blocks for modelers. I also have a definite bias toward small-scale processes. This will certainly show in what I say. In my opinion, available theories have far outstripped the field work; we are really in need of some detailed field experiments, i.e., the dense arrays that Gabe Csanady has been talking about.

Two people have spoken before me about the problems of real-time data transmission. I agree that this presents a problem, but would hate to sacrifice high-frequency current observations because of that. We should preserve them, if at all possible. I also am sympathetic with the problem of getting poor data from the field. To me, the only way to solve that is for the data taker to be the data analyst. Otherwise, people simply are not sufficiently motivated.

I agree that it is important to separate the deep and coastal scales. However, even in the coastal zone, there are various scales. A river plume or a thermal plume has, say, a 1-km scale. In a sense, that subzone is the most important zone because it is closest to the shore. But its scale is different from the 10-km scale that we normally associate with the coastal zone. Separating these scales and their interactions will take a depressingly large amount of work. I think my attention would be concentrated on scales that are even smaller than those of the coastal chain mentioned by others. I am talking about the 200-m to 1-km scale. Here, I think we can and should make direct estimates of transports due to turbulence. When we look at this scale, simultaneity is crucial. This is unfortunate, but I do not see how to get around simultaneity and dense arrays of instruments. I also think that the longshore direction is important; even on a kilometer scale things change so significantly that we probably have to monitor the long-shore changes with as close a spacing as we use with offshore changes.

I do think the question of inshore-offshore exchange is a crucial fluid dynamics question in the coastal zone. I also think it is the most crucial from a practical viewpoint. This is fortunate; it should motivate both the scientists and the policy people to attack the problem. It will not be inexpensive; we will need dense arrays in the coastal zone. Isolines of concentration of instruments should probably be circles centered at a point on shore. We would give some attention to the whole lake, but would concentrate at a point along the shore and decrease our attention outward from that point.

[illegible] intriguing questions regarding the importance of instabilities on coastal zone fronts. [illegible] fronts must be important to the exchange [illegible]. It is hard to believe that a temperature gradient on the order of [illegible] 10 or 50 [illegible] is not important dynamically. Then the question is whether this is a stable or unstable situation, and what is the time scale if it is unstable? [illegible] the importance of nearshore upwelling. Again, [illegible] be [illegible] in detail. I think in all of this, as [illegible] we certainly do not want to reinvent the wheel. Coastal oceanographers are quite a way ahead of us in many respects, and we have a lot to [illegible] experiment off Oregon and from people working in the Straits of [illegible]. I don't think we should be reticent in taking these things over [illegible] the Great Lakes.

I only have one final comment. We seem to be talking about another IFYGL, or whatever it might be called. I think there are three things we can argue about. First, we can argue about the site, second, we can argue about what we will do, and third, we can argue about the organizational framework within which we are going to do it. I think the latter two of these three things will take a good deal of time. I am not convinced that it would take much time to pick a site. I would argue for picking a site relatively early in this process because some of us have current meters, for example, that we can put in that place to start generating time series. If we were to pick a site by next summer, we could start getting some measurements and have some data. Then we could say, when we go to the big program, whether or not the data collected during the program were typical. We would hope that it would be, but we are never sure without a longer time data set at a few points.

1.5 SIMULATION OF AQUATIC ECOLOGY - C. Schelske

Aquatic ecology constitutes such a complex array of scientific disciplines that it may be better to take a slightly different tack than has been taken by some of the previous speakers. I will not attempt to elaborate on a specific group of research objectives now since these should be an outgrowth of the workshop sessions that will follow. To confuse the situation further, worthwhile specific research objectives could be set since there are large gaps in our understanding of ecology in large bodies of water like the Great Lakes. But my approach will be to speak to research problems with respect to the Great Lakes generally and not to specific research objectives.

Differences among the five Great Lakes are sufficiently great in a number of respects that each lake must be considered separately. Specific research problems can be considered for each lake, but our major thrust should be on what questions are appropriate to ask and what problems can be studied. As we all know, formulating experiments and asking questions with testable hypotheses are the difficult parts of this task. Before we can ask questions, design experiments, and make measurements, we must identify the problems with the greatest importance. I have attempted to classify the problems into two main types: experimental and descriptive.

An experimental study is one in which observations are made under at least two conditions so that one condition can be compared to another. Of course, in the strictest definition of experimental work, this is called a controlled experiment. I think it is quite obvious from what has been said earlier today that making certain observations or measurements can be considered in the experimental sense, particularly if we formulated a testable hypothesis to go along with that set of measurements. A descriptive study differs from an experimental one in that these measurements are used to determine environmental conditions in space and time. Both approaches are important. Some of us talk about surveillance and monitoring, and these might be used interchangeably with descriptive studies. I suspect that these experiments are either so complex or so obvious that they are not worth discussing in this group. I would like to mention, however, that experimental approaches, controlled experiments, have been used in Great Lakes research. There are two examples. One is the work that has been conducted during the past few years at the University of Michigan, in the Great Lakes Research Division, on the effects of nutrients on phytoplankton production and species composition. These experiments have been conducted in the laboratory with small beakers and *in situ* with large plastic bags to simulate nutrient enrichment in nature. More recently, Canada Centre for Inland Waters (CCIW) has been working with "limno-corrals," a slightly different type of experimental system. Our bags were suspended in the water, but the sides of the limno-corral extend from the surface of the water down to the bottom. Different types of experiments and hypothesis can be tested with the two types of systems.

The descriptive study is important, if for no other reason, for purposes of assessing water quality. Mentioning descriptive studies, however, produces unfavorable reactions from certain members of the scientific community. The most critical reactions are to the effect that such studies are undertaken because more important scientific questions cannot be formulated. To lessen the impact of such criticism, we need to justify these measurements. They can be justified from a scientific point and also from the point of management. In other words, they are needed so that we can assess what has happened in the environment or so that we can compare present conditions with past conditions and present conditions with future conditions.

We can justify the rationale for descriptive measurements by categorizing them into three groups of variables: causal, integrative, and descriptive. I would like to restrict this part of the discussion, illustrating these variables from the standpoint of eutrophication as it ties in nicely with the water quality work which is a part of this meeting. I have thought quite a bit about this type of rationale, and in addition, it has been considered through an International Joint Commission committee, the Research Advisory Board standing committee on eutrophication, which is chaired by Richard Vollenweider. Two of the members of the committee are here--Fred Lee and Al Beeton.

Causal variables, the first category, are those that stress or force the system when their inputs are increased and produce what is usually referred to as undesirable effects on the system. Phosphorus is undoubtedly the most important variable in this category if we are considering eutrophication. Phosphorus is the principal causal variable in eutrophication processes as its supplies limit the growth of phytoplankton. As a consequence, increases in the inputs stimulate algal growth, producing accelerated eutrophication and some associated changes in the system which may be categorized under integrating variables. We need to know more about other causal factors or variables in the system and their effects on eutrophication even though their importance may seem secondary. Heat, trace elements, organic materials of various types and with various functions, and even conservative elements may play this secondary role. There is little evidence from existing work to evaluate the relative importance of each of these.

Integrating variables simply are those which change in a predictable manner as a result of increased inputs of a causal variable. I would like to mention only two integrating variables, silica depletion and oxygen depletion, to show how they can be used in extremely different parts of the Great Lakes. One of the reasons I am here today is due to an integrating variable, and as most of you know, that integrating variable is silica depletion in Lake Michigan. Increased phosphorus inputs to Lake Michigan have stimulated the growth and increased the standing crop of diatoms, which, in turn, have depleted silica supplies in the lake. In this case then, the decrease in silica concentration is a very good measure of eutrophication

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because it reflects the inputs of phosphorus, the causal factor in the eutrophication process. I might add that, even though some of this work could have been accomplished through descriptive studies, a large part of the understanding of this particular process has come through experiments conducted in large plastic bags.

It is perhaps important to note, for the purposes of the meeting, that the success we at the University of Michigan had with this particular research program and other related programs was due to several factors. These factors included an organization dedicated to Great Lakes research, ship facilities to get on the lakes, and a staff of scientists and technicians with knowledge and capabilities that could be applied to specific problems. We were able to carry out these studies because Professor David Chandler and other people at the University of Michigan dedicated a considerable amount of effort to providing facilities and a critical mix of people. Gene Stoermer was available. He knew how diatoms behaved in the environment and, more importantly, could recognize a diatom when he saw it. Sometimes ecologists have a little difficulty recognizing specific organisms, and we tend to view them as black boxes. The important points are that we had a technical staff that may or may not be available at some organizations and that a lot of expertise is needed to conduct research. It may or may not be necessary to have a large organization to do this research, but it is essential to get a critical mass of people together. In recent years, particularly in oceanographic work, large programs have been conducted successfully either by cooperation among institutions or individuals, so I am not advocating building up large research organizations. There is, however, a definite need for large research organizations or research groups, and these will especially be needed if we are to attack the types of problems being discussed here today.

In addition to silica depletion, oxygen depletion is an integrating variable. The best example of this is hypolimnetic oxygen depletion in the central basin of Lake Erie. The importance of this integrating variable has been documented by scientists at CCIW and through a joint Environmental Protection Agency (EPA)-CCIW study known as Project HYPO. The causal variable in this case was also increased inputs of phosphorus as oxygen depletion is due to the secondary effects of the growth of phytoplankton or to the decomposition of organic matter. Silica depletion occurs in the photic zone in the upper layers of the lake. Oxygen depletion occurs in the hypolimnion, the bottom of the lake, as organic matter decomposes. Measurement of silica serves no purpose as an integrating variable in Lake Erie since, among other reasons, many of the phytoplankton of Lake Erie are not diatoms. Likewise, oxygen depletion has little use as an integrating variable in Lake Superior where very little oxygen is consumed due to the low productivity, but silica depletion would be very appropriate because most of the phytoplankton are diatoms. Integrating variables, therefore, will vary from lake to lake.

The final category is descriptive variables. These would include those that can be justified for such ecological reasons as describing seasonal changes, comparing lakes, and determining long-term changes in the system. Variables under this category would include measurements of chlorophyll, phosphorus, nitrogen, and carbon and abundance of organisms, including phytoplankton, bacteria, zooplankton, benthos, and fish. A number of studies could be cited to justify the importance of studies on the taxonomy and ecology of organisms in the Great Lakes. Probably the best known from the standpoint of management would be fishery studies.

In addition to determining what variables to include under the descriptive category, another major problem is determining the frequency and extent of sampling needed for these particular variables. How many samples, at how many stations, at how many depths, and at what time interval are important questions in our current research and data gathering efforts. It is obvious that the frequency of sampling will vary with the variable. Bacteria, phytoplankton, and zooplankton with relatively short generation times must be sampled frequently--weekly, daily, or even hourly--to describe their dynamic properties. At the other extreme are conservative parameters, or variables like chloride, which have changed on a lake-wide basis in the past 60 to 100 years. But these changes may be too small to be measured on a yearly basis, so a few measurements per year may be sufficient for chloride. The sampling scheme then cannot be uniform for all descriptive variables and has to be varied according to the objective of the particular study.

No ecologist could make a presentation like this without stressing the importance of studying interrelationships in the aquatic ecosystem.

Even though the effects of eutrophication are most obvious in the primary producers and in certain chemical parameters, at least as I presented them here, studies of eutrophication cannot be restricted to the effects of nutrient additions on phytoplankton. Studies are needed to determine the changes in the system resulting from the initial perturbations at the phytoplankton-chemical level and their resultant effects on the food chain. Since all processes in the system are interdependent, one needs to know which processes affect eutrophication.

I will conclude by reflecting on what I think we have learned from IFYGL. Some of this has already been covered by other people. First of all, there is an obvious need for advanced planning and lead time. Funding for contracts and grants should start at least 6 months to 1 year prior to the initiation of the field-year research effort so that people would have adequate time to perfect and test models. Given this amount of lead time, it would be easier to adjust schedules for logistic support if there were unforeseen delays in the scientific program.

There is an obvious need to assemble a critical mass of investigators with appropriate expertise. For large programs, this means identification of individuals or groups with such expertise and the need for coordination of efforts. Such expertise must be available when we need it. Particularly in the university community, we cannot support large research staffs on university budgets, so advanced planning or continuing support is needed if large-scale university participation is desired.

We need to recognize that the Great Lakes differ from one another. This is probably obvious, but it is important because it may be feasible to undertake some research problems on some lakes and not on other lakes. As an example, it may be more appropriate to study the materials balance in Lake Michigan than in Lake Ontario because most of the tributary inputs are quite small compared to the input and the output from the Niagara and St. Lawrence Rivers. Certainly the success or failure of a project undertaken on Lake Ontario may not be the same if undertaken on another lake.

Finally, a need exists to refine and improve the advisory process. In IFYGL a lot of the work was done through this mechanism, and those advisory groups did a commendable job. On the other hand, I think we should always look for improvements and for other mechanisms. My suggestion will be slightly controversial. We need to consider new approaches to managing research, so I would like to propose an alternate or additional system from the advisory standpoint. The reasons for proposing such a system include minimizing the chances of undertaking trivial problems and maximizing the benefits obtained from funding. We all realize that limits on funding will always be a limiting factor. Traditionally, science has progressed through the work of individuals. A dichotomy developed in this approach about World War II, when people began to talk about big science and little science. Ecologists have thought about big science, but probably have not made as much progress as desirable in this particular area. The usual approach to big science has been to take a lot of little science and put it together. By doing that we may not consider very important questions. An alternative to this approach might be to utilize individual researchers in a slightly different way. Traditionally, a scientist has always been someone who could gather and synthesize his own data. That becomes very expensive, especially on large lake problems. An alternative would be to put individuals to work on different tasks. These tasks would have individuals working on planning experiments and evaluating data. It would still allow them to participate in the overall scientific program. This would result in much better designed experiments than we have now. People would be thinking about questions to answer, rather than the data they can collect. In the long run, with proper incentives, the individual probably would get more satisfaction out of this particular approach than by working alone on a complex problem with inadequate resources. This approach would not eliminate the individual researcher as there are many worthwhile problems that can be pursued by individuals. These advisory groups would have to determine which problems could be undertaken by individuals and which would have to be undertaken by larger groups.

1.5.1 Response - A. M. Beeton

Much of the work in ecology on the Great Lakes is still in the descriptive phase. We really need a lot more imagination than we have had in the past, especially if we, as biologists, are going to take advantage of the kind of work being done by the physical limnologists and the people involved with modeling. So, for example, if we had an understanding of what is going on under 1 m^2 of the open lake in terms of biological interactions, we would then be in a much better position to interpret much of the data that have been collected in a number of large, lake-wide surveys. Lake-wide surveys have a very long history. Several were conducted on Lake Erie in the 1920's and early 1930's and on Lake Michigan in the 1930's. Various surveys have been conducted within the past 20 years, and as a consequence, there are reams of data waiting to be interpreted, but we cannot interpret these data because we do not understand some of the basic mechanisms and interactions.

Looking at some things that we could perhaps paint with a broad brush, I am intrigued by the differences in the inshore and offshore conditions in biology and chemistry. I think this is an area in which we can provide a lot of data that will fit in very well with some of the things Gabe Csanady was mentioning this morning. To get a handle on water quality in the inshore area, we must understand something about the exchange rates between the large mass of relatively high-quality water that sits in the middle of most of these lakes and the water in the inshore areas. It appears that the quality of the water in the inshore area is determined by point source discharges as well as diffuse sources and by sediment water interactions, biological activity, and mixing of inshore and offshore waters. We do not know how much phosphorus might be tied up in the clay minerals that are redistributed by every storm that comes along or how much phosphorus may be removed by the organisms and just how much of this is dispersed into the lake by exchange processes between inshore and offshore waters. Each of these factors needs to be considered, but if we did have reasonable estimates of exchange rates, then we might be in a position to start getting a handle on the role of biota in removing the nutrients. For example, if you look at some of the conservative properties, such as chloride and sodium, and compare their distribution with some of the nutrients, you find that, while there are relatively high levels of some of the conservative properties coming into the nearshore water, they are dispersed rather rapidly and concentrations do not differ greatly within one lake. When you look at the major nutrients, concentrations in the nearshore water are often 10 times what you find in the offshore waters; we cannot explain the lower levels of some of these out in the open lake and higher levels inshore just due to dilution alone. The loading rate, the biota, and some other mechanisms are involved in removing the nutrients as well as recycling those inshore and therefore keeping much higher concentrations inshore. We do not understand what these mechanisms are. I think this is very important if we are going to be able to contribute in a meaningful way as biologists to an understanding of the Lakes. This certainly fits in with our concept of eutrophication. We have more than adequate evidence of changes in the Great Lakes. This has been documented extensively. We know that eutrophication has progressed from the shore lakeward; this is logical because point sources are along the shore. Certainly we have plenty of evidence that eutrophication progressed from west to east and from shore lakeward in Lake Erie, and this is what is happening in Lake Michigan. Our conceptual model has been that inshore-offshore differences are pronounced, and we have sometimes talked of Lake Michigan as if it were like a doughnut where you have the inshore waters with somewhat degraded water quality and a big mass of higher quality water out in the central lake. That is all right for a conceptual model, but actually what we probably have are point source inputs from a number of metropolitan areas where the water is probably degraded. We really need to understand the loading rates in these areas and the rate at which these inputs can be dispersed. This is very important from the biological and water quality standpoint and fits in with the physical limnology kind of thing that we need to tie together.

1.6 SIMULATION OF WATER QUALITY--Carl Chen

Just a week ago, our group completed a survey of water quality modeling as it applies to the Great Lakes. The work was done for the Corps of Engineers. Our goal was to define the various methodologies that have been developed to evaluate the effectiveness of various waste water management problems. In this study we reviewed several methods of determining water movement and transport, including direct measurement, scale models, steady-state models, and time-dependent models. We then evaluated the state-of-the-art of water quality models. Some of them are designed for long-term projection of various quality parameters, like salinity and phosphorus, and some of them for waste heat. We also evaluated a whole slew of water quality simulation models that have been developed and may be applied with some minor adjustments to the Great Lakes problems.

Based on this review, we concluded that water quality models have been advanced greatly. There is no need for people to reinvent the wheel since, with modification of some sort, one can apply it specifically to various lakes. As has also been pointed out by others, each lake is different. The approach to be taken for each lake might be a little bit different, but the underlying conceptual framework is strong enough to make it transferable.

When we talk about water quality models, we cannot talk about water quality alone because the water quality is influenced by biology. Right now, the water-quality modeling technology has been advanced from the traditional biochemical oxygen demand/dissolved oxygen relation type of analysis to include more and more biological parameters. This is important.

Equally important, we cannot talk about water quality models without hydrodynamic transport models. The water quality models always require a transport model to drive them. A transport model is the prime mechanism to move the materials and distribute them in space where they influence the biota and the biota in turn influence the water quality.

I would like to talk about some of the basic concepts in water quality modeling and the approaches being taken to date and will present some questions which must be answered to improve the models. I would like to throw out some ideas on how we might model a Great Lake, what kind of transport mechanism we might need, and what kind of biological information we would like to have. The purpose is to simulate discussion and receive input from the audience.

Figure 9 represents a simplified conception of the interactions which bear on the water quality of an ecosystem. The figure shows many simultaneous interactions between biotic and abiotic entities of an ecosystem. Nutrients simulate the growth of phytoplankton, phytoplankton consume nutrients, and so on. Basically there are two major types of interactions or processes. The upper half of figure 10 lists the physical processes taking place to influence the distribution of pollutants. Physical processes include advection,

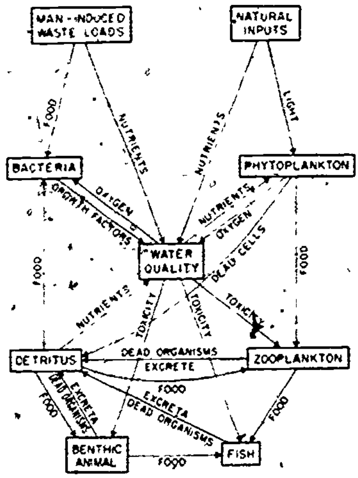


Figure 9. Definition of an aquatic ecosystem.

1. Physical Processes
   a. Advection between segments
   b. Diffusion between segments
   c. Sedimentation from the segment
   d. External input to the segment
   e. Output to external from the segment
   f. Reaeration
   g. Solar insolation
2. Biochemical transformation, uptake, and release associated with the following:

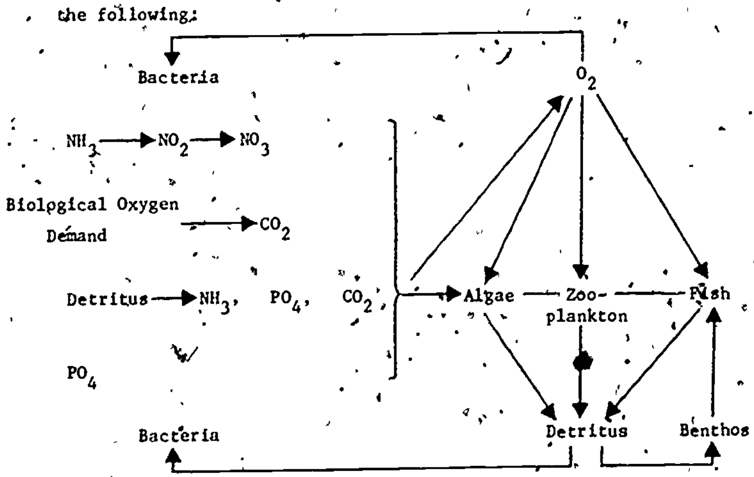


Figure 10. Important ecological processes for modeling.

diffusion, sedimentation, input (namely waste input or atmospheric input), and output. The other types of processes are chemically or biochemically mediated transformation, uptake and release of nutrients, bacterial degradation, etc.

While the approach may be similar, models can differ in the amount of the biosphere incorporated into a model to describe adequately a water quality problem. There have been models taken up to the zooplankton level in the biota that exclude the computation of oxygen. A problem occurs here, however, because, if you do not know oxygen, you do not know if bacteria are going to be aerobic or anaerobic. The problem is how to increase the parameters such that we can correctly do the biology and the water quality simultaneously. We like to have a comprehensive but tractable model.

How do we learn enough about processes to do basic modeling? The first thing is to go to the laboratory and learn something about what is going on. If we want to study algae, we put algae in a beaker. If we want to study a chemical interaction, we measure what comes in and what goes out. Through that, we develop two basic principles. The first principle is the conservation of mass law; i.e., mass has to be conserved. It might transform from one form to another, but mass has to be conserved. The second principle is the kinetic principle; i.e., when transformations occur, they do so at a certain rate. We like to know how fast algae is growing. How many nutrients are consumed from water to conserve mass?

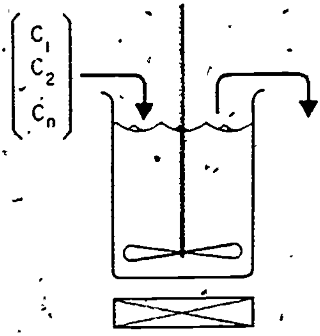
To apply such principles to prototype simulation, the water body has to be divided into small hydraulic elements such that each one of these hydraulic elements can be approximated by the laboratory condition (fig. 11). The natural aquatic ecosystem can thus be viewed as a series of interconnected hydraulic elements. Water and mass can be transferred from one element to another.

Based on kinetic and mass conservation principles, it is a classical situation to write a so-called mass balance equation. The equation says how fast mass in the element is changing due to physical and chemical processes. The following equations can be solved using digital computers:

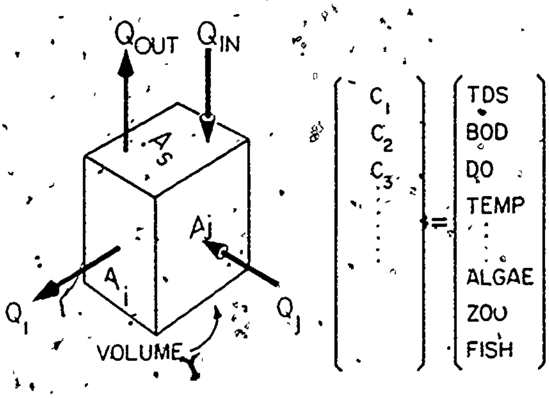
1. <u>General Mass Balance Equation for Abiotic Substances</u>

$$\frac{d\bar{V}C_1}{dt} = \underbrace{\sum_{i=1}^{n} Q_{i1}C_1}_{\text{ADVECTION}} + \underbrace{\sum_{i=1}^{n} E_i A_i \frac{dC_1}{dx_i}}_{\text{DIFFUSION}} + \underbrace{\Sigma Q_{in} C_{in}}_{\text{INPUT}} - \underbrace{\Sigma Q_{out} C_1}_{\text{OUTPUT}} \pm \underbrace{S_1 \bar{V} C_1}_{\text{SETTLING}} \pm \underbrace{K_r A_s (C_1 - C_1^{*})}_{\text{REAERATION}} - \underbrace{K_{d,1} \bar{V} C_1}_{\text{DECAY}}$$

$$\pm \underbrace{K_{d,2} \bar{V} C_2}_{\substack{\text{TRANSFORMATION} \\ NH_3\ NO_2\ NO_3}} \pm \underbrace{\Sigma b_3 \bar{V} C_3 F_{3,1}}_{\substack{\text{UPTAKE} \\ \text{BYPRODUCT}}} \pm \underbrace{\Sigma R \bar{V} C_3 F_{3,1}}_{\substack{\text{RESPIRATION} \\ \text{RELEASE}}}$$



A. a continuously stirred tank reactor, CSTR



B. an idealized hydraulic element

C = Concentration of some constituent

Q = Flow through a face (Q_i, Q_j), out (Q_{out}), or in (Q_{in})

A = Cross sectional area of face i (A_i) or j (A_j)

A_s = Surface area

TDS = Total dissolved solids

BOD = Biological oxygen demand

DO = Dissolved oxygen

Figure 11. An idealized hydraulic element versus a laboratory-stirred tank reactor.

2. General Mass Balance Equation for Biota

$$\frac{d\bar{V}C_1}{dt} = \sum_{i=1}^{n} Q_i C_1 + \sum_{i=1}^{n} E_i A_i \frac{dC_1}{dx_i} + \Sigma Q_{in} C_{in} - \Sigma Q_{out} C_1 + (\underbrace{\mu_1}_{\text{GROWTH}} - \underbrace{R_1}_{\text{RESPIRATION}} - \underbrace{S_1}_{\text{SETTLE}} - \underbrace{M_1}_{\text{DEATH}})\bar{V}C_1 - \underbrace{\mu_2 \bar{V} C_2 F_{1,1}}_{\text{GRAZING}}$$

where

$\bar{V}$ = Volume

C_1 = Concentration of constituent 1

Q = Flow through a face $i(Q_i)$, in (Q_{in}), or out (Q_{out})

E_i = Diffusion coefficients

A_i = Cross sectional area of face i

dC_1/dx_i = Concentration gradient of C_1

C_{in} = Concentration of C_1 in the inflow

S_1 = Settling rate of C_1

K_r = Reaeration coefficients

A_s = Surface area

C_1^* = Saturation concentration of C_1

$K_{d,1}$ = Decay coefficient of C_1

$K_{d,2}$ = Decay coefficient C_2

μ_3 = Growth rate of biota C_3

$F_{3,1}$ = Conversion factor between C_1 and C_3

R = Respiration rate of biota C_3

μ_1 = Specific growth rate of C_1

R_1 = Respiration rate of C_1

M_1 = Mortality rate of C_1

μ_2 = Specific growth rate of higher trophic species C_2

$F_{2,1}$ = Conversion factor between C_1 and C_2.

The following figures show different approaches to segmenting different kinds of water bodies. Figure 12 is the way that has traditionally been used to segment a river. Figure 13 is a way to represent a small reservoir. The small reservoir is usually upstream of a river. It does not receive much waste input. All the water quality influence is in the vertical direction due to thermal stratification and overturn. The reservoir is therefore cut into horizontal slices. Some river-run type of reservoirs can be cut into reaches and then into horizontal segments (fig. 14).

How does one go about segmenting the Great Lakes? One approach was conceived by Canale[2]. He recognizes that a lake has to be divided into a littoral zone and a central zone as shown in figures 15 and 16. This might be too coarse, but the concept is good. Based on the concept, we can envision what to do with another lake. I will use Lake Erie as an example. We envision that a segmentation shown in figure 17 might be appropriate. Thus, a more detailed spatial resolution is possible at the nearshore zone where the lakes receive waste water input. By that, we can see the pollution effect. When it comes to the central lake, the horizontal spatial variation is not as big. We can use a bigger hydraulic element. The advantage of this type of segmentation is that it can fit into the currently available computer core space and also that the computer time is not excessive.

There are also different philosophies in the development of water quality models. Some of the modeling technology has been advanced by starting from a simple one-reactor representation and progressing to greater detail. When one discovers a single reactor representation is not enough, he begins to cut the reservoir into two layers, and when two layers are not enough, more, and so on. That is one way to do it. Another way is to go through the literature to determine the current status of modeling. The model is initially conceived as comprehensively as possible. After a comprehensive model is developed, one begins to simplify the model to see how far he can go. I will not say which way is better, but these are two approaches. One starts from comprehensive to simple. The other starts simple, then discovers that it cannot do the job, and evolves into something complicated. Eventually both approaches may merge at the middle. Meanwhile, those taking the comprehensive route may be accused of being too ambitious or just plain unrealistic. The other group, on the other hand, may make a bad reputation for the modeling field. They build models too simple to be real.

2 Professor Raymond Canale, Sea Grant Program, University of Michigan.

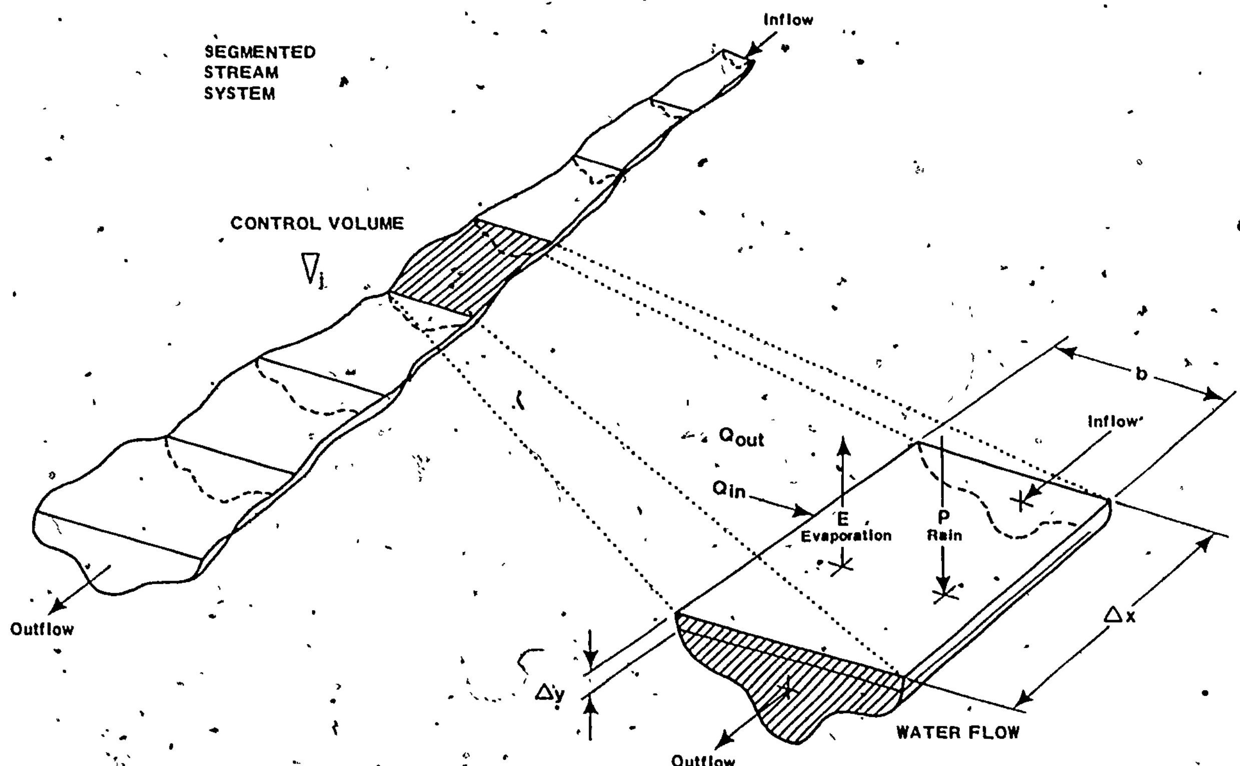


Figure 12. Physical representation of a stream.

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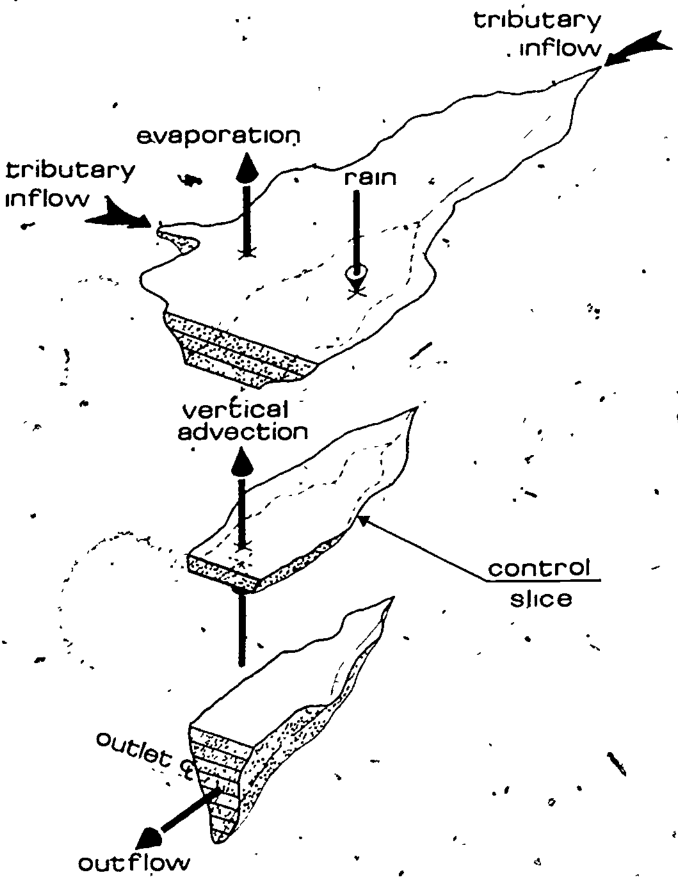

Figure 13. Geometrical representation of a reservoir.

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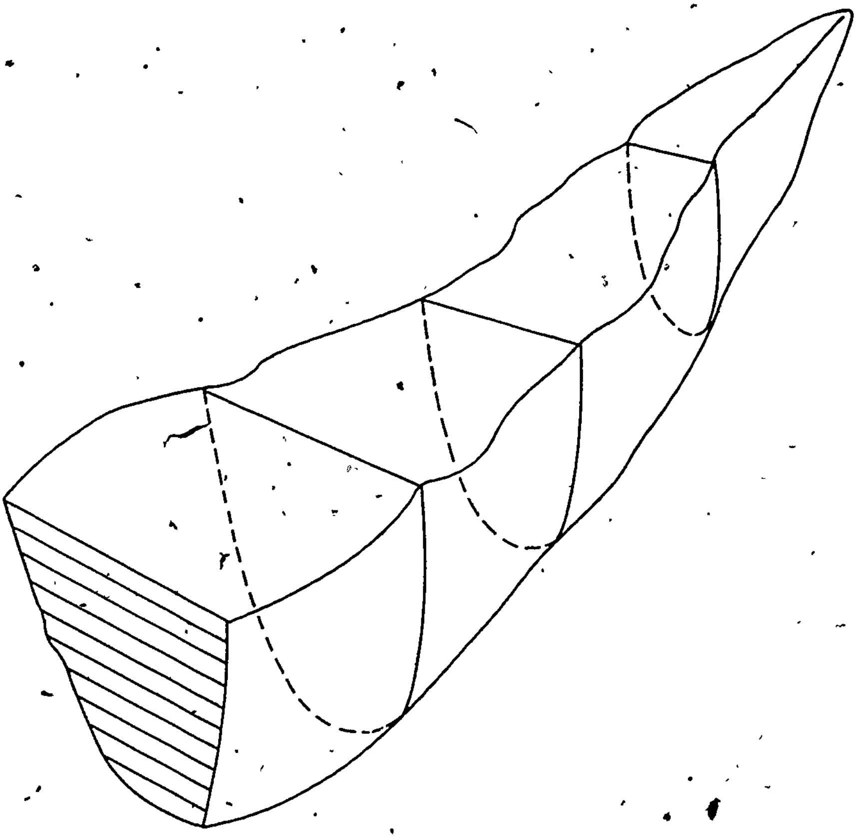

Figure 14. Segmentation for river run reservoir.

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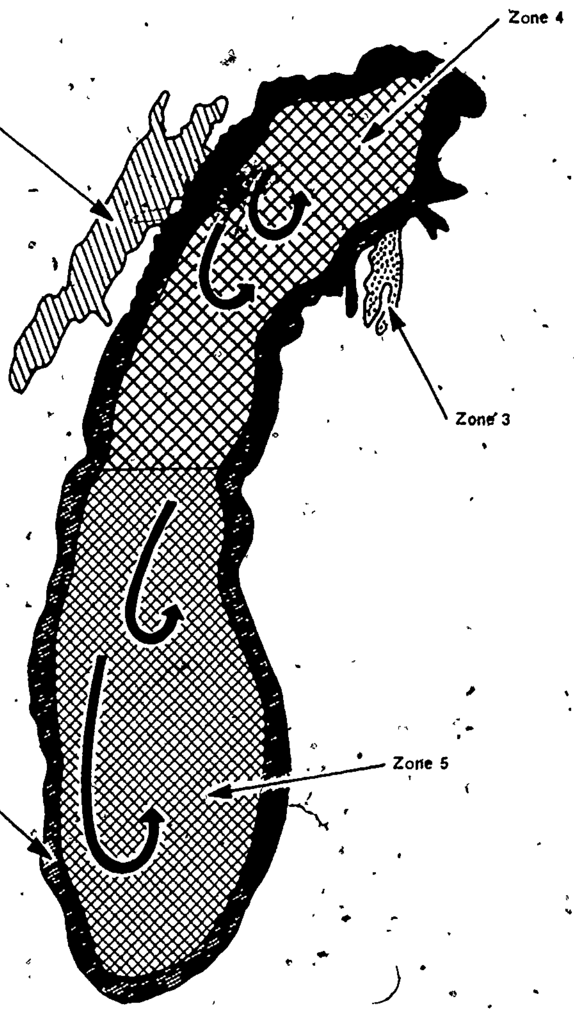

Figure 15. Hypothetical horizontal sectioning of Lake Michigan into uniform zones (from Canale).*

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* Canale, R. P. (No date), A methodology for mathematical modeling of biological production, *Report to the University of Michigan Sea Grant Project Covering Jan. 1970 - Jan. 1971,* 35 pp.

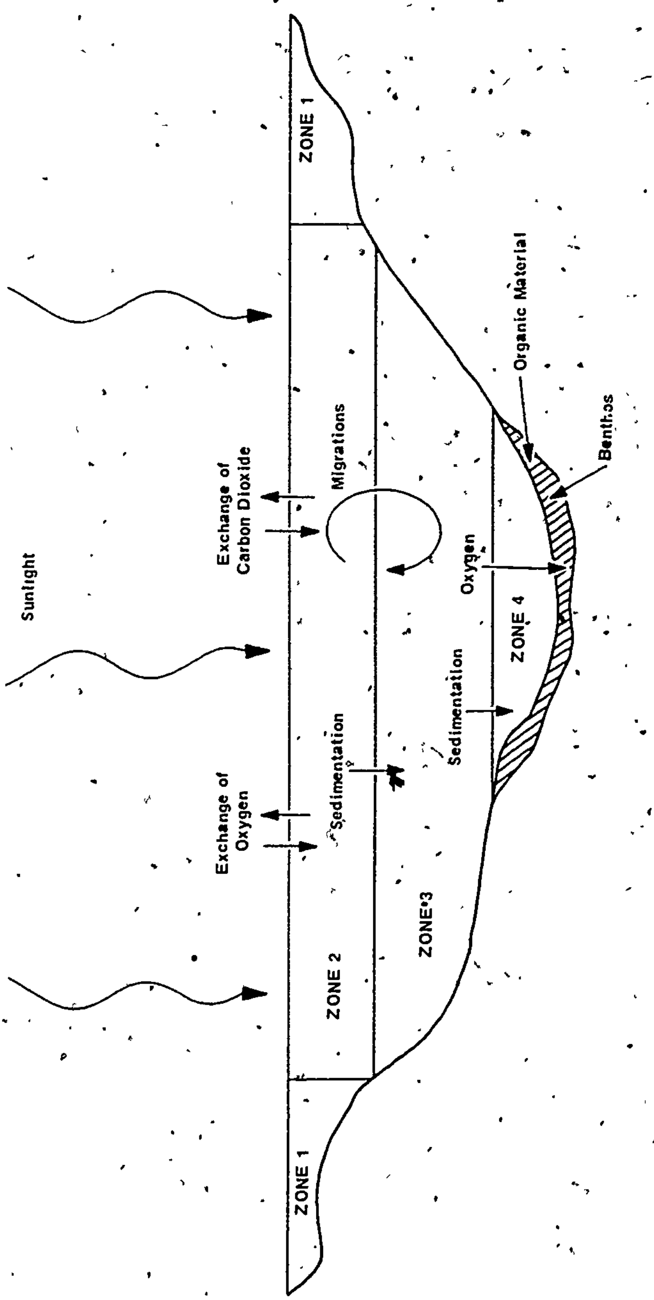

Figure 16. Vertical sectioning of a lake into uniform cells (from Canale).*

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* Canale, R. P. (No date), A methodology for mathematical modeling of biological production, *Report to the University of Michigan Sea Grant Project Covering Jan. 1970 - Jan. 1971*, 35 pp.

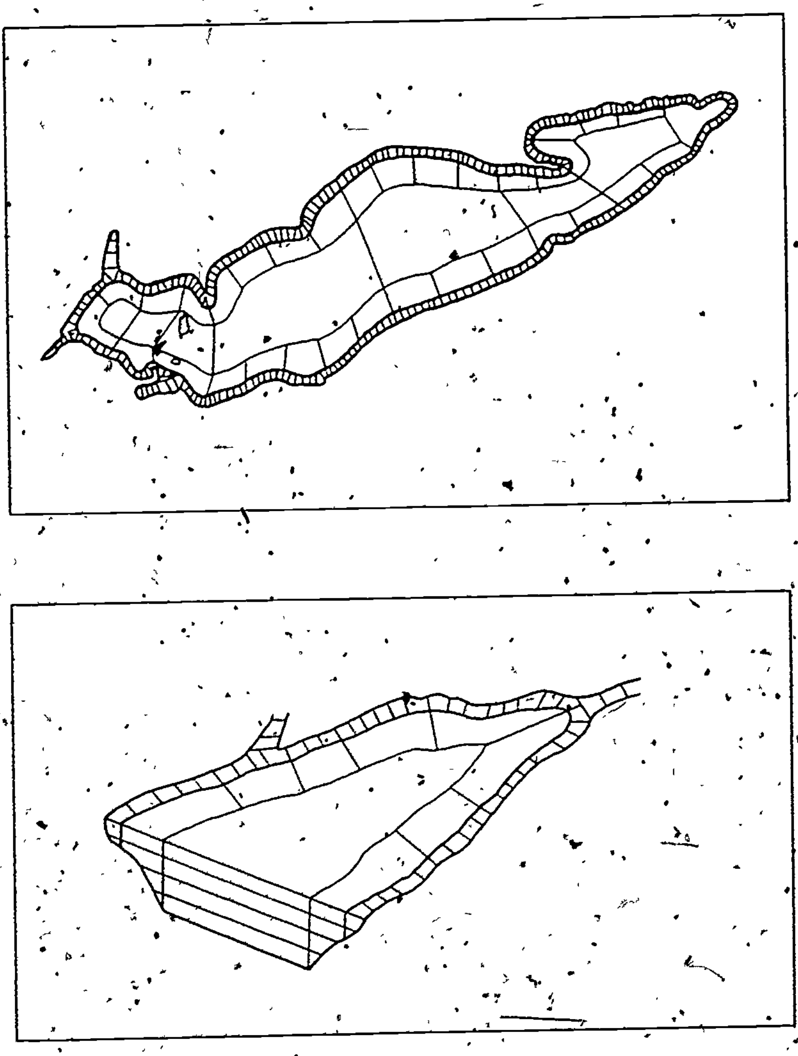

Figure 17. Segmentation for Lake Erie.

65

. Let me say one thing about what we should do in the Great Lakes. The Great Lakes model must perform computations for a series of hydraulic elements that can be arranged horizontally as well as vertically. The hydraulic element may be a small cell along the shore. It could be a big one in the pelagic zone. Any element can accept upwelling and downwelling. It can have horizontal advection which can go both ways. Mass balance computations can be performed for all the important water quality parameters. Transport can either be generated by a hydrodynamic model or prescribed according to field data. Some of the hydrodynamic conditions may be very difficult to compute, but they are easy to prescribe. The object of the model will probably be the simulation of mean monthly water-quality conditions.

1.6.1 Response - R. V. Thomann

I think Carl Chen did an excellent job reviewing the nature of water quality models and their interaction with biology. I will make three points.

First, what did we learn from IFYGL concerning water quality modeling and the interaction between water quality modeling and biological modeling? I think we learned that models that have no circulation in them at all, where lake-wide averages are taken on a horizontal plane and the model is only considered in the vertical dimension, hold a lot of promise, at least, for making long-term planning decisions on the Great Lakes. I think the dynamics of nutrient and phytoplankton behavior for such lake-wide situations are well advanced. We are understanding more and more about the behavior of some of those lake-wide dynamics. The reason I say this is that the basic analytical structure which Carl Chen just reviewed has now been applied to problem situations that span two or more orders of magnitude in total phytoplankton biomass. The analytical structure that Carl Chen reviewed has already been applied, we believe reasonably and successfully, to several different types of water bodies. Some examples are eutrophic estuaries with maximum concentration of 200 μg chlorophyll per liter for phytoplankton biomass; delta regions in California with a maximum of 50 to 100 μg; Chesapeake Bay with concentrations on the order of 10 to 50 μg/l; and Lake Ontario with 5 to 10 μg/l. We have now spanned almost two orders of magnitude, and applications are underway to model Lake Huron which would be 1 to 3 μg/l. By the time we finish with these half a dozen applications of the analytical structure, we will cover environments with almost three orders of magnitude difference. The analytical structure has really held up which I think says something for the ability to utilize the lake-wide average for planning purposes. That is point number one.

The second point I want to make concerns this whole notion of the importance of circulation to phytoplankton dynamics. In addition to the lake-wide model, one of the other outcomes of IFYGL is a first preliminary three-dimensional phytoplankton biomass model of Lake Ontario. That work is just started, but I wanted to show you one preliminary result to illustrate a point. The three-dimensional model is a rough grid five-layer model and looks something like what Carl Chen was talking about (fig. 18). It is an attempt to describe at least some nearshore phenomena. Shore segments extend about 10 km out and are about 40 km long. We use the kinetics given by a lake-wide model which was verified by about 4 years of data.

The question of circulation came up and we hassled this back and forth. How do we handle all these complex interactions we heard about all morning? We took a summer and winter circulation pattern and put in some thermal bar phenomenon when and where we think it happens. All of these phenomena are prescribed externally in addition to the waste loads. We then ran the model. Figure 19 shows a cross section across the lake comprised of segments 14, 16, and 17. Seventeen is Rochester Harbor. The area is 10 m by about 40 m,

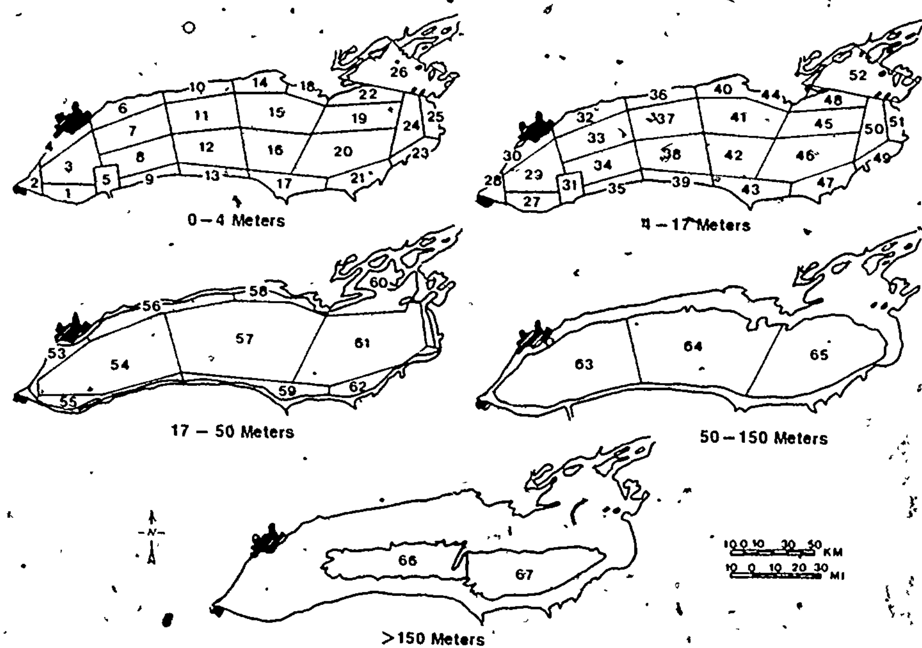

Figure 18. Segmentation for Lake Ontario.

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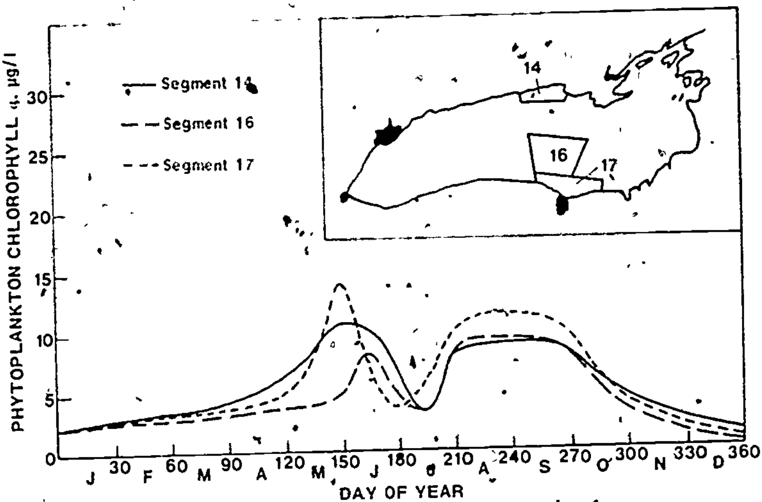
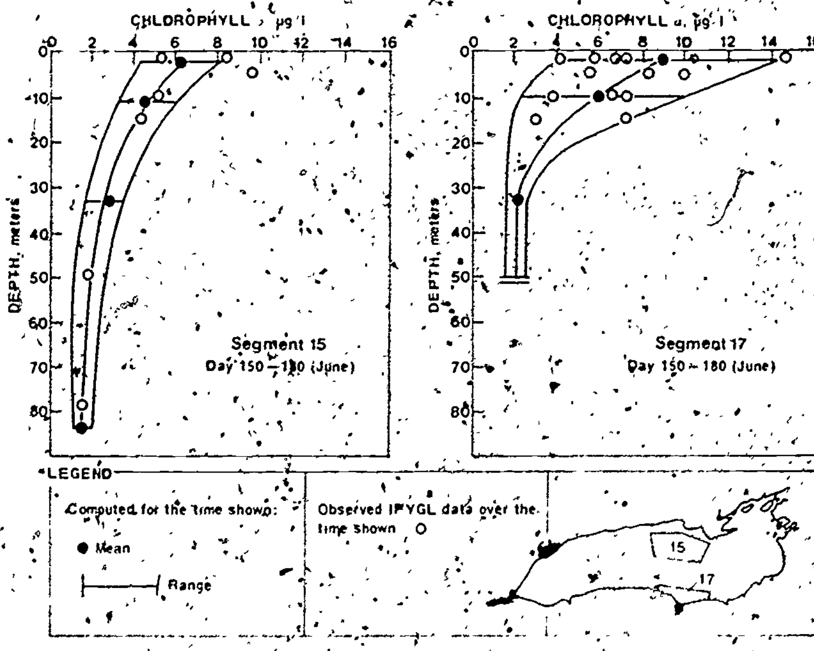


[illegible] as a function of day of year for [illegible] segments of Lake Ontario [illegible].

Which is a pretty coarse grid. The figure shows phytoplankton in micrograms chlorophyll per liter as a function of time of year. A spring bloom develops that precedes the open lake segment (16) by about 30 days, and there is a gradient of about 15 µg/l, which is a reflection of the fact that it is near-shore entrapment. Now compare model output to some observed data as shown in figure 20. The figure is for Rochester Harbor. The black dots are the mean values calculated by the model. The range is what the model calculated during June and the open circles are IFYGL data. The run used the same kinetics as the lake-wide model. The difference is in spatial detail as shown in figure 18. Local circulation, thermal bar effects, and vertical stratification were included. The comparison is remarkably good. There is only one thing wrong with this. We are a little uncertain as to why we did so well on the first shot. For example, this program was not completely finished for this run, so the run does not include any phytoplankton settling. The run also includes an order of magnitude higher concentrations for the phosphorus Michaelis constant (10 µg/l) than what Claire Schelske would normally consider for phosphorus on the basis of his Lake Michigan work. In spite of all of that and a simple circulation pattern, it is really quite surprising that the results are so encouraging.

6.)



CHLOROPHYLL a, µg/l
0 2 4 6 8 10 12 14 16
DEPTH, meters
0 10 20 30 40 50 60 70 80
Segment 15
Day 150 – 180 (June)
CHLOROPHYLL a, µg/l
0 2 4 6 8 10 12 14 16
DEPTH, meters
0 10 20 30 40 50 60 70 80
Segment 17
Day 150 – 180 (June)
LEGEND
Computed for the time shown:
Mean
Range
Observed IFYGL data over the time shown
15
17

[illegible]

Point number three is that the long residence time of water in the Lakes prohibits any kind of meaningful testing of these kinds of phytoplankton models. We cannot reduce a load and make a prediction and then see how well the model does. We are kind of describing what we have already observed in a hindcasting fashion. Also, there are a variety of processes that we have not even begun to touch; for example, the multispecies model that many people have talked about and the problem of nearshore rooted aquatic plants. In spite of these difficulties, the "success" to date leads us to think that there is considerable promise in the utility of these models for aiding the long-term decision-making process concerning effects of nutrient removal on the phytoplankton of the Lake.

1.6.2 Response - S. Chapra

I will supplement Carl Chen's and Bob Thomann's presentations by discussing a scale of analysis which has yet to be addressed today. Although it is not a scientific scale, but rather an engineering or planning scale of analysis, it is potentially useful for addressing some of the Great Lakes water quality problems.

As outlined by previous speakers, the approach has been to study phenomena on a whole lake during a year, or to resolve smaller space and time scales within a lake. Time scales of a week or less have been mentioned. Space scales on the order of kilometers or less have been addressed.

The approach I will discuss would include all the Lakes in one model in a manner similar to the way hydrologists simulate lake levels. However, instead of lake levels, water quality problems might be addressed in time scales of years or decades. This approach has been applied to water quality problems previously and stems from a chloride model of the Great Lakes published by O'Connor and Mueller[3]. Their basic idea was that social and economic parameters, such as population, could be used to generate time series of waste sources to each of the Great Lakes. These sources were then introduced into a simple transport model which treated each of the Lakes as continuously stirred tank reactors (CSTR). In this way O'Connor and Mueller made long-term projects of the chloride levels due to various waste management strategies.

As Bob Thomann just stated, many water quality models are expensive to run longer than a few years. I think O'Connor and Mueller demonstrated that, at least for a certain class of problem, a "Great Lakes space-scale" and a decade time-scale could be effectively used to predict these long-term effects.

There have been some other applications of this approach. For instance, Gustafson[4] modeled tritium levels in the Lakes due to nuclear power plants; Lerman[5] has modeled strontium-90 in this way.

In all cases, simple transport models with simple reaction kinetics were formulated. If other substances such as pesticides, total phosphorus, etc., could be reasonably modeled in this way, we would gain a valuable tool to answer questions about the future quality of the Great Lakes.

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[3] O'Connor, D. J., and J. Mueller (1970), A water quality model of chlorides in the Great Lakes, *Journal of Sanitary Engineering Division*, ASCE, 96, pp. 955-975.

[4] Gustafson, P. F. (1970), Future levels of tritium in the Great Lakes from nuclear power generation, paper given at the 13th Conference on Great Lakes Research.

[5] Lerman, A. (1972), Strontium-90 in the Great Lakes: Concentration-time model, *Journal of Geophysical Research* 77, pp. 3256-3264.

1.7 SIMULATION OF ENVIRONMENTAL DYNAMICS OF THE GREAT LAKES - C. H. Mortimer

In the final agenda for this workshop, the biological section, including ecological modeling, is listed separately from "environmental simulation." I did not realize this before preparing table 8. Without defining "environmental dynamics" too precisely, I intended to talk about interactions between hydrodynamic and ecological models, not because I can claim to be a modeling expert, but because the design of an optimum interaction strategy is the most important task facing us if limnological modeling is ever to have a usefully predictive impact on lake management. Ignorance rarely constitutes a bar to public speaking, but the results are often platitudinous. Nevertheless, I hope my five platitudes (table 8) will serve to generate fruitful debate.

The first platitude is an attempted one sentence definition, with alternative wordings, of the purpose of lake system modeling. The second is a triarchy of three interacting boxes, a triarchy illustrating the application of the scientific method to acquisition of understanding of lake systems. Box A (top left) represents the way in which limnologists have traditionally worked in the past, through development and testing of conceptual hypotheses. This box is a compendium of knowledge, or suppositions, or biases, that constitute "what every limnologist knows." Box B (top right) is a fairly recent arrival on the scene, i.e., computer manipulation of systems of equations which can be deterministic, stochastic, or a mixture of the two. We could spend all day of this 2-day workshop defining various types of models and what they do.

There is also a third "model" or source of knowledge in box C, i.e., the real lake, providing a direct avenue to knowledge through what Claire Schelske referred to as descriptive studies. Classical limnology has been largely based on a combination of descriptive study results and conceptual hypotheses derived from those results. Therefore classical limnology is represented by the box pair A-C, while box pair B-C represents the recent emergence of what we might call mathematical ecological limnology. There are various interacting arrows between the boxes. The downgoing arrows lead to improvement in experimental design, either from conceptual or numerical modeling. The upgoing arrow on the left feeds from the data base to the conceptual hypothesis. Via the right-ascending arrow, the data base provides verification for the mathematical model, most essential to test the model's worth and to improve it.

This leads me to the third platitudinous statement: IFYGL has provided an unparalleled data base in spite of what I said about instruments this morning, and this in turn gives an unequalled opportunity for verification of a variety of models. The surest method of approach is a pedestrian, bipedal, progressive iteration between modeling, improving experimental design, verifying the models, and so on. We must walk before we run, and we should not promise too much.

1. Introductory platitude: Like system models needed to assess costs of not taking management actions.
2. Model triangle: Three general interacting model classes (or approaches) are available and shown here in boxes:

Classical limnology

A — Conceptual hypotheses: "What every limnologist knows"

interactions need strengthening

Computer manipulation of systems of equations (sub-models), deterministic and/or stochastic — B

improvement of experimental design

tool for and verification of hypotheses

Experimental interrogation of nature, through limited-aperture windows, yielding the "data base" — C

model verification (essential)

mathematical ecology

3. Environmental Modeling under IFYGL: The prime purpose was to use the large data base C--already available from CCIW and other sources and later to be provided by IFYGL--to test and perfect numerically predictive schemes, designed in B. In particular, the hitherto unsurpassed physical data base was to be combined with biological data to predict production and species competition in given hydrodynamic regimes and with given inputs of radiation and nutrients.
4. Anticipated difficulties: It is too early to assess the degree of success achieved under 3, but "classical limnology" (box A founded on box C) suggests that, while hydrodynamic modeling is approaching a useful operational stage given adequate routine updating from C, combination of physical models with the present primitive biological models to form operationally predictive whole-system models will encounter severe complexities and will therefore be delayed. Two examples of anticipated complexities are given (Mortimer, C. H. (1975), contribution to North Atlantic Treaty Organization Symposium: "Modeling of Marine Systems," Elsevier [illegible]), one arising from the episodic nature of mechanical forcing and the nonlinear characteristics of localized shear-flow instabilities, and one arising from the switches to new sets of biological species, which commonly occur when a lake system is highly perturbed and which the environmental manager above all needs to predict.
5. Lines of future progress: In spite of (and because of) the anticipated complexities, developments in box B continue to be pursued. Rapid progress should not be promised, but the best hope of advance lies in a successive, two-step, pedestrian B/C iteration (modeling/verification) with hard work in both boxes. Also A/B interactions can be more fruitfully fostered, avoiding present signs of elitism in both camps, with A calling B "naive" and B calling A "numerically illiterate." Field interrogation and window design (new instruments, better resolution in time and space) in C also need strong and continued support, balanced against support of A or B, for if management of nature is an objective of modeling, nature is the best source of clues, short cuts, and tests.

Under the fourth platitude, I will talk about anticipated difficulties in both physical and ecosystem modeling. I was surprised to hear Claire Schelske say that "big science" had not included ecology. The International Biological Program provides an example. It also provides examples of counter-productive, elitist confrontations, treated in the final paragraph of the table (platitude 5).

Some classical limnologists have called systems analysts naive, and systems analysts have regarded classical limnologists as numerically illiterate. While there may sometimes be truth in both these accusations, it is more important to recognize and to strengthen the interactions between boxes A and B, as well as the important modeling-verification, i.e., B-C interactions already mentioned.

I now give (in platitude 4) two examples in which A-C or A-B interactions could be productive and indeed essential for progress. The first is a physical example, a conceptual A-type model of wind-driven motions in a small, stratified lake (fig. 21), in which wind drag at the surface moves the surface warm layer to the downwind end of the basin. A return current forms just above the thermocline and, if the resultant shear in that layer exceeds a critical value associated with a Richardson number of 1/4, the flow becomes suddenly unstable and large vortices form. The return current acts like a carpenter's plane, eroding the subthermocline layer by entrainment and intensifying the thermocline gradient at the downwind end of the basin. The "shavings" of mixed water are carried toward the upwind end by the return current, yielding the observed fan-shaped distribution of isotherms. When the wind stops and its stress is removed, the preexisting and the newly formed layers undergo redistribution accompanied by a series of oscillations (internal seiches) to new equilibrium positions.

The important point about this conceptual model--yet to be verified in detail, but obviously describing observed features--is that the final depth and shape of the thermocline depends not only on what happened in the water column at that station, but more importantly on events, e.g., flow instabilities, elsewhere in the basin. But presently available physical and mathematical models of thermocline formation and entrainment are one-dimensional and therefore of limited use in predicting day-to-day developments in a lake.

The overriding importance of the Richardson number and the nonlinear nature of the "explosive" change from stable to unstable flow when that number falls below 1/4 is illustrated by an IFYGL example from Lake Ontario (fig. 22).[6]

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[6] Boyce, F. M. (1974), Some aspects of Great Lakes physics of importance to biological and chemical processes, *Journal of the Fisheries Research Board Canada* 31, pp. 689-730.

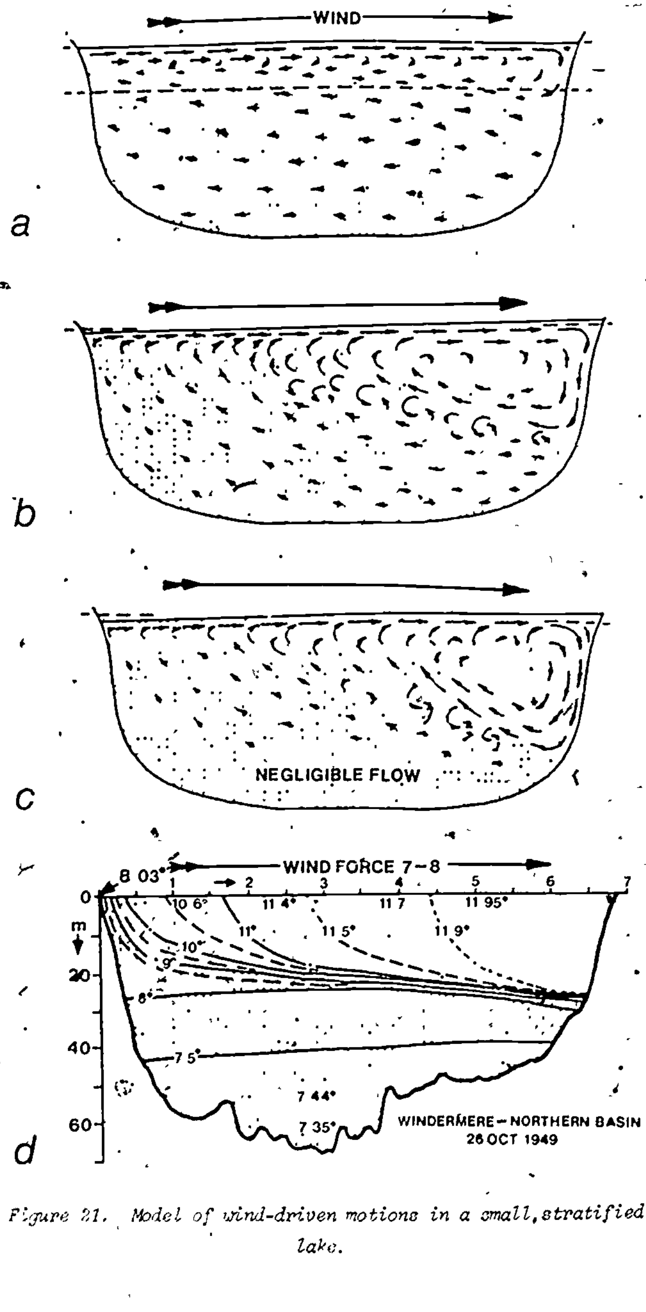

Figure 21. Model of wind-driven motions in a small, stratified lake.

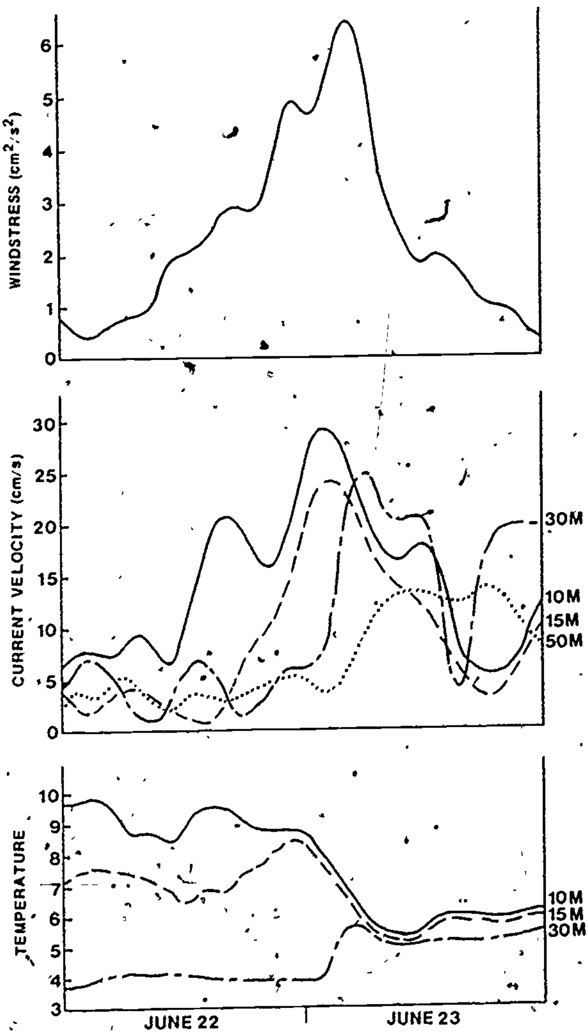

Figure 2[illegible]. [illegible] of [illegible] in Lake Ontario (after Boyce).*

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* Boyce, F. M. (1974), Some aspects of Great Lakes physics of importance to biological and chemical processes, *Journal of the Fisheries Research Board Canada* 31, pp. 689-730.

Current meters and thermographs were placed at 10-, 15-, 30-, and 50-m depths. The wind stress (aftermath of Hurricane Agnes) was computed. With rising wind stress at the surface, the current velocity began to rise first at 10 m until the velocity difference between 10 and 15 m depths exceeded a critical value, at which time the temperatures at those ~~two~~ depths were suddenly equalized. It is significant that this equalization took place when the mean Richardson number between 10 and 15 m had fallen to about 1/4, at which point mixing occurred and momentum was then transferred to deeper layers. Subsequently the same sequence was repeated between 15 and 30 m. This is a beautiful example, the first of its kind, of the downward transfer of momentum, clearly showing the relationship between shear instability and mixing.

The second illuminating example is an extract from Lund's[7] many-year study of the spring increase in diatom ([illegible]) population in three neighboring lake basins with similar nutrient input, but considerable differences in maximum depth (Esthwaite Water, 15 m; southern basin, Windermere, 33 m; northern basin, Windermere, 65 m). In most years, a simple silica-limited model, with uptake rate proportional to basin depth, fits the observations very well. The diatom population shows a log phase of growth that terminates when the silica concentration has fallen to about 0.4 mg/l SiO_2. Growth starts earliest in the shallowest and latest in the deepest basin because the average light exposure of a diatom cell is proportional to the ratio of the depth of light penetration to the depth of the water column, which is well mixed to the bottom in all three basins at that time of year. This is the simple depth-controlled, nutrient-limited model proposed by Gran in the 1920's to explain the sequence of spring diatom-population peaks in the Norwegian Sea and later used by Riley for U.S. coastal waters.

There are exceptional years, however, in which the simple model fails, as for example, in one case (Esthwaite Water during 1949), as a result of fungal parasitism of [illegible], and in the northern basin of Lake Windermere during 1948 probably because of lack of an essential but unknown micronutrient added to the lake during years of normal or above-normal rainfall, but lacking in years of spring drought. When the spring flood eventually came to Lake Windermere in 1948, normal diatom growth was resumed but with a 2-month delay. The failure of the simple model in these two cases must be attributed to biological peculiarities which are not uncommon, in one case due to parasitism that is difficult to predict in a deterministic manner and in the other case due to the result of poorly understood mechanisms of cell nutrition.

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[7] Lund, J. W. G. (1950), Studies on *Asterionella formosa* Hass. II. Nutrient depletion and the spring maximum, *Journal of Ecology* 38, pp. 1-35.

These peculiarities can also be instructive for would-be lake system modelers. During 1949, the year of failure of the simple model in Esthwaite Water because of parasitism, other diatoms took over and grew in place of the parasitized *Asterionella*. This illustrates an important property of lake systems, well known to limnologists in box A (table 8), but not sufficiently appreciated by systems analysts in box B, namely, the fact that gross perturbations of the system commonly remove one set of actor organisms from the stage, replacing them with a different set. Unfortunately, models to predict the effects of <u>perturbations</u> are precisely what lake managers need most, but it seems that this is the type of model which will be most difficult to give him.

Resolution of this difficulty and the interfacing of the very different time and space scales of hydrodynamical and biological models should be one of the prime post-IFYGL research targets.

1.7.1 Response - G. F. Lee

I have chosen to use my alloted time on the topic area of Simulation of Environmental Dynamics of the Great Lakes to focus on those aspects of water quality simulation which I feel should receive attention in the immediate future. No attempt will be made in this presentation to determine what agency or agencies should focus on these problems; instead, the problem areas will be outlined and briefly discussed.

From an overall point of view, it is important to emphasize that simulation or modeling has a definite place in research on and management of water quality in the Great Lakes. The Great Lakes, like many other bodies of water, are experiencing water quality problems due to excessive discharge of chemicals. Chemical problems can, in general, be compartmentalized into three approaches defining the sources, fate, and significance of specific chemical contaminants for a given part of or the whole of the Great Lakes. Each of these compartments can be formulated into relatively simple models which describe the overall transport and transformations of the chemical contaminants. Further, for each of the major chemical species, models can be developed that demonstrate how these species interact with various parts of the aquatic ecosystem and, therefore, how a given concentration of a contaminant could affect water quality. Models of this type serve as frameworks to compile existing information, thereby pointing to areas where additional work is needed. Therefore, such models should be developed prior to initiation of any research on the problem.

Further, these models are extremely helpful in defining possible management alternatives and benefits to be derived from certain types of management policies for chemical contaminants in the Great Lakes. The work sponsored by the EPA as part of the IFYGL studies on nutrient sources, transport, and cycling within Lake Ontario is a prime example of how such modeling efforts can be used for water quality management. The overall objective of these studies was to determine what benefits might be derived from the removal of 80 percent of the phosphorus from domestic waste waters entering Lake Ontario. The sources of phosphorus have been fairly well defined, and studies have been conducted that estimate the amounts of available phosphorus entering the lake from each source.

A major modeling effort by Thomann and DiToro of Manhattan College is on the relationships between a concentration of phosphorus in Lake Ontario water and the biomass that would develop in the open waters of the lake. When fully developed, this model will provide a technical basis for estimating the decrease in biomass of planktonic algae that might result from an 80- to 90-percent removal of phosphorus from domestic waste-water sources. Discussed below are other areas of water quality modeling which I feel should receive attention in the near future.

At present, several individuals are developing what might be called conservative element models. These models enable researchers to utilize current rates of input, future populations projections, and the mixing character of the lake in making reasonably accurate predictions of concentrations of elements such as chloride. Modeling efforts of this type are relatively simple because they deal with chemical compounds which are nonreactive in the system and, therefore, focus on dilution of the materials added to the lake. The ability to predict an open-lake concentration of a particular chemical species is directly dependent on the reliability of input data, the lake mixing characteristics, and the hydrology of the lake.

There is need for additional work in this area in order to better understand the nearshore mixing processes. For example, it is often said (without any technical basis) that at certain times of the year the thermal bar represents a significant barrier to mixing between nearshore and offshore waters. However, when one examines the conservative element composition in these waters, both for periods when the thermal bar is present and when it is not, one finds that the concentrations in both water areas are approximately the same. This indicates that the overall rates of transport of chemicals and water between the nearshore and offshore areas are independent of the presence of the thermal bar.

The open-lake eutrophication modeling efforts, being conducted as part of the EPA Chemistry and Biology Panel activities for the IFYGL studies on Lake Ontario, are progressing well. In my opinion, the models being developed by the Manhattan College group appear to be of sufficient technical validity to warrant further major efforts along these lines for the other Great Lakes. It should be noted at this time that these eutrophication models are for the open waters of the lake and do not consider nearshore processes. Also, these models do not presently relate total phosphorus flux to the lake's response. They are based on a concentration of available phosphorus in the open lake water. In order to determine the relationship between amounts of phosphorus entering the lake from both its tributaries and direct wastewater inputs and the amounts of phosphorus that will eventually become available in the lake, chemical modeling should proceed simultaneously with eutrophication modeling.

Of all Great Lakes water quality modeling efforts, probably the most needed today is development of a nearshore eutrophication model. Such a model would demonstrate the relationship between nutrient input from tributaries or direct waste inputs and the growth of attached algae such as *Cladophora*. Essentially, no significant progress has been made in this area. Yet, this is one of the most significant water quality problems in the lower Great Lakes. At present, it is impossible to predict with any degree of reliability what environmental impact reducing the phosphorus input to the nearshore waters of a given region will have on *Cladophora*. One of the problems that makes modeling of this type especially difficult is that a key aspect governing the growth of these algae is the hydrodynamics of the interface between the organisms' holdfast (i.e., substrate) and the overlying waters. Growth of *Cladophora* is not only dependent on the concentrations of nutrients in the water, but also on the rate of transport of these nutrients to the algae. From an overall point of view, I feel that the next major IFYGL study effort on the Great Lakes should bring the hydrodynamist, biologist, and chemist together to work on nearshore water-quality models emphasizing the *Cladophora* problem.

Reasonable progress is being made today on oil spill modeling in order to predict the rate of transport of oil and gasoline. Further, some efforts are being made in modeling of dredged-material disposal practices. Generally, however, modeling efforts in this area are hindered by lack of information on the short-term, high-concentration toxicity of chemical species to aquatic organisms. The acute toxicity data that are available today are generally based on a 96-hour exposure period. For dredged material disposal, the excessive concentrations rarely persist for 96 hours. Instead, after a few hours, higher concentrations are rapidly diluted below the acute lethal level. Within a relatively short time contaminant concentrations fall below chronic sublethal levels as well. For example, it is known that dredged material disposal in open waters results in a release of ammonia to the water column. In many instances, the concentrations of ammonia will be above the 96-hour LC_{50}. However, because of the intermittent nature of the dumping practice, the relatively high concentrations of ammonia are usually diluted within a few hours to below acute toxicity levels and within a day or so to background levels.

It is impossible at this time to establish criteria for such a situation since we do not have short-term ammonia toxicity data for various forms of aquatic life. Data are needed on the relationship between the acute lethal concentrations at various periods of time that match to some extent the normal rates of dispersion that occur from a point source. This same type of data is needed for industrial and municipal outfalls into the lake.

There is a need for models that can simulate (and thus offer some potential for predicting) the environmental impact of chronic sublethal effects of chemical contaminants on Great Lakes waters. The 1972 amendments to the Federal Water Pollution Control Act require that by the mid-1980's, industries, and quite possibly municipalities, demonstrate why they should not achieve a zero polluting discharge from their installations. They will have to consider technical feasibility, economics, social desirability, and environmental impact. The general problem that exists today and will certainly prevail in the future is not one of acute lethal toxicity. Instead, it is one of chronic toxicity, impairment of the rate of growth, rates of reproduction, or other vital functions of aquatic organisms. Even so, there will not be a complete blockage of reproduction but probably some impairment, i.e., a 10- or 20-percent reduction.

It is highly likely that environmental quality litigation will raise questions about the significance of even a 10-percent reduction of reproductive potential of a certain form of fish due to the presence of an apparently excessive amount of certain chemical. Largely as a result of the current relatively poor understanding of ecosystem functioning, at this time we have essentially no ability to answer this type of question. If decisions are to be technically sound, it is mandatory that efforts be made to design ecosystem models to determine the significance of a certain size waste-mixing zone where there is an impairment of fish reproduction in the fisheries of the lake as a whole.

These results would be applicable not only to chemical inputs but also to heated-effluent discharges from electric generating stations utilizing once-through cooling. Each of the Great Lakes has a large heat-assimilative capacity whereby waste heat could be added to the lake without significant impairment of overall water quality. There is no doubt, however, that it would be possible to add sufficient heat to the lake to affect adversely the overall lake ecosystem. At this time, we cannot predict with any degree of reliability what the ultimate heat-assimilative capacity is for any part of the Great Lakes. Therefore, modeling efforts should be initiated in an attempt to determine the impact of impaired water quality in one region on the overall ecosystem.

Currently some progress is being made on hydrodynamic models of the dispersion from a point source, such as a wastewater outfall. Probably the greatest success of modeling efforts in this area is in connection with predicting the size of thermal plumes from electric generating stations. This can be done today with a reasonable degree of reliability. However, virtually no information is available on modeling of the chemical concentrations and toxicity of contaminants in the nearshore zone. There is a need for modeling directed toward examining the physics, chemistry, and biology of mixing zones for wastewater input to the Great Lakes. From an overall point of view, such modeling of chemical contaminants is hindered by lack of information on the environmental chemistry, physical transport, toxicity, and/or stimulatory capacity of specific chemical species in Great Lakes waters.

There are several additional study areas. Related to the questions of modeling of the water quality of the Great Lakes is the development of monitoring programs designed to evaluate changes in water quality as a result of man's activities. At the present time, no one has determined the number and location of monitoring sites in the Great Lakes in an attempt to detect significant changes in water quality. To do this in a meaningful way, at least crude models of the expected response of the lake to various inputs must be available.

I feel NOAA or some other agency should work on ascertaining the significance of marshes and wetlands to the ecology of the Lakes. Some parts of the Great Lakes have considerable areas which interface with wetlands or marshes. In marine systems, marshes are known to be the primary source of nutrients and foods for larval forms of aquatic organisms. Yet little work has been done on the significance of wetlands to the Great Lakes aquatic ecosystems.

Another area that should be considered for possible NOAA activity is development of a data storage and retrieval system on Great Lakes water quality. The existing systems simply are either nonfunctional or unreliable. These are of little or no value in establishing water quality of the Great Lakes. There is also need for periodic critical examination of the data to ascertain whether there has been any change in the water quality of the various Lakes. In addition, someone with a high degree of technical competence should review all data going into the storage system in order to ensure their reliability. Further, some permanent record should be kept of the analytical methods that were used to generate the data. Then sometime in the future, someone from either within or without the agency can examine the historic data and determine whether or not there had been real changes and whether apparent changes can be ascribed to changes in analytical procedures and/or sampling techniques.

Another aspect of Great Lakes water-quality problem studies which I feel needs considerable attention is the diffusion of phosphorus sources for the Lakes. Recently completed studies by W. F. Cowen and myself have shown that only a small part of the total phosphorus present in the organic and particulate forms entering Lake Ontario from tributary sources will likely become available in the lake. This means that efforts to control urban and rural storm water drainage in many parts of the Great Lakes basin would result in little or no improvement in water quality because the majority of the phosphorus derived from these sources is in an unavailable form. The studies of the type conducted by Cowen and Lee on the Lake Ontario Basin should be expanded to all of the Great Lakes and include not only tributary but also atmospheric sources of phosphorus.

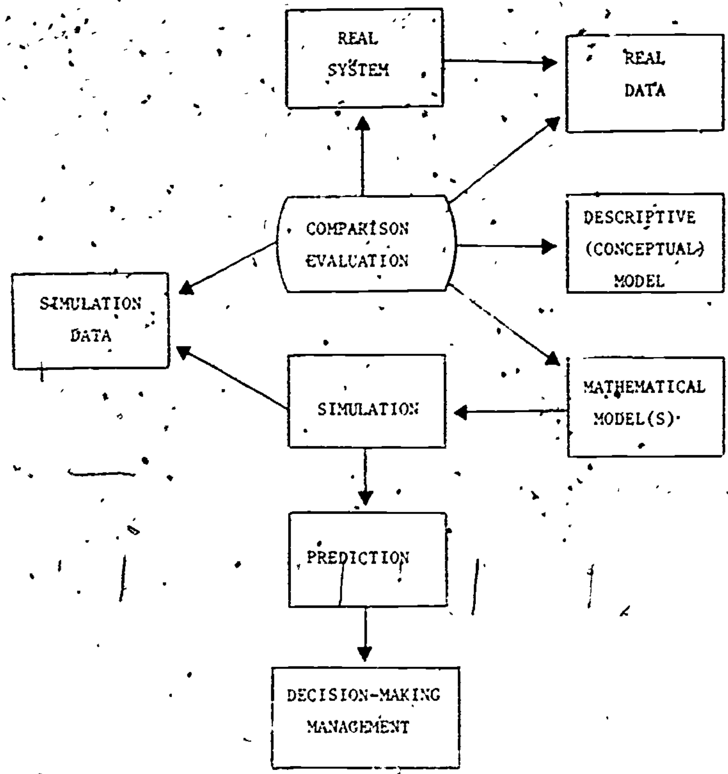
There is a great need for a comprehensive look at dredged material disposal criteria in order to determine what is the actual environmental impact of offshore disposal of contaminated dredged sediments on the Great Lakes. These criteria are of great economic significance to the Great Lakes. The current ban on open water disposal of dredged materials in the Great Lakes within the next few years will cost an estimated $230 million for dike disposal areas. There are serious questions about whether this expenditure is justified from an economic, or more importantly, ecological point of view. It is highly probable that dike disposal systems currently being developed may do more harm to the aquatic ecosystems in the Great Lakes than have open-water disposal systems used in the past. Efforts in these areas should be closely coordinated with the U.S. Army Corps of Engineers Dredged Material Research Program.

1.7.2 Response [illegible]

I will expand on Dr. Mortimer's "model trilogy," perhaps adding a little resolution to it. I will also consider what a model is and what it can do and consider some basic questions about modeling and its relationship to research from a [illegible] viewpoint. I have also detected what Dr. Mortimer called "an elitist [illegible]"; some people tend to scoff at the idea of modeling and perhaps think it is a separate endeavor. I would rather think of it as an integral part of the research process.

Relating models to research (fig. 23), what we are interested in is, of course, the real system and what makes it tick. From the real system, we derive real data; from the real data, we derive a descriptive or conceptual model. The classical feedback loop has pretty much dealt with these components: real system, real data, conceptual model; or it has gotten to the level of a mathematical model about which, after it has been put on paper, people say, "Yes, this is fine, seems to look right, etc." Again, going back through this loop that has been research up until recent years, we are now capable of testing mathematical models in a very impersonal fashion. The value of the computer is that it provides an impersonal evaluation of your model. If it is no good, the computer is going to tell you by providing simulation data. Now you have something to compare with the real data. It is usually not going to match up with real data the first time. We are going to crawl before we walk. So what do you do then? You modify conceptual models, mathematical models, and simulation. Predictive ability is sort of a spinoff. I think too many people are thinking in terms of using simulation as a predictive and decision-making tool. Maybe we will get this kind of spinoff in the future, but the real value of simulation is that of providing tests for the conceptual models that have been developed over the years. The computer can also keep track of multiple, complex-coupled nonlinear interactions which are characteristic of ecosystems and which confuse the human mind.

What is the role of simulation? It is part of the research feedback loop (fig. 23) and everybody should be involved in it. It ~~is wrong~~ to have one guy going out and collecting the data and another guy doing the simulation. Ideally, each researcher should be involved in both parts of this process one way or another because it provides an impersonal evaluation of scientific concepts. If the simulation does not match up with the real system, you should say "hurray." That is the best thing that could happen. Do not be downhearted or say, "Let's throw it out." If it works completely and matches up with the real system, your job is done and you have not learned anything. You have the old concepts; but you had them before. If it does not work, then you know something. The old concepts were not totally correct. Now you have to find out why it is the simulation data do not match up with the real data. For example, there must be something lacking in our understanding of the whole process of circulation of lakes. If the simulation does not match up, it means there is something wrong with the conceptual model. A concept may be completely lacking



[illegible] Research [illegible]

or there may be some kind of nonlinear interaction taking place that we are not aware of. On the other hand, maybe the model is no good. That is a possibility, but, assuming you have checked the model mathematically, it is doing what you think it is doing and the real data are correct, then if the simulation does not work, there is something wrong with the conceptual model.

People should not be very concerned that we are starting out with very crude models. Eventually we will add to them. Look what happened with the myoglobin molecule. First they worked out a model with 10-angstrom resolution. All they got was some big hunky thing. All they knew previously was that there was some ordering of the molecule. Gradually, as the resolution was improved, we were able to see a sharper and sharper picture of the chemical molecule. Here we are working with a mathematical model. You have to start with a crude, low resolution thing and work toward the high resolution models of the future. You must crawl before you walk and walk before you run.

1.7.3 Response - B. Eadie

I want to emphasize some of the points that I feel are really important. Primary is the one brought up by Prof. Mortimer this morning that biological models and chemical-biological interaction processes are poorly understood in comparison with physical processes. The reason is that no strong theoretical foundation for biology and nonequilibrium chemistry exists, especially in aquatic systems.

Prof. Lee's analysis of our weakest points in nearshore chemistry is also well taken. Effects of river plumes and water mass entrapment on biological systems are incompletely understood. The sublethel effects of toxic materials are something the oceanographers have begun to look at, and I think we can take a key from the beginnings of their research. They are beginning to look at sublethel effects of petroleum hydrocarbons and pesticides on some fish and smaller organisms in the ocean.

Looking back at Prof. Mortimer's diagram, what we need to do is in box C, Experimental Interrogation of Nature. That is where, in the biological and chemical areas, we have to expend our greatest effort if we are going to better understand the system. We do not understand the formalizations or the functional relationships which exist between biological uptake and some chemical species. What mechanisms are triggered by certain perturbations in the biological system? What causes a bloom to occur? What we will have to look at is something on a higher frequency scale and a smaller spatial scale than was attempted in IFYGL.

As Dr. Aubert mentioned this morning, the natural distribution and variations in the chemical parameters were not well understood in IFYGL, although analysis has just begun, primarily because they were not collected on a fine enough grid and time scale to get good relationships. What we need to do in the study of environmental dynamics is to examine the system at a higher frequency in a spatial scale we can handle.

1.8 DISCUSSION

Aubert. I want to comment on the interpretation of Environmental Dynamics as a title used for the plenary and work group sessions. I conceive of a model hierarchy with environmental dynamics being the most inclusive level. Environmental dynamics involves the interactions of all relevant processes. If a model diagram were drawn, environmental dynamics would be at the top. Four other items are roughly equal but one level below environmental dynamics: water movements, aquatic ecology and water quality, land-water shore interactions, and water levels and flows. Somebody else would probably set up a different hierarchy. A hydrologist says hydrology includes limnology; a limnologist says limnology includes hydrology. It depends on the viewpoint as to how a structure is set up, but that was my rationale in preparing the outline. No holds are barred in the area of environmental dynamics, nor should there be restrictions in the other areas. In some respects, aquatic ecology may be almost as broad as environmental dynamics, but I think aquatic ecology emphasizes biological and chemical aspects more than physical aspects.

Mortimer. Will there be an opportunity for the groups to get together aside from the group sessions?

Aubert. In the workshop sessions.

Mortimer. Could two work groups merge if they felt so inclined?

Aubert. Yes. Again, in concept, this was structured to maximize output and also to reduce groups to manageable size. While people have been assigned to work groups, it is not meant that everybody must remain in that one work group. Some amount of floating between work group sessions might make some sense in that there is clearly overlap between the groups; however, I cannot suggest how one might float in order to participate in all relevant discussions. Likewise, the chairmen may feel that for part of the workshop session it might be desirable to schedule a joint work group session. That decision will be left to the chairmen.

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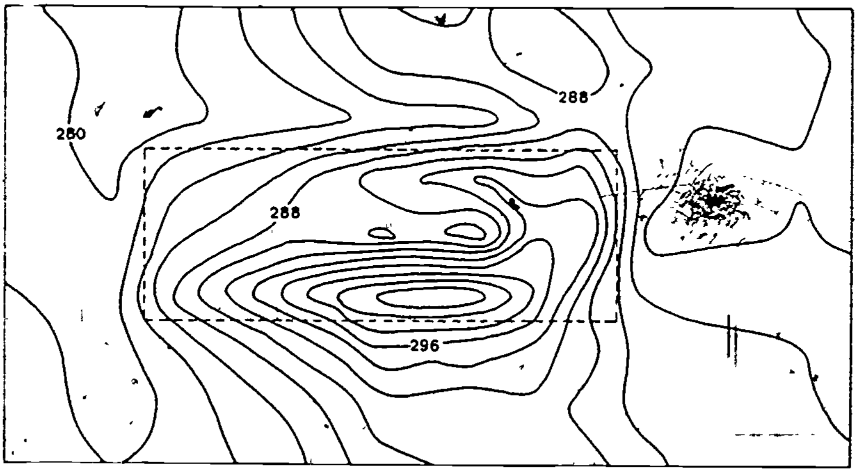
1.9 LAKE-ATMOSPHERE BOUNDARY LAYER PROCESSES OF LARGE LAKES - M. Estoque

For the purpose of my presentation, I will assume that the problem for this group is the prediction of boundary layer processes over large lakes. The term boundary layer processes is understood to refer generally to the turbulent fluxes of momentum, heat, and moisture. You might ask whether or not there is, indeed, a problem. We know, of course, the approximate behavior of the boundary layer over lakes; there is a problem only if one wants to predict the magnitudes of the processes more accurately than we can at present. The accuracy of this prediction at present is not high, in general within a factor of five, but somewhat better in cases when the thermal stratification of the atmosphere is near neutral. Why are boundary layer processes over lakes important? They are important because they are the mechanisms which generate the surface water currents and transfer heat and moisture between the lake and the overlying air.

The behavior of the boundary layer over large lakes depends primarily on the prevailing synoptic conditions and the lake surface temperature. Synoptic conditions over the Great Lakes change due to the passage of cyclones and anticyclones. The change is more or less regular, with a periodicity of about 1 week. On the other hand, the lake surface temperature changes much more slowly. The changing synoptic conditions, in conjunction with the lake temperature distribution, produce corresponding thermal stability changes in the boundary layer. One can classify the thermal stability conditions into three categories: unstable, neutral, and stable. The unstable condition generally occurs during the winter season when the lake surface temperature is warmer than the surface air associated with the large-scale prevailing flow. The neutral condition occurs when the surface air has the same temperature as the lake surface. The stable condition occurs in late spring and early summer when the lake surface is colder than the surface air. In general, the intensity of the boundary layer processes over the lake is largest under unstable conditions and least under stable conditions.

It might be of interest to give an indication of how much the large-scale synoptic condition can be modified by Lake Ontario. This is done with the aid of a numerical simulation of a thermally unstable case. The synoptic condition which is simulated is that which occurs during the period immediately following the passage of a cyclone slightly north of the lake; the period is, therefore, characterized by a veering of the wind from westerly to northwesterly over the lake. In order to simplify the numerical integrations, Lake Ontario was replaced with a rectangular lake of about the same size. The lake surface temperature is assumed to be uniform and 20°C warmer than the large-scale prevailing surface air. The simulated distributions of air temperature, pressure, and velocity (fig. 24-27) correspond to distributions after the synoptic wind has veered from westerly to northerly. Figure 24 shows the air temperature distribution near the Earth's surface. A warm pool of air is



perature distribution in the surface boundary layer.

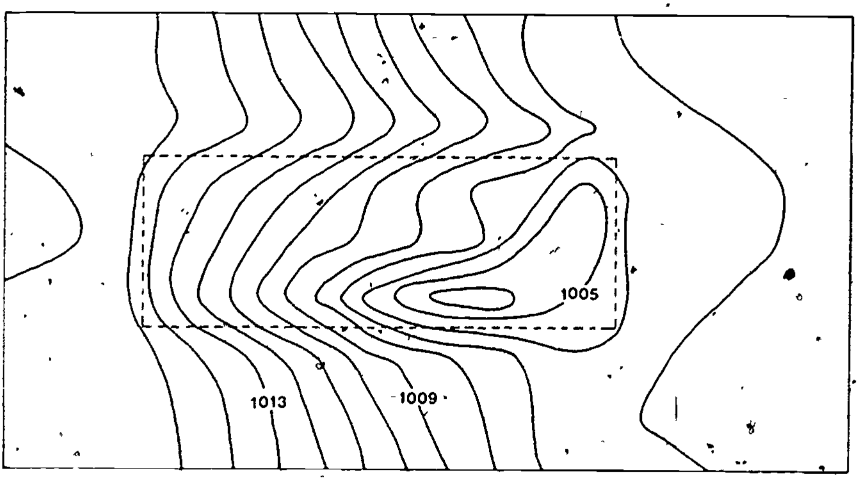


Figure 25. Surface pressure distribution.

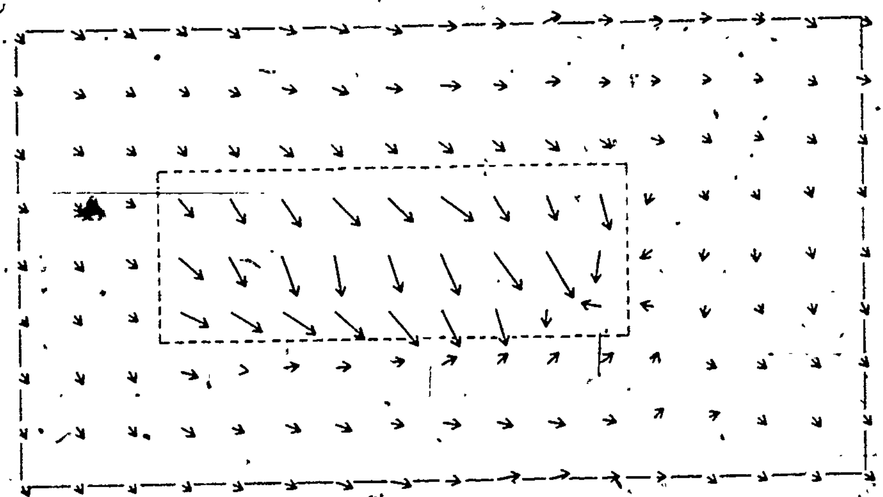


Figure 26. Surface wind distribution.

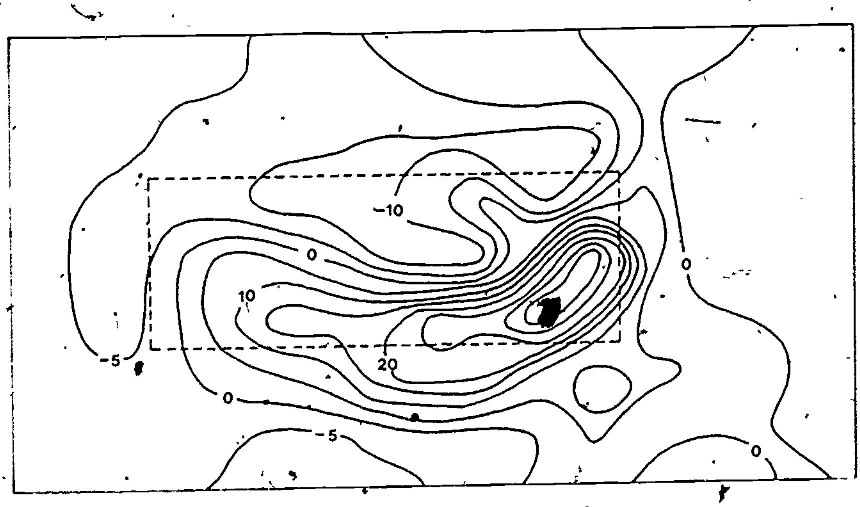


Figure 27. Vertical velocity (cm sec^{-1}) distribution at a height of 1 km.

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generated; the center is south of the lake center, close to the southern coastline. This feature is a result of the warming of the air as it moves southward across the warm lake. This warm pool is reflected as a low pressure area at the surface as shown in the surface pressure distribution (fig. 25). Figure 26 shows the surface wind distribution. Notice the strong winds which have been generated over the lake. Notice also the strong horizontal convergence along the southern shoreline. Associated with this convergence line is a region of upward motions. This is shown in figure 27, which shows the vertical motion field at about 1 km above the Earth's surface. On the basis of these diagrams, one concludes that the lake could strongly modify the prevailing synoptic flow pattern during thermally unstable conditions. It is, therefore, impossible to determine accurately the boundary layer processes over the lake under these conditions by considering only the undisturbed synoptic-flow pattern. One has to take into account the fact that the lake can strongly distort the synoptic flow pattern, thereby producing a mesoscale disturbance whose boundary layer is different from that inferred from the undisturbed synoptic condition.

The problem can be summarized with the aid of figure 28. We envision the problem to be prediction of the boundary layer processes from the specified undisturbed synoptic-flow, the lake surface conditions, and the surrounding land-surface conditions. The crudest solution would be to consider only the undisturbed synoptic flow without taking into account the mesoscale distortions induced by the lake and to use empirical-physical techniques. In figure 28, this method of solution can be indicated schematically by arrows which proceed from the given boxes (synoptic flow, lake surface conditions, land surface conditions) to the predicted box (boundary layer processes) through lines 1, 2, 3, 4, and 11. In this case the feedback loop is not considered; i.e., lines 9 and 10 are disconnected from the lake and land surface conditions. The ideal solution should consider the feedback loop. As indicated in the preceding paragraph, the surface fluxes produce a modification of the atmosphere that results in a mesoscale disturbance. The associated boundary layer of the mesoscale flow may, in turn, produce changes in the surface lake and land conditions, thus altering further the original boundary-layer processes. This complicated chain of events can be taken into account only by incorporating the feedback loop. This implies that lines 5, 6, 7, 8, 9, and 10 (fig. 28) should be taken into account.

It is appropriate to assess the current state of knowledge of the physical processes which are required for the prediction of a boundary layer. In figure 28, these processes are those which are involved in empirical-physical models (line 4) and the mesoscale-physical models (line 5). Let us consider first the current state of knowledge concerning the empirical-physical determination of turbulent fluxes. A common method for doing this is the so-called bulk aerodynamic method which requires the use of drag coefficients. The magnitudes of these coefficients are not accurately known. And one of the important

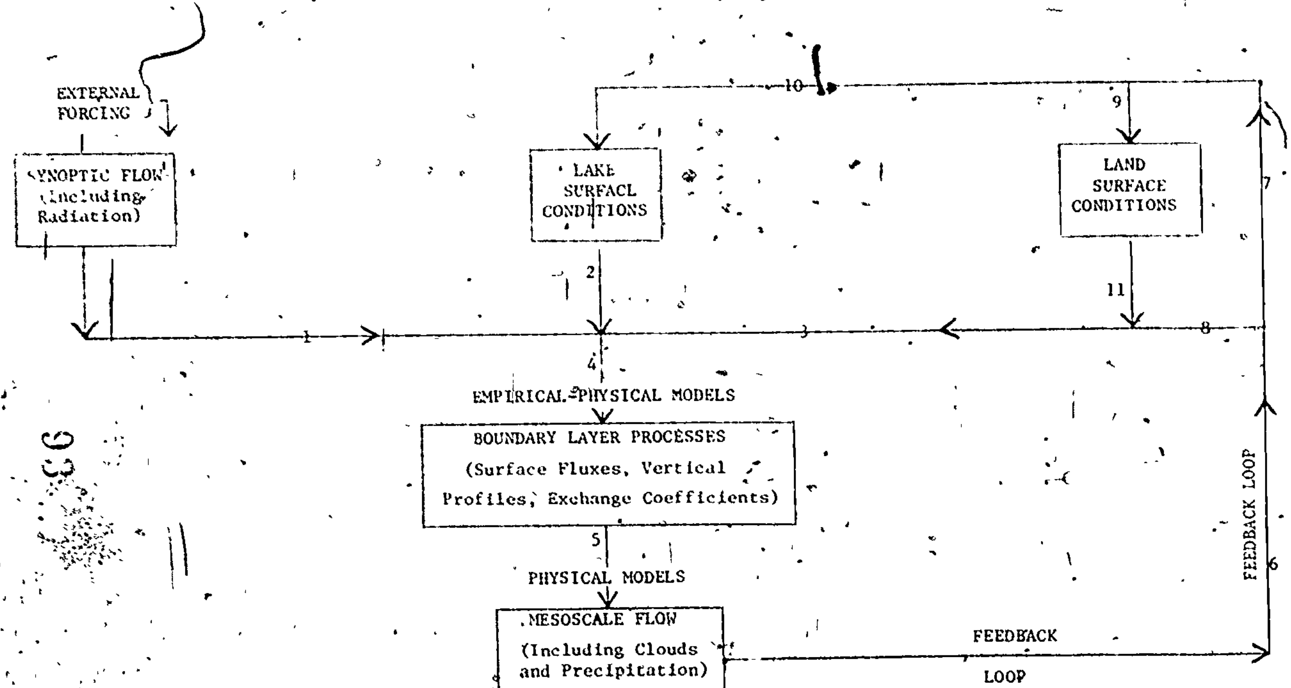

Figure 28. Schematic flow diagram of the boundary-layer prediction problem.

studies under IFYGL is to determine the values of these coefficients under various meteorological conditions over Lake Ontario. One can get an indication of the current state of knowledge of these coefficients by examining figure 29. This diagram indicates the accuracy of determining the turbulent moisture flux by using a drag coefficient. An approximate value of the coefficient would be 1.23×10^{-3}. However, such a value would seriously underestimate the turbulent flux at large values of $\bar{U}\Delta q$. The corresponding accuracy of determining the turbulent heat flux is indicated in figure 30. Again, large errors in determining the heat flux with the aid of a constant value of the drag coefficient are expected. In addition to empirical relationships between turbulent fluxes and the mean flow, one can also establish empirical relationships between other boundary layer quantities. An example of such a relationship is between the Richardson number and the bulk Richardson number (fig. 31). The Richardson number is often used as a parameter for determining empirically turbulent fluxes. In concluding the discussion of empirical-physical relationships between turbulent fluxes and the mean flow, one can say that current relationships are rather reasonable. However, in order to predict the boundary layer processes more accurately, one should formulate more accurate relationships. The current relationships are erroneous under highly unstable thermal stratification and strong winds. More research must be done in order to formulate satisfactory relationships under these extreme conditions. Hopefully, investigations under IFYGL might provide improved relationships.

We discuss next the current state of knowledge concerning the physical modeling of mesoscale flow (line 5 of fig. 28). This is normally done numerically with the so-called primitive equations. What are the current weaknesses in physical models of mesoscale flow? The most serious weakness is the description of the effects of subgrid-scale eddies in terms of grid-scale quantities. For practical purposes, the minimum grid distance which can be used for numerical integrations of mesoscale model equations is probably on the order of 10 km. Therefore, the effects of eddies smaller than 10 km in scale should be understood. Another weakness in the modeling of mesoscale flow is the incorporation of terrain effects, variations in elevation, and roughness. The description of the latter is especially difficult because it involves the effects of trees, buildings, and similar inhomogeneities of the Earth's surface. Finally, there are weaknesses related to the formulation of lateral boundary conditions and initial conditions.

To summarize the important points in predicting the boundary layer over lakes, we need first to specify the synoptic-scale flow. The synoptic-scale flow is predicted on an operational basis by the National Weather Service. The accuracy of the prediction is reasonably accurate. We can, therefore, assume that the specification of the synoptic flow is not an important obstacle in the boundary-layer prediction problem. The interaction between the synoptic flow and the lake is also an important factor which should be taken into account.

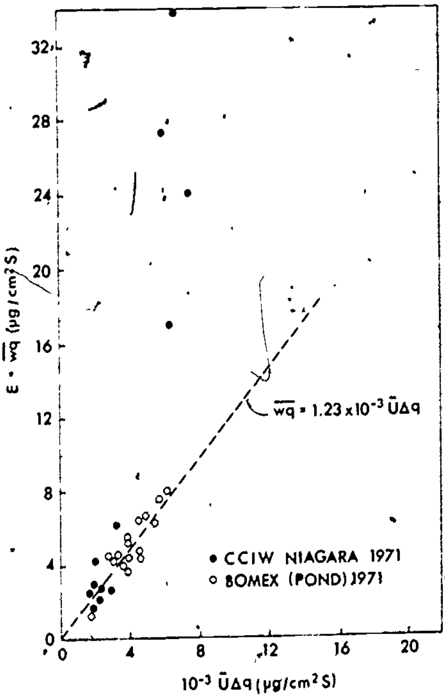

Figure 23. Relationships between evaporation and large-scale parameters (from Elder).*

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* Elder, F. C. (1973), Some results of direct measurement of Bowen ratio over an open lake surface, presented at the 16th Conference on Great Lakes Research, International Association for Great Lakes Research, Sandusky, Ohio.

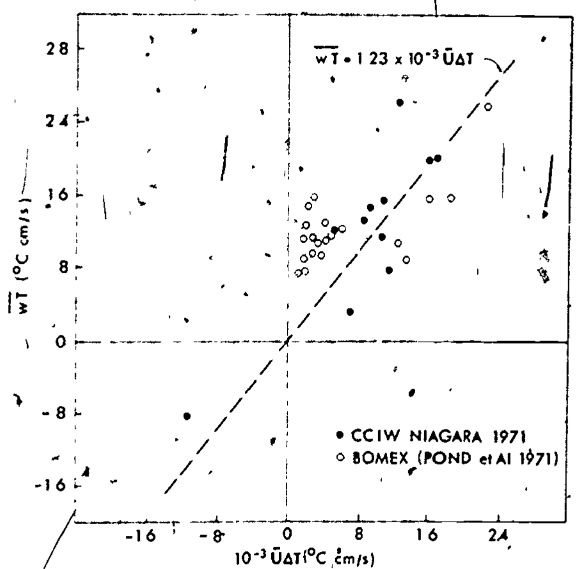

Figure 33. Relationships between sensible heat flux and large-scale parameters (from Elder, 1973).*

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* Elder, F. C. (1973), Some results of direct measurement of Bowen ratio over an open lake surface, presented at the 16th Conference on Great Lakes Research, International Association for Great Lakes Research, Sandusky, Ohio.

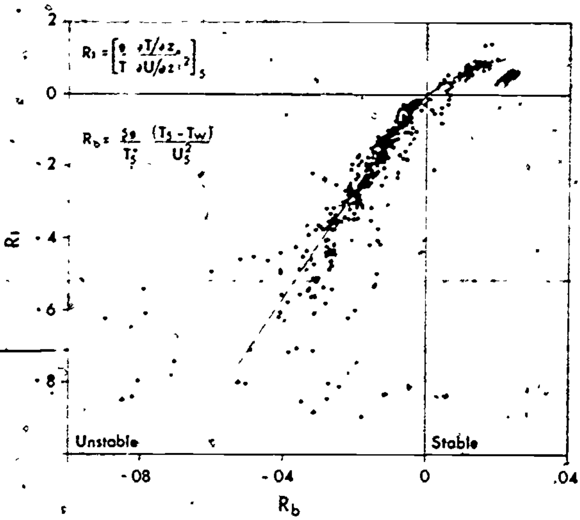

Figure [illegible]. Relationship between the Richardson number and [illegible]

for the lake distorts the synoptic flow, and the distortion produces a boundary layer over the lake which could be very different from that deduced purely from the undistorted synoptic-scale flow. Finally, the deficiencies which have to be overcome in order to achieve an accurate prediction of the boundary layer flow are as follows:

(1) The specification of turbulent fluxes and other boundary processes in terms of the mean flow, especially under highly unstable (thermally) and strong wind conditions.

(2) The specification of subgrid-scale mixing processes.

(3) The incorporation of varying terrain elevation and roughness.

(4) The formulation of lateral boundary and initial conditions.

In addition to the above items, there are certain observational deficiencies which have to be remedied. Observational data are needed in formulating empirical flux relationships as well as in testing models. IFYGL may provide adequate observational data over Lake Ontario; however, over the surrounding land areas, we may not have adequate data.

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* Donelan, M. (1974), Over water atmospheric boundary layer profiles under various conditions of wind speed and stability, presented at the International Association of Meteorology and Atmospheric Physics First Special Assembly, Melbourne, Australia, January 14-25, 1974.

1.9.1 Response - D. D. Hougton

I will expand considerations for the lake-atmospheric boundary layer processes beyond that discussed by Dr. Estoque. Dr. Aubert referred to enlarging the concept of environmental dynamics to include the total environment. I would propose the same be done here for the boundary layer. Firstly, we need to consider the planetary boundary layer over entire watershed areas, such as was shown on the map for the Lake Ontario study, instead of just over the lake. Secondly, attention needs to be given to a layer much deeper than the surface boundary layer. Dr. Estoque alluded to this deeper layer, but referred explicitly to the 10-m surface layer only when discussing fluxes. With these enlargements, the important man-lake interactions can be studied, particularly with respect to air pollution. Man's environment must include the air we breath as well as the water we drink.

An important point that Dr. Estoque made was that the boundary layer-lake interaction was not all one way. Many oceanographers and limnologists deal with the atmosphere as the forcing function for the water. But, for the atmospheric boundary layer, it can also be the other way around. Atmospheric temperature gradients and changing static stabilities due to water temperature can be important factors in determining the boundary layer mixing process. If we consider air pollution, the lakes may act as an active sink.

The important thing to note is that we are talking about a turbulence phenomenon for nonhomogeneous situations. If the atmosphere near the lake edge is examined, local circulations and other inhomogeneities are common, causing important deviations from homogeneous and isotropic turbulent mixing. Perhaps the adjustments in the lowest 10 m are relatively rapid and non-homogeneous conditions can be handled locally. For the layer from 10 m to 2 km, it is not so clear how the boundary layer responses can be handled. Therefore, I would encourage further effort to get turbulence and flux data for the layer from 10 m to 2 km so that better studies can be made. Experience gained in the recently completed Global Atmospheric Research Program Atlantic Tropical Experiment suggests that a tethered balloon system might be sufficient to provide actual heat and moisture flux determinations under various synoptic conditions.

Dr. Estoque indicated that we already know vertical fluxes to within one order of magnitude, based on simple turbulence models, and that this matter is under control. I feel that one order of magnitude is not good enough; particularly for cases of extreme instability, a more accurate determination of magnitude is needed.

As mentioned earlier, the Lakes are a sink of atmospheric air pollution. This makes it important to determine the rate at which the atmosphere is bringing the pollution to the surface of the water. This needs to be explored with emphasis on the near lake-edge areas. Sometimes the lake-edge circulation

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becomes closed so that pollution advected away from a city may be recycled back to the area, leading to enhanced pollution danger.

Finally, with reference to scales, Great Lakes boundary-layer effects involve mesoscale phenomena with a depth on the order of several kilometers. Add to this a significant diurnal variability, and we are in a scale range where the dynamics and interactions are poorly understood. We need a much better data base for proper study of these processes. I endorse Ted Green's concept of having a local concentrated observing setup nested within a broader scale network. This will provide needed mesoscale data.

1.9.2 Response - J. Holland

I will enumerate a few things that I think, as a result of our experience in IFYGL, still need to be done on the Great Lakes. IFYGL did not properly cover the stable season in the spring when the lake is cooler than the air. The instruments were not installed early enough, they were not working well enough, and the vertical resolution and vertical coverage were not adequate, i.e., the airplanes did not fly low enough, the towers were not high enough, and we had no tethered balloons so the interval from about 10 to 100 m above the lake where a big transition occurs between lake surface conditions and the free atmosphere was not observed. We had some evidence of negative fluxes of moisture and heat, but this is an important case theoretically and dynamically and from the standpoint of pollution (although it contributes little to the lake-air transport of heat and water vapor because they are very near zero during that season) because this is one that needs more work. It should not be done soon. We need adequate platforms and instrumentation for tackling this problem before a new experiment is done.

We had a peculiar year, as every year is peculiar. One of the peculiar things about the year of IFYGL was Hurricane Agnes. Because the June-July period was so severely perturbed by that, there is some question as to how much we can generalize any of the results obtained in IFYGL during that time of the year to other years; obviously, we need data in other years.

We certainly should get much farther along in understanding what we learned from IFYGL before we finalize the design of a program to check the applicability of IFYGL results under other large-scale or seasonal conditions. Similarly, Lake Ontario is a peculiar lake, but we called this the field year for the Great Lakes. It was the field year for Lake Ontario and for generalization of IFYGL results to the other lakes; it will again be necessary to test these results on the other lakes, although maybe not with a project the magnitude of IFYGL. But certainly when the IFYGL data have been analysed to the point where we can say what we learned from IFYGL, we should anticipate what will be found in the other lakes based on generalization of the IFYGL results. These should then be tested by suitable observations on other lakes, maybe not all the other lakes, but certainly lakes which are different in important respects. Lake Michigan, for example, is elongated in the meridional direction instead of the zonal direction; therefore gradients along the axis of that lake perhaps cannot be as readily neglected as they can be in Lake Ontario.

We need information on the nearshore atmospheric gradients. Nearshore limnological gradients have been a matter of very great interest and have led to important discoveries in IFYGL, but we had poor coverage on the atmosphere in the nearshore region and the shore region. We had good coverage on homogeneous instrumentation nicely exposed all over the lake. On the shoreline, we had few stations. There was no attempt to standardize the exposure of the instruments.

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Each one had some peculiar local effects. There were not enough to establish the boundary conditions of the lake and the interplay of the meteorology between the lake and the land. What happens in the nearshore region in the atmosphere is still a mystery as we try to analyze the IFYGL meteorological data. This is a subject that needs more work, and again it needs thought and suitable instrumentation and platforms before extensive field work is done.

The Center for Experiment Design and Data Analysis in the Environmental Data Service of NOAA will be working with IFYGL data to get mean values and ~~confidence~~ limits on some of the exchange coefficients and to determine whether some of the nonlinearities of the coefficients can be found. The evaporation graph that Mariano Estoque showed suggests the possibility that high evaporation rates ~~must have a bigger coefficient~~. Also, one of the previous speakers showed a graph in which a drag coefficient of 3×10^{-3} was used, and limnologists tend to use numbers like this. Meteorologists use numbers more like 1×10^{-3}, and we do not know the effects of the intermittency of these phenomena on the mean values of these coefficients. One of the things we learned in IFYGL was about the intermittency of the fluxes. We knew that most of the energy flux from the lake to the air occurs in the fall season and that most of that occurs in a few episodes of a few days each, and we found in IFYGL that, within those few days, most of it occurs in a few hours. We also found that it occurs in a small percentage of the lake area. When a big cold outbreak occurs over the lake, it turns the lake over and pushes all the warm water over to the downwind end of the lake. Essentially all of this flux is taking place in a short time in a small space in the lake. What this means in terms of exchange coefficients in Estoque's model is that, in addition to the mesoscale perturbation of the atmosphere, the perturbation of the lake also is going to effect these exchanges. We may or may not be able to learn from IFYGL what we need to know about these nonlinearities, these intermittencies, in order to evaluate the feasibility of modeling means over months, seasons, or years, or whether these have to be built up statistically from the probability distribution of the widely differing states that occur.

Another thing that will interfere with mesoscale modeling is the possible importance of small mesoscale or large microscale, that is, the kilometer-scale structure that may occur. We know from satellite pictures that lines, clouds, and streaks tend to occur and that these things have varying widths and intensities, so this is going to make some difference in the exchange coefficients that are used. There is apparently structure on all scales, and IFYGL was not able to cope with the observational requirements of this kilometer scale. It was very good for lake scale and maybe down to one-third lake-scale, but we did not instrument to 5-percent lake-scale in IFYGL. There is systematic behavior on this scale, which is essentially the scale of the nearshore transition zone.

These are areas for further work which I propose would need a high priority as a result of deficiencies in the IFYGL program.

1.10 SIMULATION OF GREAT LAKES WATER LEVELS AND FLOWS IN CONNECTING CHANNELS - D. D. Meredith

The Great Lakes are the earth's greatest expanse of fresh water. Due to the extensive demographic and industrial development of the Great Lakes region, the hydrologic conditions of the lakes influence the economy and growth for a major region of both the United States and Canada. The water levels of the Lakes and the flows in the connecting channels influence many of the activities on and around the Lakes. Commercial navigation requires certain minimum lake levels and flows in the connecting channels to provide the necessary minimum draft. Hydroelectric power generation requires minimum flows in the channels to maintain power capacity. Shoreline property may sustain inundation and erosion damage from direct flooding during high water periods. In order to achieve the maximum benefit from the Great Lakes, they must be managed in the most efficient way to achieve the objectives of those who enjoy their use.

To manage the Great Lakes water levels and flows in connecting channels, the inflows to the Lakes or the outflows from the Lakes or both inflows and outflows must be controlled. In order to control the lake levels and flows in connecting channels, we must have a regulation policy which incorporates the basin's hydrology, regulatory works, and political and management issues.

The purpose here is to present a brief review of the basin hydrology and the procedures used to determine optimal regulation plans.

From the conservation of matter principle, a water balance equation can be written for each lake as follows:

$$\Delta S = P + R - E + I - O \pm D \pm G,$$

where ΔS is the change in amount of water stored in the lake, P is precipitation on the lake surface, R is runoff into the lake from the surrounding land area, E is evaporation from the lake surface, I is inflow from the upstream lake, O is outflow from the lake through its natural outlet, D is diversion into (+) or out of (-) the lake, and G is ground water flow entering (+) or leaving (-) the lake. All variables are expressed in the same units and for the same period of time. Obviously, any variable may be equal to zero for a lake where it is not pertinent. The change in amount of water stored in the lake, ΔS, is a positive amount when supplies exceed removals and is a negative amount when removals exceed supplies.

The Tides and Water Levels Section, Marine Sciences Branch, Canada Department of Energy, Mines, and Resources; National Ocean Survey, NOAA, U.S. Department of Commerce; and Detroit District, U.S. Army Corps of Engineers, maintain water level gages on the Great Lakes, rivers which connect the Lakes, and channels in which water is diverted into or out of the Lakes. Change in amount of water stored in a lake is calculated from the area of the lake and the measured change in the elevation of the water surface over a period of time. The amount of inflow from the upstream lake, outflow from the lake through its natural outlet, and diversions into and out of the lake are determined from the water level records and rating curves which give the relationship between the amount of flow past a point and the surface elevation of the water at that point.

Precipitation, evaporation, runoff, and ground water terms in the equation above are sometimes combined into a single term which is called the net basin supply (NBS) to the lake. The water balance equation can then be written as follows:

$$\Delta S = NBS + I - O \pm D.$$

The value of the NBS term in the equation can be determined as the sum of the precipitation, evaporation, runoff, and ground water contributions to the lake, or it can be determined as the residual after the value of the other terms have been determined.

There have been numerous studies of the hydrology of the Great Lakes and their subbasins. Buetikofer and Meredith (1972) prepared an annotated bibliography of studies made prior to 1972. The latest description of the hydrology and hydraulics of the Great Lakes system, including a discussion of factors which affect the water supply and the response of the system to its supply, was prepared by the International Great Lakes Levels Board (1973). This study, as with most of the studies on Great Lakes regulation, is oriented toward use of the NBS as the hydrologic input to each lake. Each historical NBS value was computed as the residual after the value of the other terms in the latter equation above had been determined. In addition to the historical studies, attempts have been made to develop models to forecast the NBS for use in regulation of the Lakes. These studies are summarized by Meredith (1970) and the International Great Lakes Levels Board (1973).

Jones and Meredith (1972) determined monthly values for precipitation on each lake, evaporation from each lake surface, and runoff into each lake from surrounding land areas for the calendar years 1946 through 1965. The former equation is not satisfied when the precipitation, evaporation, runoff, river flow, and change in storage values are substituted into it (Jones and Meredith, 1972). This indicates that either ground water, about which we know very little in the Great Lakes, should be considered, or there is some other explanation for this discrepancy.

The consideration of the thermal expansion of water would change the values of the ΔS in the former equation and would have the effect of decreasing ΔS for months when the temperature is increasing and increasing ΔS when the temperature is decreasing. Recent results indicate that, for some months of the year, the temperature effects on lake levels are on the same order of magnitude as the NBS of the lake (Meredith, 1975a). A step-wise multiple regression analysis indicates an apparent influence of upstream lakes on the precipitation in downstream basins (Meredith, 1975b). For example, a statistically significant relationship was derived which indicates that the precipitation in the

Lake Erie Basin during June is a function of the evaporation from Lake Superior in May and June (Meredith, 1975b).

Little data exist concerning ground water contributions to the Great Lakes. The usual assumption is that it is insignificant and can be ignored (International Great Lakes Levels Board, 1973). About the only data are from the work by Haefeli (1972) concerning the northern shore of Lake Ontario. Ground water contributions may become important in terms of water quality, especially if there is the practice of disposing of wastes into the ground water.

Another component that will come into play a little more in terms of lake levels is the increase in consumptive use of water around the Great Lakes. Increase in the amount of water that is taken out of the Great Lakes and not put back in will have a long-term gradual effect on the levels. This effect will probably be minor when compared to other factors affecting lake levels.

The development of a regulation plan is dependent upon the data used and the procedure used to formulate the plan. The International Great Lakes Levels Board study (1973) used the monthly NBS values for the period from January 1900 to December 1967 as the "study period" for the regulation study. The NBS values were determined as residuals after the other terms in the latter equation were determined. These historical NBS values were used to develop operational regulation plans. Additional testing of the regulation plan was conducted, using 68 years of data generated by a multivariate model.

The river flows used were those values developed by the Coordinating Committee on Great Lakes Basin Hydraulic and Hydrologic Data (International Great Lakes Levels Board, 1973).

The current regulation plans in operation on Lake Superior and Lake Ontario were based on hindsight. A regulation plan which would benefit one or more interests was established somewhat arbitrarily, and the effects determined by computing the resulting levels and outflows that would occur with this regulation plan, if the historical sequence of NBS values were to occur again. If the regulation plan did not satisfy the objectives of criteria for regulation over the critical period, adjustments were made to the regulation plan and the adjusted regulation plan was tested. This process was repeated until a regulation plan satisfied the objectives or criteria over the critical period (International Great Lakes Levels Board, 1973).

The International Great Lakes Levels Board (1973) used dynamic programming and a successive approximation technique to develop trial regulation plans using the January 1900 through December 1967 historical sequence of NBS values. These trial regulation plans were then tested by using synthetic sequences generated by multivariate models.

Neither the current regulation plans nor the best of the trial plans were able to satisfy the criteria during a test run using the critical period of 1968 through 1973, during which time the Great Lakes Basin received extremely large amounts of precipitation (International Great Lakes Levels Board, 1973).

Even the use of synthetic sequences in the testing of trial plans does not indicate how the Great Lakes system will respond to the plans under more extreme conditions. Most attempts to generate synthetic sequences of flow variables are aimed at preserving the statistics which are used to define the historical sequence. If the historical sequence of values does not contain the most extreme events that can occur, then any synthetically generated sequence will most likely not contain the extreme events either because the multivariate model is designed to preserve the characteristics of the historical sequence. I know of no case in which a synthetically generated sequence of events contained a critical period which was more extreme than the critical period of the historical sequence.

The International Great Lakes Levels Board (1973) used a deterministic approach to optimization. The optimization was performed by using a particular sequence of flows; whether that sequence of flows was the historical sequence or a synthetically generated sequence does not matter. Lake Superior regulation plans have been developed by using a nondeterministic approach to optimization (Su, 1971; Su and Deininger, 1974). The inflows are treated as stochastic random variables, and this stochastic nature is incorporated directly into the optimization technique. However, this technique results in excessive computation times when applied to the entire Great Lakes system. A greatly simplified example for a four reservoir case required 161 minutes of computation time (Su, 1971).

Morris (1974) proposed a modeling procedure for utilizing all the relevant information in a multiobjective decision-making scheme to develop an optimum operating policy of the Great Lakes system. A multiyear linear-screening model is postulated to provide an initial regulation plan, and a simulation model is to be used to evaluate proposed alternatives.

Other studies for determining optimal operating rules for multiple-purpose, multireservoir systems might also be readily transferable to the Great Lakes system. One such approach is a two-dimensional dynamic programming approach (Rood, 1974).

Much of the concern with flows in the connecting channels has been with determining the effects of ice retardation or with flow conditions at regulatory works. The ice retardation problem has been analyzed, using a hydrologic response model with stage-fall discharge relations, rather than using the hydraulic routing techniques to determine the flow conditions (International Great Lakes Levels Board, 1973; Quinn, 1971, 1973).

To sum up, Great Lakes hydrology has been briefly discussed and sources of more complete information indicated. Procedures for determining plans for regulation of lake levels due to changes in lake water volume have been described, but changes in lake levels due to waves, tides, wind, and pressure cannot be controlled by regulation.

The Great Lakes system is subject to natural regulation. The approaches and techniques used in the past have resulted in regulation plans which provide for more efficient use of the Great Lakes than if there were no regulation. However, there needs to be further studies on lake regulation plans. There are powerful optimization techniques which could be used on this problem. There must be some procedure devised to allow for testing of regulation plans for extreme conditions which are worse than have ever occurred in the past.

We are just beginning to understand Great Lakes hydrology. We are just beginning to approach the problem from other than a lumped parameter model. The application of conceptual models in the study of Great Lakes hydrology will be another improvement in our knowledge and understanding.

The decision processes of any regulation plan require some knowledge or assumption of future water supplies to a lake. Forecasts of weather would enable the extension of hydrologic forecasts. Current skill in forecasting weather and related phenomena can only be measured in terms of a few days. The International Great Lakes Levels Board (1973) reports that, with 4-month perfect forecasts, benefits on the Great Lakes can be increased by one-third.

We have just begun; there is much to do.

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Appendix A--Hydrology and hydraulics
Appendix B--Lake regulation
Appendix C--Shore property
Appendix D--Fish, wildlife and recreation
Appendix E--Commercial navigation
Appendix F--Power
Appendix G--Regulatory works

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1.10.1 Response - F. Quinn

On the overall aspects of lake hydrology, a series of models relating to the water quantity are available. They are basically mass continuity models where the flows are routed through the system. The model inputs consist of evaporation, precipitation, and runoff, either individually or considered as the lumped term, NBS, into the lake, that Dale Meredith mentioned. Also, the hydrologic response models that are now being used encompass the regulation plans for Lakes Superior and Ontario. As many of you are probably aware, both of these lakes are completely regulated by man. The middle lakes in the system, Michigan, Huron, St. Clair, and Erie, are all governed by their natural responses. Therefore models for the system consist of the operational regulation plans for Superior and Ontario and the natural response models for the rest of the system. Several areas need a much larger input as Dale Meredith has brought out. One is on the interactions between precipitation and runoff. It has been pretty well documented, just by looking at time-series relationships between precipitation and lake levels that the lake levels lag the precipitation by about 2 years. This is a result of the precipitation-ground interaction in which the basin reacts similarly to a sponge. However, there is a problem of quantifying this and using it as a type of predictive model to determine the inputs into the hydrologic response models. In addition, as has been mentioned in most of these hydrologic studies in the Great Lakes area, ground water has been completely neglected. The reason for this is that no one has any ideas as to what its contribution is. A study is going on now in relation to IFYGL which may give some insight into this problem.

One of the things that should be mentioned about the hydraulic models is that they also serve as a major input in water quality models. If you want to know how much of a constituent is coming into one lake from another on a volumetric basis, you have to know how much water is coming through that system, where that water is coming from, and where the water is going. This is something that the hydrologic models provide. In addition, there are hydraulic transit models and steady-state models of the connecting channels. At the current time, I have models for both the Detroit and the St. Clair Rivers. These models have as inputs the upstream and downstream hydrographs; for example, for the case of the Detroit River, hydrographs for lakes St. Clair and Erie are used. With these as the forcing functions, river flows can be computed on an hourly basis at about four or five sections in the Detroit River. The importance of this stage-discharge relationship has been brought out earlier. Wind tide and seiches on Lake Erie can cause flow variations coming into the lake of between 2,200 and 9,100 m^3 D^{-1} of water. This type of variation can occur over a 12- to 14-hour period. If what is happening biologically or chemically is being monitored during any of this time, it becomes necessary to know how much water is coming in at any particular time. We have used models in several studies: One study for the Great Lakes Regional Office of the International

Joint Commission used the Detroit River model in conjunction with its chloride data to compute loadings and to determine the effect of river flow variation on loading and on the representativeness of the sampling as reflected in Detroit River loading.

Looking toward the future, one of the prerequisites for a better hydraulic model will be more accurate discharge information. It comes back to the same thing, a model was formulated using the equations of continuity and motion, and now we want to calibrate that model. But, to date, all we have is discharge information which was made basically at one point during a limited time and usually many years ago. What we need to do is to devise a measuring system and go out and provide additional and better discharge measurements for the river models. To illustrate the importance of the river model, for Lake Erie approximately 70 percent of the water which comes into the lake enters via the Detroit River, and about 70 to 80 percent of the water that leave the lake discharges through the Niagara River. Therefore you can see that slight errors in the computation of the river flows can create considerable error in the water balance and consequently in the chemical models.

Looking toward the big picture many years in the future, I can see, and this is a suggestion which has been brought up several times by Dr. L. Bajorunas, that one of the important aspects of the ecology of the Great Lakes may be in terms of water quality regulation. The water quality of the Lakes varies with time, and during various times of the year pollutants may be in different areas of the Lakes. Therefore all the models which are being derived, those in the biological and chemical realm, those in lake circulation, and those in the hydrologic realm, must be combined into operational models using operations research to regulate the Great Lakes. The long-range view of what might conceivably come to pass indicates that all these models may be amalgamated into a large scheme which will provide, in addition to the current lake-level regulation, perhaps a more important regulation as far as the future of the Great Lakes in terms of water quality.

2. WORKSHOP DISCUSSION SESSION

2.1 Guidelines for Work Groups - E. J. Aubert

First of all, I will comment on the work group makeup. Each of these groups has a chairman, a scientific secretary, and various members. I have already mentioned that some movement between working groups is desirable. Too much motion is probably going to be chaotic; exactly how the line is drawn, I leave to you. The handout material (Appendix 1) contains a sheet entitled Workshop Group Membership.

In my introduction this morning, I mentioned five purposes of the workshop that I consider relevant. Whether these are all compatible in one workshop, I am not sure. The first objective is to identify future Great Lakes environmental research initiatives, i.e., major programs required to provide a satisfactory state-of-the-art in environmental simulation and prediction to support the decision process for Great Lakes activities. This includes prediction, simulation, and those experimental studies necessary to support the modeling effort as well as the environmental description

The second objective is to provide an opportunity to the Great Lakes research community to discuss and recommend future Great Lakes environmental research initiatives.

Third is to consider possible United States-Canadian joint research initiatives.

Fourth is to identify logical follow-ons to IFYGL.

Fifth is to explore the priority environmental research needs of NOAA, viewing NOAA operating units as users of environmental information. They actually are producers, but you can look at it from the point of view of developing research products to backup the operating environmental units. The National Weather Service is one of the major operating units of NOAA, but it is not the only one that was considered. Sea Grant and Coastal Zone Management are others. Amor Lane, one of the NOAA representatives, could comment on this point. He has responsibilities at a program management level in Sea Grant, Coastal Zone Management, and Project Independence, which, translated into everyday language, means energy-environment problems. This is a Department of Commerce activity.

The purpose of our plenary session was to set the perspective for work group sessions, and it is perhaps clear that not all of the topics that are relevant to some of these work groups have been covered, either by the principal speakers, or by the responders. This gap was brought out clearly relative to an operational problem with surface wave prediction, which has not been mentioned. The National Weather Service considers wave prediction to be an important forecast problem, and I hope that one of the work groups will discuss this. In fact, since National Weather Service people are here, I hope they bring up such problems. I hope it gets discussed from the point of view of the state-of-the-art and what future research is meaningful. Wave prediction could be included in one or two workshop sessions. It could be in the Water

Movements group, which is where it was in IFYGL. Clearly, the action of waves is not only related to the stress on the lake but also to the condition of the lake. So one must know something about the lake thermal structure in order to know how the surface stress is going to affect waves. Another aspect of surface waves concerns the scope of the boundary layer. As I would view the boundary layer, it includes not only the atmosphere down to and including the top skin of the lake but also the upper layer of the lake. As Mariano Estoque and others pointed out, the stress is very much dependent upon the stability conditions. The effect that a certain synoptic situation has on the surface waves is very much dependent upon the lake-atmosphere boundary layer and the situation it is in relative to a synoptic weather situation. Surface waves as a topic fall between the Water Movements and Boundary Layer work groups. If time can be found to get together, it would be appropriate to consider this jointly. I suggest that the co-chairmen meet on this topic tomorrow morning for an hour in joint session. Is that reasonable? Do you have a suggestion?

Baer. There are two or three other things that I think go along with waves, for example, storm surges, which present serious practical problems. Also, I have not heard ice mentioned. I presume that most of the ice is caused by cold atmosphere. I just wonder if all of that could not go in one session.

Csanady. Yes. But you see from experience with past planning sessions in IFYGL that, unless you have somebody representing some of these constituents in the work group, there is not going to be much significant discussion because we do not have the competence or the interest to go into these things. There might be more interest in the atmospheric group in waves, but even then I would suggest you attack this separately. Waves and the littoral zone transport, coastal erosion, and so on, seem to hang together and require the calling of another group whose prime interest is in this field and who could make a useful contribution. We could then either say yes or ignore it. This is what happened last time.

Aubert. What you all say makes sense. My point is that the work group sessions should not be restricted by the scope presented in the plenary sessions. However, the work group sessions will be limited by the interests and capabilities of the people attending. Unless these topics are raised, they will not be discussed. Since there are more people in the work group sessions than there are people who have made presentations at the plenary session, it will be up to this broader membership to bring up these other topics. How well these additional topics get discussed is not known. They may be inadequately covered.

Baer. Could you put it all some place so the interested people would know where to go instead of picking up a little bit all over the place?

Aubert. We mentioned a few topics, but there are more. You cannot separate them all. I do not think you have a reasonable question. I cannot leave this without mentioning something I saw in print from Will Pearson; it must have been about 10 years ago, but is relevant to this mix of purposes for this

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workshop. I do not know which people in the marine environment try to cross marine animals to generate new species, but this guy was trying to cross an abalone with a crocodile. Somebody put this together and came out with an abadile. Somebody else put it together and came out with a crocobalone. Maybe these purposes are not completely compatible. Be that as it may, we would like to achieve the maximum possible from the competence that we have gathered. The plan is, then, that we will reconvene in our working group meetings at 8:00 p.m. and again in the morning. We will reconvene here in plenary session after lunch. Are there any questions?

Csanady. What do you expect in the plenary session? A detailed presentation of what we want to do in 1977?

Aubert. Not what you want to do, but what you think are priority research problems. Also something about how you may go about it if you have done that much discussing. Problems, I think, ought to be defined with some thought to their relative importance within the scope of your discussions. While the questions "What have we learned? Where to go from here?" only appeared in the Prof. Mortimer introductory plenary session this morning, they apply to all of these topics and not just IFYGL. What have we learned from all of the research on the Great Lakes? People who are not IFYGL participants have been invited to this workshop, and many of you have pursued other Great Lakes research, too. From what you understand the need is--and we do have some people here who ought to speak up on need, at least from these NOAA groups--what are the important research problems that ought to be attacked? Include something about how, but you are not going to develop a research plan during the work group sessions. Ideally, we will come out with the proper research questions to be asked and, ideally, some of the objectives one might want to pursue.

Holland. This is not for a FY 1977 field program? Looking over time, what should be done during FY 1977? This is for field work that might be in 1980, or analysis of data collected in 1981 or whatever.

Aubert. I am looking at it from the point of view of something new. I would see IFYGL, as a formal program, terminating in 2 to 3 years. That is as far as IFYGL goes in the budget process. It will no longer be a line item. Whether there are future Great Lakes research initiatives involve a lot of decisions that go higher up than this Laboratory--the budget channel and that sort of thing.

Holland. It could be the start of a 5-year program? It does not have to be a 1-year program carried out in 1977?

Aubert. No, I would view this as a major effort. It could be a 5-year effort. If there are research objectives of importance, they should be identified. I do not view the objective of this workshop as telling our Laboratory in detail what we ought to be doing in the various projects we are now working on. We have an on-going program. There are other considerations that will go into that. I am asking you to identify a logical follow-on to IFYGL, if there is one.

Mortimer. Can you say something about the interagency arrangements? I believe you had one conference last year. Is there another interagency conference planned for this year? How are these kinds of programs in the EPA and the Atomic Energy Commission going to weld together with the NOAA effort?

Aubert. You are referring to the First Federal Conference on the Great Lakes held here, in Ann Arbor in December 1972. This was sponsored by the Interagency Committee on Marine Science and Engineering (ICMSE). Dr. Robert White is the chairman, and each of the major agencies or departments has a representative on ICMSE pertaining to the marine environment in the Great Lakes. A second conference is now in the early planning stage.

Mortimer. The scale of Great Lakes research now calls for interagency programs. Even IFYGL was funded by multiple agencies. The biological work was funded by EPA, and there was fairly considerable National Science Foundation funding.

Aubert. Your question was broad, so I cannot answer it in a few words. A second ICMSE conference is planned.

Mortimer. Should we think about interagency programs or about NOAA only?

Aubert. Clearly, there is overlap in the mission of several of the U.S. Federal agencies pertaining to environmental research.

Mortimer. There are also the principal users. The EPA is a major user for monitoring and enforcement, and the Atomic Energy Commission is the user for power generation and dispersal and fate of radioactive materials.

Aubert. I guess the answer is yes. I will tell you what I plan to do relative to interagency information as a result of this conference. We could not invite more people; obviously the room is full. We did not want to expand the objectives and purpose of this meeting to invite all the other U.S. agencies that have environmental missions and research programs on the Great Lakes because the scope of this workshop would be so broad that we could never get done in a day and a half. But recognizing that suggestions might come out that clearly overlap the other agency missions--namely, fish, pollution, power--I will make the other Federal agencies aware of the results of this meeting, give them a copy of the proceedings, and explore any ideas that might have some interagency merit with them.

Mortimer. You want us to think about science and not politics.

Aubert. I prefer that you do that; and yet consider science from the point of view that it is problem-oriented, not knowledge for the sake of knowledge, but knowledge for better management of the Great Lakes.

Mortimer. Call it strategic research.

Hess. We should not try to design another IFYGL for another couple of years. The major field activity should be more spread out than that.

Aubert. That comment came from a high authority; the situation being what it is, it is better not to design another IFYGL.

Hess. There will be some arrangements about interagency coordination. You are asking us what it is, and I do not think either one of us knows right now. It would be a waste of effort to come up right away with another major field activity like IFYGL. Address yourself to the problems, but do not try to put them all into one big bag to try to solve them like that. Let's have something that is evolutionary.

Aubert. I did not fully answer your question, Prof. Mortimer, but I think it could take half an hour. There is a second Great Lakes conference, sponsored by ICMSE, which is in the preliminary planning stage. The date has not yet been set. The first planning session took place yesterday and Dr. Bajorunas from our lab attended. The Atomic Energy Commission at Argonne, Illinois, has the lead. EPA was the lead agency at the earlier conference. I gather from Dr. Bajorunas that yesterday's meeting did not result in any clear direction of where they were going or when. More planning will be necessary to get that point, but ICMSE has requested that another conference be held. Any other questions? This session is adjourned.

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2.2 RECOMMENDED RESEARCH INITIATIVES

2.2.1 Water Movements - G. Csanady, Chairman

This work group addressed the research initiative problem under "what" and "how." We have several recommendations under each.

Our first recommendation is that we should fully exploit the present data base. This is, of course, something we have already agreed upon, but nevertheless we would like to put on record the strong suggestion for a broad-based exploitation of existing data from IFYGL and earlier Great Lakes studies. By broad-based exploitation, we also mean to include the interrelation of each individual's work with the work of others. This kind of activity is only now beginning to start as data become widely available. We are only recently in a position to take advantage of what other people collected during IFYGL. Having looked at our own data, we should now look at everyone else's to exploit them and make whatever scientific advances we can. Also in this category is the verification of numerical models. Models of physical processes must be tested against existing data in a broad kind of way.

The next point relating to future research is the high priority we place on work on nearshore-offshore exchange processes. A concentrated study of time and space scales of flows nearshore, and specifically of the structure and dynamics of fronts, is required. The interchange of momentum, heat, and pollutants across fronts has relevance to research on the Great Lakes as well as to general oceanography. The effects and parameterization of friction nearshore are also important. Coastal irregularities and their effects on general circulation, the coastal entrapment of materials, and flushing processes around bats, bays, and prominences all come under this heading of nearshore-offshore exchange.

The next recommendation in order of priority is a further study of large-scale long-term lake circulation or, if you like, circulation climatology. Winter circulation is of special interest in this context. Some field data extend into winter, but most do not, and this leaves an important gap in current knowledge.

Our fourth recommendation concerns vertical mixing processes. The surface mixed layer and its interaction with the atmospheric boundary layer, including the overturning periods in the fall and spring, is of considerable practical and scientific interest and relates in an important way to the physics of turbulent friction in a stratified fluid.

Other problems that have been raised here, such as wave studies and forecasting, beach erosion, beach movement, and ice movement, should be considered by a more competent panel.

Turning now to the "how" of the program, one question is: "Is a son-of-IFYGL desirable, and what would be the scientific purpose of such a program?" We agreed that coordination of scientific work would be beneficial. When a number of investigators work together on physical problems relating to the

Great Lakes, generally speaking, more is likely to come out of a coordinated effort than the sum of the parts. There is certainly a favorable point in thinking of a successor to IFYGL. Concerning the logistics of such a program, the concept of again having a core program, with auxiliary programs arranged by individual investigators, is recommended. The core program should provide the necessary background body of data much as it did in IFYGL.

Southern Lake Michigan is a good place from our point of view. It is scientifically interesting, reasonably simple, and accessible. This choice also seems, although we are not the ones to say, to be politically wise.

As for details about how to carry out such a future program, the one point we all agreed upon was the need for long-term, careful planning well in advance of field operations. This is to define clearly the scientific problems to be attacked and to evaluate previous achievements.

We also discussed instrumentation. If we are going to use instruments which are relatively new, they should be tested and used by the people who are going to use them in the field well before a major deployment. In any coordinated program, it is essential to be able to rely on the instruments. In this problem category, we also talked about using whatever technical achievements would be available to us, including satellites, possibly blimps, and any other technological improvements or advances in the state-of-the-art. Although one cannot be too specific at this stage, it is desirable to develop instruments capable of profiling temperature and current velocity. Such instruments have to be developed, and the whole project has to be attacked well in advance of its execution. A number of years are required to develop and test instruments. The coastal chain, as a way of looking at the shore zone, has been very useful and will no doubt be used again, but it has a fair weather bias and other shortcomings. It would be desirable to learn from what we have not been able to do by this technique. To develop new techniques, it is necessary to start planning fairly soon even if field work is done in 1980.

The instrument array in such a core experiment would perhaps be similar to the one suggested by Ted Green. That is, we would probably have a central array somewhere between Chicago and Milwaukee, if we can agree with the other groups that this is a desirable area.

2.2.2 Aquatic Ecology and Water Quality - C. Schelske, Chairman

One of the approaches used by this group was to review what had happened during IFYGL and to discuss what IFYGL provided in the way of understanding aquatic biology. There was a dichotomy in our group between what the aquatic biologists thought was important and what the modelers thought was important. I will try to represent both of those views, but, since some of the modelers were not present at the end of the session, the modeling input may be limited.

What do we feel IFYGL provided. In the way of understanding the biological processes, the IFYGL program was geared more toward understanding climate than understanding weather, if we can apply that analogy. Coordination needed for predictive modeling was lacking, mainly because the biological-chemical design was added to the original program at a late date. More time was needed for interaction and development of programs. Good data were obtained from IFYGL, but we feel the next step should be for models that will predict weather and not climate. Weather, in this case, is the sum of the processes in the nearshore zone, where the frequency of the phenomena is much greater, and the phenomena are more varied than in the open lake. Another reason is that most of the severe management problems are in the nearshore zone. The IFYGL design, particularly from the biological point of view, was fairly well restricted to the offshore waters. The people who worked nearshore has little help from the physical modeling point of view, and that is an essential element for future studies.

What do we do with existing data, IFYGL, as well as other data sets? Five points related to this question were identified.

First is the need to identify different existing data sets. This becomes more and more important with the passage of time. As more and more new investigators come into the system, the need to identify sources of data becomes more critical. Eventually studies that have been done may be lost. There are unpublished results that should be identified and collected in some organized form.

Second is the need to develop a means to make this type of information available to investigators. This is something people could do without actually going out and collecting data, and it might be a profitable way to spend money from the standpoint of government agencies. Many university people have students that would be interested in certain aspects of this problem.

The third point is that funding must be provided for the analysis of these data sets.

Fourth is the need to determine what kind of biological samples are available that have not been analysed and whether they have been stored properly. Some samples have gone to the Smithsonian Sorting Center, but other samples may eventually be discarded. Ideally there should be a museum that would curate samples, or maybe a Federal laboratory with a museum in it that would perform this function.

Fifth is the need for a continuing effort on the problems addressed above. These problems will continue as long as science, so we might as well face them now. It may be significant that Dr. Beeton, who is probably the senior member in our group from the Great Lakes point-of-view, felt that this was very important and everyone agreed with him. One of the reasons for making this point is that sometimes the perspective of experience is needed to realize the importance of factors such as continuing programs.

Regarding new research initiatives, we had trouble with specific research initiatives, but neatly avoided the issue by proposing a title for the new research initiatives. It is "Biological and Chemical Processes As Influenced by Materials Input and Transport in the Nearshore Zone." I will outline this--the where, what, why, and how of these initiatives. "Where" is, of course, defined as the nearshore zone. Specific nearshore zones will be considered later. The next question is why we picked this title. One reason already mentioned is that many practical problems are in the nearshore zone. The scale and frequency of measurements that would be needed would be a function of the specific problem of interest. It is difficult to define research objectives within 1 day. We know there is a gap; in fact, we might say there is a lack of biological and chemical knowledge about the nearshore zone, particularly with regard to modeling. Available predictive models, as I understand it, cannot address certain significant problems in the nearshore area. If that is not correct, will somebody correct us. Therefore, there is need for information of a descriptive nature and data on significant processes in the nearshore zone prior to mounting large-scale modeling efforts. Finally, how do we do this?

First would be the comparison of different nearshore areas either within one lake or between lakes. It is important to find out whether all of the lakes behave similarly or whether differences exist. The study should be designed so the inshore zone could be compared with the offshore zone. Any of you who have ever been to a meeting where the inshore was discussed know questions always arise as to the distance from shore to the offshore waters. These studies should be designed so that offshore stations will definitely represent the open lake and therefore assist in defining the boundaries of the nearshore zone for a particular region. From this program, offshore water within lakes can be compared if more than one nearshore region is sampled, and if not, we can certainly compare offshore waters among the lakes that are selected.

Second, there is a need for coordination among the aquatic ecologists and people working with water dynamics and water circulation.

What will we obtain from this extensive study of the nearshore zone? First of all, there will be descriptive knowledge of the lakes. Prof. Mortimer chided me for possibly minimizing its importance, but I think all biologists realize there is a need for descriptive knowledge and that there is a gap in this area for the Great Lakes. We feel that the descriptive parts, at least, will be a byproduct of the experiments that are undertaken as part of the program. The needed but unspecified experiments will define mechanisms and processes in the environments under study. Although the experiments are not defined, it is apparent that there is need for coordination due to the problems of scale. For instance, to coordinate with people who are studying nearshore water transport, we have to be certain the appropriate data can be obtained for the area under study since water transport is a significant input for any nearshore models as it is needed for materials transport. Our people also felt strongly that we should have actual water transport data that were obtained while ecological data were being collected, rather than a water transport model.

Our specific experimental design is very general, but this is to be expected at such an early stage of a long-term program. We have proposed a 5-year plan. 1 year for planning, 1 year for a feasibility study for testing on a limited scale, and 3 years to run the actual experiments. A number of people stressed that we need data for more than 1 year. As some of you know, phenomena such as Hurricane Agnes occur frequently in the Great Lakes, resulting in atypical years for ecological purposes, so we need data for more than 1 year.

Time is needed for feasibility studies. I feel strongly, and I think most of the panel agree, that feasibility studies should be carried on until we are actually prepared to do the experiments. This may mean, in some cases, that studies never get beyond that point of feasibility, implying all projects should not be continued throughout a funding cycle. That viewpoint could reflect a personal bias on my part.

It may not be possible to study more than three sites. Four of the five Great Lakes have been selected, leaving out Lake Erie. Eventually one must decide whether, for comparative purposes, to select one site in each of the multiple lakes or to select multiple sites in one lake. Several options for sites were discussed. In Lake Ontario there were two sites--the Rochester, N.Y., area and the Oswego, N.Y., area. Again, the scale of the study area is not apparent at this point, so possibly one site could extend from Rochester to Oswego.

In Lake Michigan, three or four sites were selected. If we take the lead of the previous group, we would end up with three. Possibly there is an area near Chicago as well as an area near Milwaukee. We could not decide whether this should be one or two areas, and I think the previous group put the middle of the study area halfway between Chicago and Milwaukee, resulting in only one study area. There should be another area on the eastern shore of Lake Michigan somewhere between Benton Harbor and Muskegon. Muskegon is almost directly across the lake from Milwaukee, and Benton Harbor is roughly at the same latitude as Chicago, so this would provide east-west comparison. Then an unpolluted area of Lake Michigan should be included as well; it would have to be in the northern part of the lake.

Dr. Beeton proposed that we assess the land effect as it relates to water quality. He has collected data from Beaver Island. This is another way to study the nearshore--by selecting an island site in the middle of the lake with clean water and little pollution from land.

In Lake Superior, there are two logical study areas. One is the Keweenaw current which flows along the south shore from the Keweenaw peninsula to Whitefish Bay. It has been recognized by a number of investigators, and it is a discrete water mass. Then, of course, the area around Duluth is one that has been affected by man. We also felt there might be some justification in proposing a study site in Lake Huron, particularly south of Saginaw Bay, since the area is being studied extensively this year as part of the Upper Lakes Reference Study. The ongoing work will provide background data, but these studies are aimed more toward the open lake than toward the nearshore area.

A number of important items were discussed that have not been covered yet. I will conclude by presenting a shopping list of six or seven items. First is the need for high frequency sampling in the nearshore zone. Perhaps, a nearshore experiment might last only 3 or 4 weeks, but sampling would be intensive during that period. Another way of undertaking high frequency sampling is to study a square meter of the lake, as Dr. Beeton mentioned yesterday in his response.

Second is the need to develop instruments, particularly instruments for continuous monitoring, so that data can be obtained without using a ship to occupy a station.

Third is the important problem of pathogens that was outside the competence of our group. Pathogenetic organisms are released into Great Lakes waters, but little is known about their fate in the environment. Cooperative studies might be arranged with agencies who have public health responsibilities; these studies could be conducted simultaneously with the proposed program.

Fourth, a lot of people were concerned with sediment-water interchange, and I am sure it will come up again today.

Fifth, there is a great need not to neglect the study of the atmospheric contribution of pollutants, even if we are talking about a localized zone. The preliminary phosphorus budget for Lake Huron is one-third the nutrient contribution from the atmosphere, another third is from the two major inputs which are Lake Michigan and Lake Superior, and the final third is from Saginaw Bay. This gives some idea of the importance of the atmosphere, and, of course, there are also toxic or hazardous materials in atmospheric inputs.

The final thing which possibly should be stressed is that the term "biological and chemical processes" refers to studies at every level of the ecosystem, including phytoplankton, zooplankton, benthos, fish, and bacteria. These processes include the function and quantification of various biological components. Although this observation is apparent to most ecologists, we stress it because it may not be essential for presentations from other groups. We have to be concerned with interactions, not only the biological interactions but also the chemical interactions, among these different biological groups.

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2.2.3 Lake-Atmosphere Interactions - M. Estoque, Chairman

This group interpreted differently from the two previous work groups what it was supposed to do. Instead of considering general research initiatives, members of the group considered specific problems on lake-air interaction processes. This was done first by calling upon each member to suggest specific problems. A list of the problems was made, and then each problem was discussed in detail in order to define it clearly. Finally, we assigned priorities to the problems on the basis of socio-economic value, scientific merit, and resource (manpower and money) required for solution of the problem. As expected, when assigning priorities, each person was plugging for his own suggestion. Priorities were assigned by consensus among members of the group.

The specific problems which were suggested are summarized in table 9. The first two problems come under the general category of wave studies. The first item under wave studies is concerned with the physical processes which are responsible for the generation, growth, and breakup of waves. Investigation of this item requires observational as well as analytical studies. The second item under wave problems is the applied problem of wave prediction by semi-empirical techniques. The empirical relationships will be formulated with the aid of pressure distributions or other large-scale synoptic descriptive parameters which are observed over the land surrounding the lake. The next problem concerns the prediction of surges. The problem should be restricted to surges which are induced by subsynoptic-scale weather disturbances. The next problem is the determination of the effects of waste heat disposal on the quality of the environment, in both air and water. The next problem is concerned with coastal erosion--the effects of wind stress and waves of coastal erosion under severe weather conditions. The next problem involves the prediction of the space distribution of ice and the physical characteristics of ice on lakes. The prediction of ice distribution needs an understanding not only of the physical properties of the ice but also of the atmospheric conditions which tend to break up or melt the ice and transport them in the lake. The next topic is concerned with the evaporation from the lake; this is one of the problems being studied under IFYGL. The problem is the determination of the amount of evaporation from the lake, primarily for the purpose of using it for the analysis of the lake water budget. There is still some question in my mind about how the group wanted to approach this problem. I feel that what was intended was a determination of the rate of evaporation from the synoptic conditions by empirical techniques. The next topic concerns precipitation, also in connection with lake water-budget calculations. When it was originally proposed, this problem was to be concerned only with determining the amount of precipitation from synoptic observations by empirical methods. But as the discussion progressed, it gradually evolved into the more complicated problem of calculating precipitation by physically modeling the mesoscale disturbance generated by the lake. The next

Table 9: Summary of Priorities Assigned to Various Problems

	Socio-Economic Value	Scientific Merit	Required Effort and Resource
Waves (Physics)	3	1	2
Waves (Empirical)	1	3	1
Surges	2	2	1
Waste Heat Disposal	1	1	3
Coastal Erosion	1	3	2
Ice	1	1	3
Evaporation	2	2	1
Precipitation	3	1	3
Air Pollution	1	1	2
Fluxes (T, M, Q)	3	1	3
Vertical Structure	2	1	2
Nearshore Boundary Layer	2	1	2
Submesoscale Processes	3	1	3
Lake Effect Storms	2	2	2
Water Pollution Drift	2	2	1

problem is concerned with air pollution, with special emphasis on the role of lake-induced air circulation in transporting and dispersing pollutants over the lake and vicinity. In addition, the problem includes the transport of gaseous material from the atmosphere into the lake. I had not expected this transport to be substantial so was happy to hear from the previous speaker that this transport is important. The next topic is concerned with the basic problem of determining the fluxes of momentum, heat, and moisture from the lake surface. Again, this is one of the important problems being investigated under IFYGL, but the group feels that the IFYGL program will not completely solve this problem. Determination of fluxes during highly unstable conditions, including strong winds, and also as a function of fetch from the shore will probably not be solved by current IFYGL studies. The group would like analogous studies over other lakes. Such studies will show whether empirical relationships formulated with Lake Ontario observations are valid for other lakes. The next problem has to do with the determination of the vertical structure of temperature, moisture, and wind in the lower planetary boundary over the lake. This experiment was also planned during the IFYGL field program, but I believe the plans were not carried out adequately. The next problem involves the determination of the nearshore atmospheric boundary-layer structure. This is important because the shoreline, which separates the land from the lake surface whose characteristics are sharply contrasting, will produce strong horizontal gradients under different atmospheric conditions. Therefore, it has been suggested that this structure is an important consideration in the analysis of diffusion processes, especially in relation to the pollution problem. The next problem is concerned with eddy transports associated with disturbances of scales from 1 to 10 km, scales which were not observed during IFYGL. As one may recall, the observation stations during the IFYGL field program were relatively far apart; therefore, it was not possible to observe effects of disturbances of these scales in transporting various quantities, such as momentum, heat, and moisture. The next to the last problem is the description and prediction of lake effect storms. Finally, the last problem is concerned with pollution of lake surface waters. Although the problem involves the water instead of the air, the group feels it is appropriate for us to suggest it because the solution depends upon an adequate knowledge of lake-air interaction processes.

After discussing the above problems, we assigned priorities. Three factors were considered in assigning priorities. The three factors were the following: (1) the socio-economic value of the problem; (2) its scientific merit; and (3) the amount of effort and resources (money and scientific manpower) required for conducting the research needed to solve the problem. Priorities in table 9 are indicated by numbers from 1 to 3. In terms of socio-economic and scientific values, 1 means the highest value. From the point of view of effort and resources, 1 indicates the least amount of effort and resources required. The highest priority problem would be that which has a 1 in all three columns. But no problem had 1 in all three columns because, as expected, there is a tendency for a high rating in socio-economic value to go with a low rating in scientific merit. It has been suggested that the table may be used for determining whether a university, private research organization, or government laboratory is best suited to do a particular problem. The basis for this suggestion is the notion that a university is best suited for undertaking a problem with high scientific merit (basic research), while a private research organization is best suited for a problem with high socio-economic value (applied research). Moreover, a government laboratory would be ideal for doing a problem which requires a large amount of effort and resource. For example, a good problem for a university is one with a rating of 3-1-1; for a private research organization, 1-3-1; and for a government laboratory such as GLERL, 1-1-3.

In conclusion, the collection of problems which have been presented by my group looks like a rerun of the Boundary Layer and the Lake Meteorology programs of IFYGL. This similarity did not emerge by design, but rather by chance.

2.2.4 Environmental Dynamics - C. H. Mortimer, Chairman

The environmental dynamics group had, we believe, one of the most difficult tasks of all. One of the difficulties was defining what is meant by environmental dynamics. This we took to include the physical dynamics of water, air, and solid substrate, the dynamics of chemical transformations and transports; the dynamics of biological production; and the interactions between all these. At the same time, although we did not consider it in detail, socio-economic interactions should be kept in mind. These categories cover almost everything! Perhaps surprisingly, we did come up with a consensus on a number of points, and although we were instructed to consider science rather than policy, most of the things we agreed on are in the policy area.

We agreed on the following:

(1) The nearshore zone, defined hydrodynamically as 10- to 15-km wide, should be the principal focus for post-IFYGL investigations and modeling because this is the zone of maximum physical activity, maximum chemical and biological variance, and maximum human use.

(2) The proposed concentration of field work, instrument arrays, and modeling efforts in the nearshore zone must not lose sight of the fact that the physical and biochemical driving forces are developed on larger whole-basin, drainage basin, and meso-atmospheric scales.

(3) Active support for analysis of IFYGL data should continue (and this was strongly emphasized) for several years to exploit this unique base for progress under (2) and to plan the proposed nearshore zone study, including essential modeling testing activities listed below.

(4) The distinctive mission of GLERL should be development of the scientific basis and support, with the appropriate in-house interdisciplinary expertise and facilities, for a post-IFYGL effort directed to the nearshore zone and for the Great Lakes mission-oriented programs or needs of NOAA.

Among the NOAA programs, we referred specifically to the Sea Grant Program and the Coastal Zone Management Program and to interaction with the International Joint Commission.

The International Joint Commission was specifically mentioned because the Research Advisory Board has been very active over the last year or so and has created a number of standing committees in water quality and physical and biological fields. The Upper Great Lakes Reference Study will be coming to an end within the next 2 or 3 years and will be reported upon. If Canadian cooperation in the post-IFYGL activity or in other Great Lakes research activities is to come about, as we recommend, then the International Joint Commission provides a convenient and proper vehicle. Recommendations of the International Joint Commission Research Advisory Boards and its standing committees and existence of the new International Joint Commission headquarters office in Windsor will facilitate Canadian-United States collaboration smoothly and "legally."

Turning more specifically to the distinct mission of GLERL, while part of its research effort will be directly applied to problem solving, the distinctive function of GLERL should be development of fundamental understanding of natural and perturbed systems through:

(1) Observation, experiment, and monitoring.

(2) Model development, verified at each stage by data produced from new and, of course, existing data banks.

(3) Some of the research themes should be basic, strategic, and long-term in nature, i.e., strategically selected to provide research support and research output needed by identified users, by other components of NOAA, and by other agencies involved in environmental management and decisions.

(4) The emphasis should be on natural science, rather than on social science, although we cannot ignore the social and legal aspects of institutional design which will be needed to put some of the scientific recommendations into effect.

(5) In the planning and implementation of GLERL programs, including the proposed nearshore investigation, cooperative activities should be encouraged with the academic community, other Federal agencies, and research groups, both United States and Canadian--the latter case through the International Joint Commission as appropriate.

Most of our debate was concerned not with these points of consensus, but rather with examples of activities to be undertaken. The following possible activities within the GLERL mission were mentioned:

(1) Designing a sampling network through a space-time analysis to develop the optimum spacing and frequency required to understand natural distributions and to follow significant trends.

(2) Assembling and critically reviewing existing and emerging physical, chemical, and biological data for the purpose of model testing, model development, and design of effective long-term strategies. This would include a critical review designed to detect and analyze the significance of long-term trends.

(3) Intensively studying inshore-offshore exchanges and partition of energy.

(4) Studying the mechanics of upwelling and subsequent whole-basin responses, generation and decay of nearshore currents, and transport of material, nutrients, toxins, and organisms.

(5) Standardizing and intercalibrating methods of measurement and analysis.

Within the framework of increased understanding of relevant physical, biological, and chemical mechanisms operating principally in the nearshore zone, the scientific basis for action on a number of present or emerging problems was discussed. These problems, identified by individual panel members as important, were energy management in the Great Lakes, recreational planning for the Great Lakes, natural resource inventory and utilization, and water quality criteria. Under water quality criteria, the following subtopics were proposed by individual panel members: chronic sublethal effects of pollutants on lake ecosystems (not enough attention is being given to these effects, which probably have long-term significance); development of a scientific basis for evaluation, under the present U.S. laws, of tradeoffs between costs, technical feasibility, and the social desirability of zero pollution input; evaluation of stream loadings for both nutrients and toxic substances; criteria and techniques for disposal of dredgings, involving water quality criteria and recreational considerations; and shore and beach processes, including erosion and material transport. The *Cladophora* problem was also identified as eutrophication effect in the Great Lakes of maximum public visibility.

2.2.5 Water Levels and Flows - D. Meredith, Chairman

During the workshop, the research objectives and information needs related to lake hydrology and regulation were identified.

A conceptual hydrologic model of the entire Great Lakes system is needed that will be responsive to existing and anticipated user needs. In order to move toward this objective, we recommend the following actions:

(1) Replace the present empirical relationships for computing monthly connecting channel flows. This should include the implementation of the equations of continuity and motion and a time scale required for the development of an improved conceptual hydrologic model and other discipline needs.

(2) Make simultaneous discharge measurements in the connecting channels to calibrate and tune the model.

(3) Investigate the application and adaptation of existing rainfall runoff, snow accumulation, and ablation models in order to define basin runoff more accurately.

(4) Obtain the necessary parametric input data (wind, temperature, radiation, etc.) to support a more complete conceptual hydrologic model.

(5) Extend and refine hydrologic models to use remote sensing data.

(6) Investigate the magnitude and flux of ground water in the system. The first phase should be a limited investigation to determine its relative order of magnitude, including bank storage. If warranted, a full investigation would require a comprehensive data collection and analysis program for use in conceptual models.

(7) Develop conceptual models of lake ice information, growth, and decay.

(8) Investigate ice retardation in the connecting channels and the impact of ice on hydrologic and coastal zone processes. This will require a data base which includes lake heat budgets, areal ice distribution, and geochemical composition.

The present status of lake regulation, based on trial and error procedures, has proven to be inadequate. For example, both present and proposed regulation plans for Lake Ontario failed last year. We recommend the use of operations research techniques and stochastic inputs to derive improved regulation plans for the Great Lakes. This will require the following:

(1) The use of probabilistic and stochastic models to generate supplies to the Lakes. Models for both lumped and individual parameters should be considered.

(2) Sensitivity and optimization studies coupling the parametric inputs with the constraints and criteria by which the optimum is defined.

(3) Inclusion of water quality, water levels, dynamics, and other environmental effects in regulation criteria.

3. PRIORITY RESEARCH INITIATIVES - E. J. Aubert

You may not have had an opportunity to digest everything fully, but this session is now open for discussion. One topic for discussion may be areas of overlap between the panels or areas where there are differences or agreements among the recommendations. Any comments you have are appropriate at this time.

Scott. Much was said about the shore zone and I think we ought to define it. We have to define what it is along the lines of what Prof. Mortimer was saying. I agree with Cliff. It's in quotes right now--"shore zone."

Chapra. The Aquatic Ecology and Water Quality work group had some discussion as to what was the "shore zone" in terms of its biology and chemistry. I would like to point out that it might be defined differently from a physical as opposed to a biological or chemical point of view. This should be kept in mind when designing a field program to insure that the zone is described with a sensitivity to all important perspectives.

Mortimer. What is wrong with 15 km?

Aubert. Does anybody need a shore zone wider than 15 km?

Scott. I was thinking in terms of a water quality and biological definition as well as a physical definition, rather than an arbitrary boundary somewhere beyond the coastal jet.

Mortimer. What drives the coastal zone biologically, chemically, and physically is, of course, the whole lake, including the regime of the regional atmosphere and the drainage area. So I do not think we should regard this nearshore study as being geographically defined in that way. I think there is going to be an array of observations concentrating in the area of immediate interest, but also, in some cases, taking account of whole-basin motions.

Scott. I agree with the designation of scale. Maybe 10 km is a little small.

Mortimer. But we should give some order of magnitude. To some people, the shore zone is the beach zone where the waves break, causing shore erosion and transport of solids. I would want to go to where the Kelvin waves become unimportant, and that is 10 to 15 km offshore.

Csanady. I think 15 km would be fine.

Beeton. In the Aquatic Ecology work group, we thought that we should have sampling out into the open lake, as Claire Schelske indicated. I do not know exactly where that would be. It might be 15, even 20 km, depending upon the lake; in order to study the perturbations that occur in the nearshore zone, we need reference levels. So if we study processes out in the open lake and the same kind of thing in the nearshore zone, then we might start to get a handle on some of the things that are going on.

Aubert. Several people have referred to the sampling diagram proposed by Ted Green. It shows the total lake being monitored, with the sampling intensity greatest in a particular segment of the nearshore zone. I think several of us were looking to that sort of a grid consistent with what you are saying, Dr. Beeton.

Mortimer. Such a pattern was followed somewhat in IFYGL with a concentration of observations in the Niagara bar, if you recall, but not on this fine a scale. The question of site has been raised. We did not consider that in the Environmental Dynamics work group. The question of Canadian cooperation has also been raised. This is a fairly long-term planning decision, and until we get a reaction from our Canadian colleagues, the question of sites has to be shelved, at least for the time being.

Aubert. We can examine alternatives, but it is certainly premature to make a decision at this point.

Baer. I want some clarification on the same question I asked yesterday morning. You said, if at the end I still had the question, to ask it again. It appears to me that both the Water Movements and Aquatic Ecology panels are speaking about a son-of-IFYGL, a large-scale, massive program to cover a great area. I am talking about things on a smaller scale, not a repetition of IFYGL.

Aubert. Why not have the chairmen of those two panels speak to your question.

Csanady. I think the expression was used, but, when the Water Movements group got down to details, the experiment turned out to require a relatively concentrated array. In our mind, it is one array; and in the Aquatic Ecology group, it is several arrays of relatively small dimensions. These arrays are to be supported in order that they may be put into the broader picture, by lakeward measurements of decreasing intensity like the T. Green diagram which seems to be a pretty much agreed-upon scheme.

Baer. How would the interactions and cost relate to the original program?

Csanady. In terms of expense?

Baer. In terms of expense, number of institutions required, and other things of this nature.

Csanady. I think the scope would be less than IFYGL, but that is my feeling. From what we have discussed, individual members might feel otherwise.

Schelske. What was the IFYGL cost; for instance, the ships, buoys, and this sort of thing?

Aubert. I cannot cite numbers for specific parts of it, but our estimate of the total program is something like $30 million between the United States and Canada over a 7- to 8- year period, all of which has not been completed yet. A cost estimate depends on what is included. That amount was not earmarked on the U.S. side in a budget item called IFYGL. A lot of existing resources were directed toward this cooperative effort, and I think on the Canadian side the major input was the redirection of existing resources. If they had not been working on IFYGL, they would have been working on some other Great Lakes activity. The numbers could be added differently. Four or five large vessels were involved, with many supporting ones.

Schelske. You are concerned about the cost. That may be the wrong way to approach it. Unless it is a fairly large study, the returns may not be maximized for the amount of money spent. I think we learned from IFYGL that with a large number of people working in the same place utilizing common facilities, we got more for our money. So I think the size of the program has to be determined after the requirements are defined; if it cost $10 million, it is important to find $10 million. With more planning, one could get a better handle on the needs. The Aquatic Ecology group felt that this study would not be a son-of-IFYGL, but maybe a cousin to it. In terms of working in the Lakes, it is entirely different. We want to study local areas. IFYGL covered a whole lake, and the size of the areas that we propose for study may have been covered by only one station in IFYGL. It is an entirely different problem.

Aubert. The monitoring program would undoubtedly be quite different.

Schelske. In the kind of program we are proposing, there might not be an effective way to use the *Researcher*.

Baer. The point I wanted clarified was, what is the ultimate scale of research? I was not trying to find whether it should be big or little at this stage. I was trying to determine your recommended optimum scale. Finances, people, and dollars all amount to the same thing at this stage.

Aubert. I think it could be put into a range between 0.1 and 1.0 of IFYGL. The scale must be significant. The required data acquisition systems are not such that one could dip a thermometer in the lake and expect to come back with useful data. It will require instrument development. The discussions referred to more than 1 year of monitoring--3 years of monitoring. IFYGL had primarily 1 year. The distribution of monitoring stations would be altogether different so that the types of systems that might be deployed would have both similarities and differences. The time scale of monitoring also would probably be different.

Holland. The program will be of the pilot type through the second year, with field work for 3 more years. You are going to need time to complete the processing and analysis of the data, so it will not be a 5-year program. It might be a 7- to 8-year program. Even with good advance planning and quick turn-around of the data processing, it still takes time to digest, analyze, interpret, and integrate. Another point is that we have not reviewed ongoing programs, and I would hate to see the implication left that, if one major thrust is identified for high priority, it consumes all the effort so that essentially nothing else gets done during this time. We have not addressed this question, but I think our assumption is that other efforts on the whole lake or in the middle of the lake or other problems will be taken on their merits and not swallowed up by this program. This is a new initiative over and above other things that may be ongoing.

Aubert. Prof. Mortimer can speak to this latter point because the Environmental Dynamics panel discussed this question from the point of view of whether we are talking about single-agency funding, multiagency, commercial, or a host of other potential sources of funding.

Mortimer. Speaking for myself, I am sure that people responsible for university programs would be willing to modify them in order to participate in a program of this kind. There would be nothing more important that we could do for the Great Lakes than participate. The question of other agencies needs consideration.

Our panel did not review that matter because it would be getting into politics. There are, for example, two other agencies developing missions on Lake Michigan, and it would be sensible planning to find out what they are doing and, where possible, to fuse the work together. I am not a fiery proponent of large-scale science, but in projects of this kind, particularly if we have Canadian cooperation, we can do something better together than we could do it separately. Most people would have a great interest in participating and would drop everything they are doing for those years. The availability of funding would be the key as well as the availability of research vessels which are expensive. We do not need another *Researcher*, particularly if it is only going to work 4½ days a week as it did in IFYGL.

Schelske. I want to reemphasize that the proposed biological program is one that would be important from the practical point of view. Again efforts have to be concentrated in the nearshore zone where there are tributary inputs, municipal water intakes, and a whole range of ecological and sociological problems. We need to know about short-term response as well as long-term response.

Aubert. Many of the topics that Clifford Mortimer mentioned from the Environmental Dynamics panel are obviously long term. IFYGL was a project with a planned start and end. This Laboratory has a long-term mission which is not of a project duration, although I cannot say what the duration may be. The focus of this workshop was placed on a next major initiative in the Great Lakes. There appears to be unanimity for this focus to be concentrated in the nearshore. Many of the activities that have been mentioned are of a longer duration than 5 to 7 years; they are decades. We cannot wait that long to produce useful answers, but neither can we work simultaneously on all of these problems. Some sort of a priority listing must be established. If at some future time the priority shifts or when some are finished, the list can be modified. Work can only be done on problems at the top of the priority list. Another point is that the activities include topics that do not all come under a single project structure.

Csanady. In connection with either a long-term or a short-term approach, I want to call attention to a common failing of these programs. When people come into them and have to design a program on short lead time, an early decision is often made that state-of-the-art instrumentation will be used instead of trying to solve the problem the best possible way. Let's take things off the shelf, put them in the lake, and see what we get. This works sometimes, but it would make much more sense to allow enough time to develop the most sophisticated instrumentation techniques this age is capable of.

Aubert. The Water Movements panel report alluded to that. This means more lead time is required before deployment of any new major instrumentation or data collection system.

Beeton. I do not see how NOAA can develop an effective program in the Great Lakes without getting a handle on what is going on in the nearshore zone so that you can logically plan a longer term program. It would be very wise, whether you call it a small IFYGL or something else, to have a project like this with one spin-off being the definition of the long-term program that you should begin developing.

Sloss. It seems then that one of the first priorities would be to define what is the nearshore zone for a particular process. Some of them may extend farther out than others, and it would be a logical type of pilot study to see just what kind of an area must be looked at.

Scott. We have now heard the individual groups, but not too many people "floated" between them as was planned. Some mechanism should perhaps be developed where the right scales are used for the processes that were mentioned. I talked to Don McNaught and Fred Lee and they complained that the wrong scales were used. The first station was out 1 km from shore; a lot more detail was needed near the shore. If the physical people set up a station pattern without consulting other interested parties, as was apparently done in some cases in IFYGL, I think we are again making a mistake. Maybe GLERL will be the mechanism for this needed intergroup cooperation.

Birchfield. I am not sure how this applies to all of the panels, but it seems to me that one must always make a conscious effort to make planning more than simple instrument development. The tendency in IFYGL was to do only the latter. As someone mentioned yesterday, the Mid-Ocean Dynamic Experiment had a workshop lasting an entire summer in which to develop dynamical models that would act as a focal point for ideas on how to gather the observations for that experiment. It seems to me that, since the focus is on the coastal zone here, some sort of development of dynamical models should be started right away in that area, for example, numerical models. There is none now.

Aubert. This could be lumped under experimental design, I presume.

Monahan. To do it a little more evenhandedly, I think evolution of the models should go hand-in-hand with the evolution of the instrumentation, and needless to say, both of them should precede the actual field data collection.

Aubert. Instrument development and numerical model development both need to come early in order to be available when they are needed.

Csanady. The way they handled it in the Mid-Ocean Dynamic Experiment was with a scientific council. Maybe you should establish such a council if you decide this Laboratory will support such an effort. One good and relatively cheap way to start is to set up a scientific council and maybe have a workshop.

Aubert. That is a mechanism that should certainly be considered.

Comment. I think we need a more specific mechanism to learn about the IFYGL results. No one has mentioned how they are going to feed into any of this activity. What are the plans for scientific discussion of IFYGL results?

Aubert. I think what we are talking about here is more of a scientist-to-scientist interaction. I will comment as to what now exists. The IFYGL program is divided into panels. Lloyd Richards is the Canadian co-chairman and I am the United States co-chairman of the Joint Management Team. We are meeting the latter part of this month to review the status of the outlines for the final *International Scientific Reports* from each IFYGL panel, which should be published within the next 1 to 3 years, then to identify potential delays, and finally to resolve these delays to insure that the *Scientific Reports* are prepared. That perhaps is a narrow objective from the overall point of view, but these reports are considered to be the final IFYGL product. The *International Scientific Reports* will summarize all of the hundreds of articles and reports that will have been published in scientific journals and the reports of various agencies and institutions. The scientist-to-scientist interaction in the IFYGL plan is within the panels, with the panel co-chairmen defining and producing the scientific reports. Lloyd Richards and I have to insure that workable plans are developed by the panel co-chairmen consistent with all of the conflicting constraints from other programs. What you are suggesting goes beyond our plans.

Csanady. One problem in IFYGL was cross-panel communications. Those panels have large walls around them.

Aubert. Cross-panel meetings need a defined context. Lloyd Richards and I are pushing from the context of the *International Scientific Report* series, the product which will wrap up IFYGL in 3 years. We believe it is important, and if we do not give it continuing attention, it will never happen.

Mortimer. Another product should be a data catalog.

Aubert. That is already included in the plan. People are working on it. It is just a matter of time before the archive will be generated and an archive catalog will be available.

Mortimer. I think there should be a final workshop meeting someday, perhaps at the same time as an IAGLR conference. A whole day can be spent on IFYGL wrap-up. It would be helpful in about 2 years time.

Aubert. An IFYGL symposium was held last April in Washington at the American Geophysical Union meeting. We had ½ day and 11 invited papers. At the IAGLR conference in August 1974, IFYGL had 54 individual presentations. More papers were submitted, I think. Most of these papers will be in a special proceedings due to be published in the spring of 1975, but that still is not the interaction you are talking of. Somebody gives a formal presentation followed by a minute of discussion. Of course, one can then seek the individual out for personal interaction. Do you have something to suggest?

Comment. One thing that would be helpful would be for someone to write a review paper on all the publications.

Aubert. One of the final *International Scientific Reports* will be an overview of IFYGL. Lloyd Richards and I are listed as co-authors, but we may get help. This final *International Scientific Report* will not be published for 3 years.

Csanady. Another point that I already raised in a group meeting was that, when you get these data availability catalogs, it would be helpful if an individual set of data was called something other than GS1500 235MB.

Aubert. If you cannot learn the system, you will have to ask for assistance from an expert.

Holland. You do not have to learn the system; just take the listing, find what you want in it, and ask for it. The filing system must have some such methodical labeling system, and you have to give the person who works in the files the identification he needs to retrieve the item you want. He has a catalog. If you give him the name, he has to look up the number.

Schelske. Before the meeting breaks up, there is one thing I would like to make a statement on. I am a little concerned about some of the items that appear on this list, in particular, zero pollution discharge. That has broad implications. A lot of people have worked very hard to get that kind of law on the books. We also talk about energy management. These are almost philosophical questions. If we are going to do that, it is fine with me, but I think we also ought to extend that list and include items like no-growth policies and zero population growth. All those are related. There have been tremendous advances in terms of controlling pollution from this one law on zero pollution discharge, and if we now say this is a scientific question that has to be studied, that is obvious. But on the other hand, we are making an issue out of something that is almost philosophical.

Aubert. I think a rebuttal from the Environmental Dynamics panel chairman is needed here.

Mortimer. These are examples of pressing national questions or questions some way down the road for which this Laboratory will provide part of the scientific basis for rational decisions. I think the important thing to stress here is that the Laboratory should not express opinions on environmental politics, but should provide a sound scientific basis for rational decisions, if such are possible. The zero pollution law, as defined, involves a decision, or so I am informed by a panel member, on what is socially desirable and what is technically practical. There will be tradeoffs between zero pollution, which is, of course, unattainable because you have diffuse sources as well as point sources, and what is socially desirable. For wise decisions on pollution control, or wise decisions on the use of the Great Lakes as heat sinks, a sound scientific basis is needed. These points were raised by the panel only as examples of questions for which a sound scientific basis is badly needed.

Schelske. I would agree with that, but my criticism then is, why do you disregard zero population growth?

Mortimer. Because no panel member raised that particular question.

Aubert. Thank you for attending this workshop. I hope you got as much out of it as we did. All who attended will get a copy of the transcribed tapes after review by the principal speakers and responders.

APPENDIX A. WORKSHOP GROUP MEMBERSHIP

WATER MOVEMENTS	AQUATIC ECOLOGY AND WATER QUALITY	LAKE-ATMOSPHERE INTERACTIONS	ENVIRONMENTAL DYNAMICS	WATER LEVELS AND FLOWS
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G. Csanady, Chmn.	C. Chen	C. Emanuel	D. Chandler	L. Bajorunas
T. Green	D. Frits	M. Estoque, Chmn.	B. Eadie, Sec.	J. Derecki
E. Monahan	C. Schelske, Chmn.	J. Fletcher	C. Gunnerson	D. E. Meredith, Chmn.
R. Pickett, Sec.	J. Schmidt	A. W. Green	C. Herdendorf	E. Peck
D. B. Rao	J. Spain	J. Holland	W. Hess	F. Quinn, Sec.
G. K. Rodgers	E. Stoermer	D. Houghton	J. Judd	
J. Saylor	R. Sweeney	C. F. Jenkins	R. Kolf	
J. Scott	S. Tarapchak	P. Liu	A. Lane	
P. Sloss	R. Thomann	M. Mull	G. F. Lee	
	H. Vanderploeg	C. Snider	C. Mortimer, Chmn.	
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