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RECOMMENDATIONS FOR RESEARCH

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marine sciences research center
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Approved for Distribution
J.R. Schubel
J. R. Schubel, Director

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PREFACE

Origin and Purposes of the Workshop on Transport Processes in Estuaries

In the spring of 1976 a symposium to review our knowledge of physical transport processes in estuaries was held at the Belle Baruch Institute. At the conclusion of the symposium the following statement, drafted by J. R. Schubel, was endorsed by the participants:

> "On 20-22 May 1976 a group of estuarine oceanographers from the United States, Canada, England, and South America met at the Belle Baruch Institute for Marine Biology and Coastal Research of the University of South Carolina to review and critically assess our knowledge of estuarine transport processes. It was the very strong consensus of the group that recent data show many of our previous ideas of estuarine transport processes to be overly simplistic and that a greater level of sophistication of our understanding of these processes is required not only for a significant scientific advancement, but also for effective environmental protection and management.
>
> "A knowledge of the physical oceanography is fundamental to understanding the biological, chemical, and geological processes that characterize an estuary. This information is in turn necessary for the formulation of the predictive tools needed by governmental agencies for effective management and rehabilitation of the estuarine environment. Reliable predictions can not be made of the dispersion of pollutants, the resuspension and movement of dredged spoil, or the assimilative capacity of an estuarine system without a working knowledge of its characteristic physical processes.
>
> "While millions of dollars are being spent each year on monitoring of the estuarine environment, the resulting data are generally of little use to oceanographers interested in processes, or in formulating, constructing, and verifying analytical, numerical, or physical models. The data are also, unfortunately, frequently of little value to regulatory agencies in attaining their long-term pervasive goal--effective management of the coastal environment. Through proper coordination and planning, experimental programs can be designed that not only satisfy the short-term needs of regulatory agencies, but also provide the oceanographers and managers with the data they require for development of predictive models.
>
> "A proposal will be submitted to appropriate Federal agencies within a few weeks for support of a workshop to identify the important problems of physical transport processes in estuaries, and to explore the most effective ways of attacking these problems. Efficient utilization of existing manpower and facilities for an adequate field study of the dynamics of any single estuary will probably require collaborative efforts of scientists from several academic institutions and from governmental and management agencies."

Pursuant to the foregoing statement, a Workshop on Transport Processes in Estuaries was held at the Marine Sciences Research Center, State University of New York, Stony Brook, New York from 10 November to 14 November 1976. Thirty-one participants from some 18 institutions and agencies focused their discussions on transports of water, salt, and fine-grained suspended sediments.

The primary goal of the Workshop was to identify the important unresolved problems of physical transport processes in estuaries; problems that must be solved, not only for their scientific urgency, but also for effective management and rehabilitation of estuaries.

The secondary goals of the Workshop were:

(a) To assess the manpower and materiel necessary for the field experiments on which the solutions of important unresolved problems must depend.

(b) To explore the means for inter-institutional cooperation and collaboration which will be required if the necessary large-scale, extended field experiments anticipated are to be made feasible.

(c) To explore ways in which current monitoring programs, which are relatively expensive, can be made more useful both to management and to science.

The present report by the authors was written with due consideration for the discussions which occurred during the Workshop and for the written suggestions submitted by the participants but it should not be interpreted as a report which has been endorsed in full by all participants. In this report we have focused on the primary Workshop goal and the first of the secondary goals listed above.

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ABSTRACT

A review of the state of current knowledge of transport processes in estuaries is presented. A better description and quantification of those terms in the equations of motion not given *a priori* by the physics of the flow and commonly referred to as "diffusive" or "dispersive" remain elusive goals. Proper verification and testing of three-dimensional time-varying models that are universally applicable to different types of estuaries have yet to be undertaken.

A set of field experiments is outlined in broad terms. It is hoped that these experiments will provide new insight into basic nonadvective transport mechanisms in various types of estuaries ranging from well-mixed to highly stratified.

1. WHY SHOULD TRANSPORT PROCESSES IN ESTUARIES BE STUDIED?

Anything that moves water and its dissolved and suspended material is a transport process. Transport processes range from movement by simple streaming currents, through spreading by turbulence, to diffusion by molecular motion. Among the forces that produce transport are river discharge, winds, and tides. Some of the material is simply carried about without change; substances such as the water itself, dissolved salt, and fine-grained sediments. Others change with time, some slowly, some rapidly; things like heat, dissolved oxygen, nutrients, pesticides, and PCB's.

In order to know where these substances will go in a particular estuary we need to know how the processes which move them work and which ones are important there. As things are now, we know something but not enough to say in much detail what will happen in particular situations. It will be wise to begin the study of these transport processes by concentrating on the movement of those materials which do not change with time. They are easier to understand and, if we can understand the mechanisms which transport them, we will be a long step ahead in our understanding of the movement of materials which do change with time; which is a far more difficult problem.

Estuaries are a natural resource having a wide variety of uses. Wisely used they benefit many; misused, the benefits are not realized and may even become losses. Estuaries are a livelihood to the waterman; to biologists, large incredibly productive subaqueous farms. The average man goes to estuaries to boat, to hunt, to fish, to swim, or simply to restore his soul. To physical oceanographers they are huge natural laboratories for the study of fluid mechanics; to sanitary engineers, a dump for their sewage; and to the ecologist, an irreplaceable nursery for marine life. The utilities use them to get rid of their surplus heat and shipping uses them as fluid highways. To developers their water fronts are miles of the most desirable real estate. The list could be extended indefinitely. And all these uses are not compatible. Use for one purpose may make use for another impossible. Therefore, to the coastal zone manager estuaries are a gigantic headache; places where a wrong decision may cost millions of dollars, adversely affect thousands of people, and, in extreme cases, destroy the natural resource whose protection is his aim.

To the extent that science understands estuarine physical processes, of which transport processes are among the most basic, the manager can turn to the scientist for information on the probable outcomes of the choices he is contemplating; he can act with confidence that the good he proposes will be gained and the evil he seeks to avert will be averted. To the chagrin of the scientist and the dismay of the manager, many estuarine processes are not yet adequately understood.

Estuaries are anything but simple mechanisms. They are the regions where rivers and ocean meet and dispute for dominance. They change rapidly under the influence of the tide, shifting winds, variable river flows, intermittent ice cover, and fluctuating evaporation. They are highly complicated natural systems worthy in themselves of scientific study--arenas where physical, biological, chemical, and geological systems meet, mesh, and interact. An understanding of the physics of estuaries, and in particular of the transport processes, is a fundamental requisite for many problems dealing with questions which are not physical. Without it, understanding of the biological, chemical, and geological systems is, at best, uncertain if not indeed impossible in many aspects.

But the need for more astute management of our estuarine resources also forces the need for a better understanding of transport processes. We can not now say with much precision where such things as spoil from dredging, nutrients, sewage discharges, exotic chemicals, spilled oil, or heat will go--whether they will be flushed out of the estuary within any given time span or whether they will accumulate within the estuary; and where. There is an increasing number of government agencies which are "monitoring" the movements of these materials. But unless the transport processes are understood, it is difficult to see how the measurements are to be made at the right place at the right time. It is even harder to see how they are to be interpreted so that they make useful sense. In the face of this how can the manager manage?

Both man and nature are continually changing estuaries or, in the case of some man's efforts, attempting to change them. Nature provides hurricanes, floods, droughts, and a wide variety of other caprices. Tropical Storm Agnes brought more sediment into the Chesapeake Bay in the 10 days between 21 and 30 June 1972 than had probably been brought in during the previous 25 years. Man builds groins, piers, dams, and wiers; dredges channels and channelizes streams; creates islands out of spoil and makes building lots out of wetlands. This is just a small sample. Many of his activities alter the transport patterns. It would be as well to know before the work was actually done and the money spent what changes, direct and indirect, short-term and long-term, are to be expected.

We can scarcely afford to go on "experimenting" blindly with our estuaries as we have in the past. An excellent example of what we mean is the story of what was done to Charleston Harbor, South Carolina. Before 1941 the main river entering the harbor was the Cooper, a river whose discharge was quite low. Transport of sediment was seaward at all depths throughout the harbor and maintenance dredging was nominal. But during the 1930's the Santee River was dammed for hydroelectric power and conservation and diverted to empty into the Cooper River. The greatly increased flow into Charleston Harbor altered the transport patterns; sediment in the deeper water now moving landward. Bars began to build rapidly. Dredging costs to keep the harbor usable shot up to $5 million a year. It is now planned to put things back--to the tune of $91 million. If this is done, the dredging costs for Charleston Harbor seem likely to return to something more reasonable and the rediversion of the Santee will probably halt the severe delta erosion problem at the mouth of the Santee. On the other hand, it will destroy the lucrative hard clam industry that has grown up there since 1941. You can't really put the system "back." It would have been nice to have an idea of what to expect when the original diversion was decided on. It would be nice to know what to expect before we spend $91 million to redivert the Santee.

We just can't afford this sort of thing. The only remedy is to study estuaries until we know enough of their mechanisms to foresee the consequences of our actions.

2. MODELING AND SIMULATION OF TRANSPORT PROCESSES

2.1 *What is a model?*

Models are sometimes spoken of as though they had an isolated independent existence. This is never the case. A model is, of necessity, one half of a duality. There must always be both the model and the thing modeled. A model reproduces some, but never all, of the features of the thing modeled. Thus, the prime requisite of any model is that the features of the thing to be modeled be known. For example, a model of the Cutty Sark may reproduce on a smaller scale the masts and yards together with the standing rigging but not the running rigging. If the model is to be a "true" model, the proportions of the spars and the lead of the standing rigging of the Cutty Sark must be known before the model is built. The same can be said for a model of transport in Long Island Sound or of a model of the advection-diffusion process. Without a knowledge of the features of the original, no "model" is possible.

Models can be constructed of many kinds of materials and in many ways. For the study of transport processes three kinds are particularly useful: hydraulic models, analytic models, and numerical models. Hydraulic models, for example, the Army Corps of Engineers model of the Delaware at Vicksburg, Mississippi, reproduce the shape of the basin being modeled, usually with carefully controlled distortions, fill the model basin with fluid adjusted to match the known properties of the original, impose a force, such as tidal motions, and, after adjusting the model to reproduce the currents observed in the system being modeled, go on to study the transports of introduced contaminants. Such "naturalistic" models are often very large and very elaborate but even the most complete of them never pretend to reproduce every rock and sand bar of the original. In fact, if the features to be modeled are few, a simple rectangular flume may "model" an estuary.

Analytic models and numerical models are both mathematical. They deal with the concepts, numbers and measurements, which describe the features of the original. The models are expressed in the one case by analytical solutions to sets of equations and in the other by finite difference analogs of the equation sets. They too, may be complex including many features in great detail or simple describing only a few crudely. Since the number of known solutions to the dynamical equations is quite limited, most mathematical models are, in fact, numerical.

We build models in order to get something more manageable than the original. The word "model" comes from the Latin *modulus* which means "a small measure." Geometric models, e.g., ship models, are literally smaller than their originals. But if we take "smaller" to mean "simpler" or "easier to handle," all models are "smaller" in point of complexity. We might, for example, wonder how a particular estuary would behave if a dam were built which diverted nine-tenths of the freshwater inflow. We dare not "experiment" with a real estuary; we could hurt too many people and it would be too big to play with.

But if we have a satisfactory hydraulic model, it's easy to turn off the tap. If we have an analytic or numerical model, it is only a matter of modifying a few equations or changing some of the input to the computer. We can "stimulate" the behavior of the real estuary and, thus, get some guidance on our decision to build the dam.

But an extrapolation from model behavior to the behavior of the thing modeled is a chancy business. The way the real estuary works, its mechanism, is essentially unknowable. What we can know of it is the measurements of a few of its features under the range of conditions which actually obtain. The model is a quite different matter. We know how it works; what pushes what and how. Further, since it is a model we have been careful to make it duplicate the known features of the original. The leap we take is to believe that, since we know the mechanism by which the model duplicates the measured features of the original, we know that *the same mechanisms are at work in the same way* in the original. But the set of features modeled is always small. It is quite possible to build other constructs with quite different mechanisms which reproduce the features. Which then is the "true" model; the one that tells us what is really going on in the original? Perhaps the best approach is to hold all models provisional. The mechanism of the model that reproduces the known features of the thing modeled and continues to agree with observations taken after its construction is more convincing than one which does not. The mechanism of a model which shows agreement with the original in aspects for which it was not deliberately designed becomes positively intriguing. Clearly, an act of faith is required in every inference from model mechanism to original mechanism.

2.2 *How are models made?*

Model making of any kind is a craft, an art. Two of the most important tools the craftsman of mathematical models has are approximation and averaging.

To illustrate the use of approximation consider Newton's Second Law of Motion which says, in effect, that momentum is conserved. When the law is expressed in a form descriptive of fluid flows we find that it says: The total rate of change in the momentum of a flow at each point is the sum of the forces acting on the fluid. The total rate of change is made up of two parts: the local time rate of change in momentum at the point and the advective transport of momentum to the point by the sweep of the fluid motion. The forces which can act on a fluid are: the pressure-force, the Coriolis force (which depends on the earth's rate of rotation), the gravity force (which is gravitation adjusted for the centrifugal acceleration due to the earth's rotation), and the frictional or viscous force.

$$\begin{aligned}&\text{local rate of change}\\&\quad + \text{advection} \equiv \text{total rate of change}\\&\quad = \text{pressure} + \text{Coriolis} + \text{gravity}\\&\qquad + \text{friction}\end{aligned}$$

Such an analytic "model" incorporates everything that can influence the momentum of any flow and is thus complete. Unfortunately, it is so complex that it is mathematically intractable; we can deduce little or nothing from it.

However, in some flows not all the forces at work are equally important. Coriolis effects are very small in comparison with the others when the flow is of the size customarily found in laboratories. In such flows the terms representing the Coriolis effects can be set equal to zero and an approximate model constructed by omitting them.

There are a number of steady large-scale oceanic flows which show very little momentum change with time so that the total rate of change can be approximated to zero, e.g., the Gulf Stream. Further, water has a very small viscosity so that the frictional terms can also be dropped. A satisfactory model for these particular oceanic flows says that the pressure, Coriolis, and gravity effects are substantially in balance, i.e., add nearly to zero. The model says that in the horizontal the pressure and Coriolis effects are equal while in the vertical the hydrostatic equation gives a good description.

Knowing when you may safely approximate a term to zero and when the neglect will make what is retained useless nonsense as a model for the particular flow being studied requires an intimate acquaintance with the characteristics of the fluid flow you are trying to model.

The reduction in the complexity of a mathematical model secured by approximating terms to zero is gained by throwing away whole classes of effects. To the extent that the real flow is only slightly influenced by the neglected processes, the model may be a good one.

The reduction in complexity secured by averaging is of a quite different kind. Averaging simplifies by blurring the picture, not by throwing things away. In a way, it is a technique that seeks to bridge the gap between the instantaneous point by point descriptions offered by many analytic models and the world that we, as humans, perceive. The water temperature at each point within Long Island Sound is a perfectly good idea but one which humans are unlikely ever to realize. If we average the temperature over each square mile, we have a better chance. If we average over the entire Sound, we come up with just one temperature. The larger the interval over which the average is taken, the simpler and smoother our picture becomes; the details have been blurred and can no longer be seen.

But even though you can't see the details, their effects are not lost. As an example consider the way that the advective transport processes enter the equations describing the time rate of change of a substance s. In the instantaneous point by point description it is represented by terms like $u(\partial s/\partial x)$. If we average u and s we get things like $u \equiv U + u'$ and $s \equiv S + s'$ where U and S are the averaged values of the instantaneous point values of velocity, u, and substance, s, and u' and s' are what you have to add to the averaged values to make them equal to the instantaneous point values. The average of the advective transport term exhibited is then equivalent to $U(\partial S/\partial x) + \overline{u'(\partial s'/\partial x)}$. The first of these terms is of the same form as the original term and says that the averaged value of the substance is transported advectively by the averaged velocity exactly as the instantaneous value of the substance was transported advectively by the instantaneous velocity.

However, the second term says something quite different. It says that the details smeared by the averaging are not lost but appear on the average as though they were a transport of the substance by diffusion. In the instantaneous point form the only diffusion is molecular. This apparent diffusion, created by averaging, is usually much much greater. It is one of the most difficult problems introduced by averaging since just how big it is depends on how the averaging is done; and how it works depends on how the smoothed out details of the motion are related to the averaged motion. After all, we average in order to get rid of the details and here are their effects right back again in another disguise.

The art of the modeler is shown in the way he relates this apparent diffusion to the averaged motion. How he is to do it is something we don't yet understand very well. It needs a lot more work.

Models usually use both approximation and averaging. They are often built by first neglecting one or more processes and then further simplified by averaging to smooth out the picture. Whether the result is a good model, a satisfactory model, or even a model at all, can be determined only by checking to see whether it reproduces the features of the thing modeled.

2.3 *Scales of averaging and observation and their effects on prediction*

All instruments have scales, the intervals of time and space over which they sense. In other words, instruments do not give instantaneous point measures but rather "averages" over more or less restricted regions of space and time. Oceanographers sometimes use these *instrument scales* which determine the smallest features that *can* be seen but more often they find them too detailed and too responsive. Scientists smear the detail and coarsen what can be seen further by averaging, i.e., they put the data through low-pass filters. The averages chosen reduce our ability to sense fluctuations in the quantities we measure. They impose scales, which we may call *measurement scales*, below which we can no longer detect changes in the quantities measured.

There are also upper limits which we may call observation scales. They are just as ubiquitous as instrument scales or measurement scales although they are seldom explicitly formulated or discussed. *Observation scales* are the largest volume of space and the longest period of time over which we continue our observations. The observation scales must be larger than the measurement scales--which as a lower limit are instrument scales--but, once that restriction is met the choice of observation scales depends on what use the observer plans to make of his data. Characteristically, a man using a hot-wire anemometer picks the volume of the working section of his wind tunnel, perhaps 12 cubic feet, and a period of at most a few hours. For comparison, the meteorologist works with the "weather net." The observational time scale is at most about a century, the period during which systematic weather observations have been made. The corresponding "instrument" scale is of the order of one hour since observations are ordinarily made each hour. Changes more rapid than hourly can't be "seen." The spacing of observation stations in the net provides an "instrument" space scale by defining the size of the "particle" of fluid with which the meteorologist can work. The observational volume scale covers the surface of the globe in a patchy way and extends to the height of routine use of radiosonde balloons.

One must know both the maximum and minimum scales applicable to any model since the properties and even the laws which govern them may change radically with a change of scale. The models and the kinds of predictions that can be made from them also change just as radically as do the laws.

As an example, consider studies of the earth's atmosphere. There is a whole heirarchy of scales on which it is modeled.

The largest scale, the *planetary scale*, is earth sized and, at that scale, the atmosphere is a very thin shell as compared with the earth's radius. The horizontal gradients of atmospheric properties are small but their vertical gradients are large. The best size "particle" for modeling is long and flat, say 5000 x 5000 x 1 cubic kilometers. The maximum volume is that of the earth and its atmosphere. For time scales a reasonable minimum is a year or two while the maximum is, unfortunately, what we are stuck with; the length of time since we began to make decent systematic weather records. On these scales the properties of the earth's atmosphere are very simple. The atmosphere is a thin layer rotating with the earth at a constant rate. It has large vertical gradients of temperature and pressure. Relative to axes fixed to the earth the law governing the motion is inordinately simple: merely the velocity is identically zero.

The next smaller scale is appropriate to the study of the *general atmospheric circulation*. A reasonable "particle" size is now something like 1000 km x 1000 km horizontally by 100 m vertically. For time scales we should go to a minimum of a month and for the maximum we are still stuck with that 100 years or less of available record. At these scales we begin to see motions of the atmosphere relative to axes fixed in the earth, i.e., we see the general circulation. Our previous velocity law is now false, or better, inapplicable, simply because of the change in scale. Now we have that the horizontal component of velocity is not necessarily zero. However, the vertical component of the velocity is still zero.

Still smaller is the *meteorological scale*. Let it be the smallest made possible by the net of the World Meteorological Association. The "particle" is still flat; something like 10 km x 10 km x 10 m at the very best. The minimum time is 1 hour corresponding to the hourly observations and the maximum time is about 40 years, the period since hourly observations were initiated. On these scales we see the atmosphere as the meteorologist sees it. There are air motions additional to the general circulation. In particular, the wind, which was invisible on the previous scales, emerges. Wind is, by definition, horizontal so that we will still have a zero vertical velocity component but horizontal turbulence enters the problem for the first time.

If the scale is again reduced to the *aerological scale*, the "particle" at last begins to be more cubical and is of the order of a few meters on a side. The maximum volume on aerological scales runs around 100 to 200 cubic kilometers. The minimum times are around a minute. This is a sort of shortest time interval in which cloud formations usually show perceptible motions. Here, time lapse photography helps us to see the motions which turn out to be quite simple, e.g., cellular rotation. The maximum time is the time it takes an observer to get tired of observing--barring antlike dedication, a few hours. When we look at the atmosphere at the aerological scale, which being "man sized" is how you, as a person, see it directly, we see nothing but turbulent motions. They appear so complex that the prospect of trying to formulate laws and models is most discouraging. The motion appears to be random and all three velocity components may be non-zero.

Still smaller scales which we will not go on to discuss are the *aerodynamic scale* and the *molecular scale*.

In summary: the physical laws appropriate to the organization of a body of observations are critically dependent on the scales at which the observations were made. In every case, before we begin to discuss, model, or predict we must understand clearly:

Measurement Scales

The minimum volume which determines the size and shape of the "particle" with which we work. The minimum time which is either the smallest response time of the instruments used or the period of the most rapid fluctuation passed by the averaging process.

Observation Scales

The maximum volume defines the region of space over which the observations extend. The maximum time is the duration of the observations.

The message for modeling in general and modeling of transport processes in estuaries in particular is clear. You can not model anything unless you have first measured it. How you measure it, the measurement and observation scales, control the kind of model you can build. The processes in estuaries are of many scales, some of them very long. For example, there is often an annual cycle related to the seasonal variations in river discharge and solar radiation. If your observational scale is only one month, you have absolutely no hope of modeling the annual cycle. At least a year of data is the minimum and 10 to 20 years is more like it. Again, if the measurement space scale is an average over 10 mile squares, there is no reasonable prediction that can be made of the course of an oil spill of smaller dimensions and no point in asking for a prediction of its detailed progress. Again, if your observational scales cover only New York Harbor, don't come around asking for predictions for the New York Bight.

It is nothing short of tragic that no agency of the government has seen fit to support the collection of estuarine data on the extended and detailed scales necessary for the construction of the models upon which satisfactory prediction can be based. We now find ourselves with the problems which were foreseen and with little or nothing with which to work. About all that can be said is that we ought to get at the job--right now. We'll never start sooner.

3. ANALYTIC AND NUMERICAL MODELS

3.1 *The equations*

The best formulations of the kinematic and dynamic equations which serve as a basis for estuarine models are three-dimensional in space and time dependent. They contain terms representing all the physical processes at work. Thus, they are complete.

However, to use them for particular estuaries and, in the face of our meager data, they must be averaged. What emerges is strongly affected by the averaging methods chosen. Further, and more important, averaging creates additional terms, the nonadvective flux terms, e.g., the Reynolds flux of momentum, whose forms are not determined *a priori* by the physics of the flow. The principal difficulty with estuarine modeling centers on these nonadvective flux terms. As artifacts of the averaging, not only are they heavily dependent on the choice of average, but they are critically dependent on the choice of measurement scales in both space and time. Since the non-advective fluxes are not controlled by the physics of the flow they must be explored empirically. And that means adequate and properly taken data.

It is axiomatic in fluid mechanics that our understanding of real flows depends on increasing our knowledge of those terms not determined by the physics. For example, the Reynolds momentum-flux terms depend on variance and covariance functions of the instantaneous fluctuations of the velocity components from their mean values. Except in highly restricted flows unlikely to occur in an estuary, they are complicated functions of position and time (x_1, x_2, x_3, t) as well as of space and time separations (r_1, r_2, r_3, τ). For many years it has been common practice to assume that they could be expressed as a product of a constant, the "eddy" coefficient, and the gradient of the mean; a Fickian assumption. That such a form for the Reynolds momentum flux is inadequate to represent the complexity of the covariance function in general is so glaringly obvious as to scarcely call for comment. It is simply wrong and it's use lends a nightmare quality to our perception of the real world. But for all that, it continues to be used *faute de mieux*. We must measure the nonadvective fluxes sufficiently well to intuit a better form than the Fickian assumption. Only then will we be able to make a closer, more useful contact with reality and, incidentally, to retire the "eddy" coefficient to the intellectual junk heap where it so richly deserves to rest.

The time scales are somewhat easier to explore than are the space scales. An instrument can be mounted and kept running for a considerable length of time. When this was done in the Patuxent and the Potomac by Elliott (1, 2, 3, 4, 5) the variability characteristic of an estuary became very evident. In contrast with the open ocean, measurements of any property within an estuary fluctuate much more widely and rapidly with time. Estuaries seldom reach steady state and it is very difficult to guess what an estuary's condition will be at any instant. Elliott (1, 2, 3, 4, 5) shows that during the year of his study, the Potomac approximated one or another of six different estuarine circulation types but that more than half the time it didn't look like any of the classical types.

Spatial variations in an estuary are also highly irregular in comparison with the open ocean. The space scales are harder to study since they require simultaneous measurement by many instruments at many positions. We really know far too little about the spatial variation. If an estuary shows little sectional variation laterally and with depth, then it is attractive to average properties in sections and attempt a one-dimensional longitudinal model. If the section has little longitudinal variation but does show depth dependent variation, perhaps a two-dimensional longitudinal-depth model is in order. Similarly, if the estuary is very wide and shallow with little variation with depth, as in the case of Corpus Christi, Texas, one might try a two-dimensional longitudinal-lateral model. Where the variation is marked in all three dimensions only a full three-dimensional model is really useful.

We do not know enough to say *a priori* when a model of reduced dimensionality will be satisfactory and useful. In practice we have advanced only to the use of two-dimensional models. There are three pragmatic considerations which account for this. The complexity of three-dimensional models makes it very hard to gather the data with which they may be verified and tested. The computer costs are high for three-dimensional models. Finally, when nobody knows anything much perhaps you can find something or other that will work from a simple approach. But models with reduced dimensionalities are not adequate. Evidence is beginning to accumulate that suggests that the three-dimensionality plays an essential role in all estuaries. Even strongly stratified estuaries and wide shallow estuaries can not be properly explained by models with less than the three spatial dimensions.

3.2 *The Boundary Conditions*

In addition to the kinematic and dynamic equations every model must prescribe boundary conditions. These are inherent parts of the model and, when averaging is applied to the equations, similar specifications must be applied to the boundary conditions in order to ensure compatible resolution in space and time.

During periods when data are being taken for the construction of a model, the boundary conditions must also be measured. During periods when data to test a model are gathered, the boundary conditions must also be measured. When a model is used for prediction the boundary conditions must also be predicted. How well a model will predict depends critically on how accurately the boundary conditions can be predicted. When a model is used to explore the range of estuarine behavior, the boundary conditions must be hypothesized.

The boundary conditions relevent to an estuary are:

(1) The freshwater flow to the estuary.

These flows include the flux of water at the head of the estuary. If rivers enter the reach, their fluxes must also be known. In some estuaries there may be an appreciable flux of ground water through the bottom. Evaporation and precipitation are, effectively, freshwater fluxes out of and into the estuary through the surface, respectively.

Freshwater fluxes from rivers are perhaps the best known since many rivers are gaged. The trouble is that the gages are usually well above the head of the estuary leaving substantial areas uncovered. This won't do. We must measure the freshwater fluxes where we need to know them; not in the next county.

All of these freshwater fluxes are time dependent and usually highly variable.

(2) The rise and fall of surface elevation on a line across the mouth of the estuary.

This is often supplied by one tide gage or by two tide gages located on the opposite shores; the elevations across the mouth being inferred from theory or simply assumed to follow some conveniently simple function. Again, the condition is time dependent and variable.

(3) The spatial distribution of salinity as a function of time across a section at the mouth of the estuary.

Present models often use some artifice such as assuming that the salinity present at the mouth is simply advected seaward during ebb current and then jumping it to full sea water on the section when the current changes to flood. This kind of thing won't do. The spatial and temporal behavior of the boundary salinity must be measured at a level of resolution compatible with the level used for the body of the estuary.

(4) The wind stress on the surface of the estuary as a function of horizontal position and time.

It is probable that we know less about this boundary condition than about any other. But we know that it can be important. In shallow estuaries it can produce water surface elevation changes much larger than the astronomical tide. Depending on the wind, as it does, it is very highly variable in both space and time. For the most part, while it is included in the equations, it is ignored in the models. We can't measure wind stress on the water surface directly but, at the very least, good wind velocity measurements should be made. They alone won't solve the problem since the mechanism by which air motion transfers stress to the water surface is not well understood. There are forms for the drag which can be used but they may be inadequate. Like the nonadvective fluxes, the wind stress on the surface is one of those things not determined *a priori* by the physics of the flow. Still, measurement of the wind where it matters--over the water; not some irrelevant five miles away at an airport--should give us more than simply turning our backs on an important forcing function.

(5) The bottom configuration of the estuary must be known.

In some estuaries it may be possible to consider the bottom as a rigid container but it is actually a slowly varying function of time. Whether it is a rigid boundary or an elastic one, permeable or impermeable, may also need consideration. Of one thing we must be particularly careful: the spatial resolution. If the resolution is too coarse to define the essential topography well enough, subscale bathymetric quirks of individual estuaries will appear falsely as dynamic effects thus rendering the model useless in any estuary save the one for which it was constructed. A knowledge of the frictional drag on the bottom is just as necessary as a knowledge of the wind stress on the surface. It, too, is not determined *a priori* by the physics and the same sorts of difficulties arise.

To sum up; our knowledge of the boundary conditions is inadequate. Many of the models, as actually worked out, do not include their critical time dependence. Those that do include it do so only in the most simple and unrealistic way. Here is an area of ignorance and omission that must be made good before progress in estuarine modeling can be expected.

3.3 *Verification and testing*

All models should be verified. That is to say that they should be able to reproduce the data used to construct them tolerably well and that they should be shown to do so.

Any model in which we are to repose much confidence should also be tested. Once it has been made and is in existence, at least one more set of data of the sort on which it was verified and from the same estuary should be taken. The model, if it is to be taken seriously as a model, should do just as well at reproducing the second set of data; and that without further modification.

No model which has not been both verified and tested can be considered anything but "work in progress." It is something of a scandal that none of the "models" we now have has been either verified or tested in its complete form. The data with which to do so have never been taken.

3.4 *The areas to be addressed*

The problems which urgently need attention fall under three general heads:

(1) A better comprehension of the forms of the terms in the averaged equations and boundary conditions which are not determined *a priori* by the physics.

(2) A better definition of the boundary conditions as they actually exist.

(3) A clearer idea of the variability in both space and time of the properties and processes occurring within and on the boundaries of estuaries.

Progress in all of these areas turns on the selection of averaging methods, measurement scales (resolution limits), and observation scales (extension limits). Field experiments will be necessary.

4. FIELD EXPERIMENTS

The field experiments required to resolve problems in the three areas and to build a useful three-dimensional model for estuaries are of two sizes: small- to intermediate-scale experiments and large-scale experiments.

4.1 *Small-scale experiments*

The primary purpose of small-scale experiments is to obtain direct and indirect measures of the nonadvective fluxes of salt and momentum in all three spatial directions, horizontal and vertical. Proper measurements of all boundary conditions must be made during the course of each experiment. From these data we can get:

(1) A more realistic form for the nonadvective flux terms.

(2) A better partition between advective and nonadvective fluxes as functions of the averaging methods and of the time and space measurement-scales.

The experiments, discussed in more detail in section 5.1, will consist of enclosing a carefully selected, relatively modest volume of an estuary (the spatial observation scale) with a ring of sensors. Interior to this ring another ring of sensors will be disposed. At the center of the array a station instrumented with the most responsive sensors the state of the art provides will be located. The measurements from them will be recorded at instrument scales.

Among the questions for which answers can be expected are:

(1) What is the partition between advective and nonadvective fluxes as a function of the time average selected and the time resolution chosen?

(2) What is the partition between advective and nonadvective fluxes as a function of the spatial average selected and the spatial resolution chosen?

(3) What are the best empirically determined mathematical forms for the nonadvective flux terms temporally?

(4) What are the best empirically determined mathematical forms for the nonadvective flux terms spatially?

(5) What relations between the nonadvective and advective fluxes exist? How can they best be parameterized?

(6) What relations exist between the nonadvective fluxes and the bulk parameters of the estuary, e.g., the Richardson number?

(7) What relationships and values are reasonable among the coefficients which appear in models?

It is clear that these small- to intermediate-scale experiments will shed light on all three problem areas listed in section 3.4.

The problem of the boundary conditions is of a somewhat different kind. They either force or modify the flow within the experimental volume. We must measure them every time our sensor array is in operation if we are to have any hopes of interpreting our data clearly. Guessing at the boundary conditions will vitiate the experiment.

4.2 *Large-scale experiments*

The large-scale experiments extend the spatial observation scale from a "modest volume" to an entire estuary from head to mouth. Their primary purposes are to *construct*, *verify*, and *test* an improved model for an estuary based on the results from the small- to intermediate-scale experiments and to explore the conditions and field-data effort required to transfer a model from the estuary for which it was constructed to another.

As a practical consideration, the instruments necessary for the small- to intermediate-scale experiments will serve equally well for the large-scale experiments. Few additional instruments are required; only redeployment will be necessary.

An estuary will be selected for modeling and divided into five reaches by six sections; one at the head, one at the mouth, and the remaining four at intermediate positions. The estuary initially selected may be either partially mixed or highly stratified (including fjords). The first set of data taken from it will be used to construct the model and verify it. When this has been done, a second set of data will be taken and the model tested.

At the second stage another estuary somewhat different from the first will be chosen and data gathered. These data will be used initially to test the model exactly as it came from the first estuary. If no adjustment of the coefficients is required for satisfactory performance, the transferability of the model to the second estuary will have been established. If adjustments of the coefficients are required, they will be made and the adjusted model verified. A second set of data collected in the second estuary will be used to test the adjusted model.

At the third stage, an estuary quite different from the first and second choices will be selected. The sequence of estuaries can be taken in any order. We could begin with a well mixed estuary and finish with a highly stratified estuary; or the other way around. Again we will take data and test our model from the first estuary and our adjusted model from the second estuary. Again, another set of data will be taken for testing our adjusted third model.

What will we have at the end of this sequence of large-scale experiments? First, we will have models for three estuaries. These models will have much closer contact with the real world than any present models. Most important, they will have been both verified and tested and can thus be expected to perform reliably. Second, we will have gained valuable experience with the transferability problem. Should the model for our first estuary not require adjustment for use in our second estuary, we will have immediately established a range of applicability. Should adjustment be required, we will have the means of judging the minimum field effort required to gather data on which the adjustment could have been made. This will establish the conditions for transferability to many other estuaries. Third, we will have comparative studies of the full range of estuaries and so be able to move more quickly and efficiently to the solution of particular problems in individual estuaries.

4.3 *Some comments and caveats*

The small- to large-scale experiments suggested here collectively represent *one* experiment which will go a long way toward resolving the most troublesome deficiencies in our knowledge of transport processes in real estuaries. But it is important not to conceive of this as a great monolithic set piece which, once launched, is to go thundering along a prescribed track with results only at the end of it all. It is rather a step by step process. At each step computing and analysis must be swift enough to guide the next step. Each step will yield results of value but the full value of each can be realized only in relation to the whole.

We repeat: the boundary conditions must be properly measured at each step.

There are many experiments which might be run with mutual profit at the same times and in the same places as the proposed experiments. For example, diffusion has been studied with both Eulerian and Lagrangian measurement techniques. Much data exists. Unfortunately, statistical theory has, as yet, only the most rudimentary information on how to relate the statistics of one view to the statistics of the other. Studies using dye or drogues made concurrently with the small- to intermediate-scale experiments could give the empirical base for improved relations between Eulerian and Lagrangian information; thus making data in hand more useful.

5. A SKETCH OF THE SHAPE, SIZE, AND COST OF THE FIELD EXPERIMENT

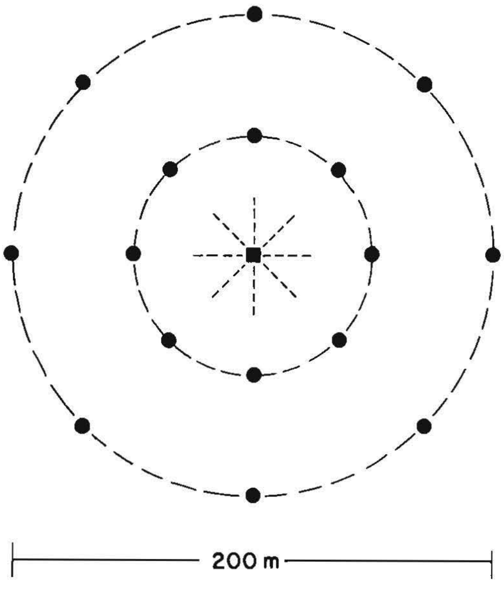
The shape, size, and cost of the field experiment can best be communicated by sketching a possibility. But it must be remembered that this is only a sketch and a very incomplete one. Specific arrangements and particular places will be named but only as examples in order to make a concrete picture. This is not a proposal nor are the costs a budget. Both are matters for consideration and decision by the scientific community during the planning period which must precede the experiment.

Let us suppose that the Delaware has been selected as the first estuary to model, Long Island Sound as the second, and the Duwamish as the third. This gives a sequence from partially mixed with classic geometry through partially mixed with aberrant geometry to highly stratified.

5.1 *The nonadvective flux experiments*

To have a brief name for the small- to intermediate-scale experiments we will call them FLUX 1 and FLUX 2. FLUX 1 and 2 could be carried out in the Delaware since their results will be applied there during the first large-scale experiments. However, they could as well be done in any convenient partially mixed estuary. The space scales are to be modest and the idea in any case is to avoid local idiosyncrasies which could induce non-transferable results. An area with a level bottom, or at most gentle relief, with no nearby abrupt features would be suitable. The water should be 15 to 20 meters deep and shoal water at the sides at least a kilometer away from the instrument array.

The instrument array should "enclose" a volume of water 200 meters across and extending from surface to bottom. It's precise shape and the disposition of the instruments is a matter for the planning stage with modifications as the results become available. It might look like this.



There are eight stations in the outer ring, eight stations in the inner ring, and one central station. Each ring station is to carry at each of five depths a recording velocity-salinity-temperature meter of the ANDERA/ENDECO types. The central station will be a fixed tower mounting the fastest response 3-component velocity, salinity, temperature sensors that can be kept functioning under field conditions, instruments such as those developed and used by Smith (6). In conjunction with the array, 10 paper-tape recording tide gages, one meteorological buoy station, and four shore-based meteorological stations will be installed and operated.

The array and its associated instruments will be operated continuously for a month at a time. During periods of operation a barge with living and working accommodations for a technician, tape recorders, monitoring devices, repair facilities, and power supply will be anchored with the array.

FLUX 1 will consist of deploying the array, operating it continuously for one month (Which month is immaterial.), performing the necessary calculations, and analyzing the results for answers to the questions posed. Analysis of the dispositions and operation of the array during FLUX 1 will also be made for guidance in data gathering during FLUX 2.

Upon completion of FLUX 1, and not before, the array will be modified as indicated and FLUX 2 will repeat the pattern of FLUX 1.

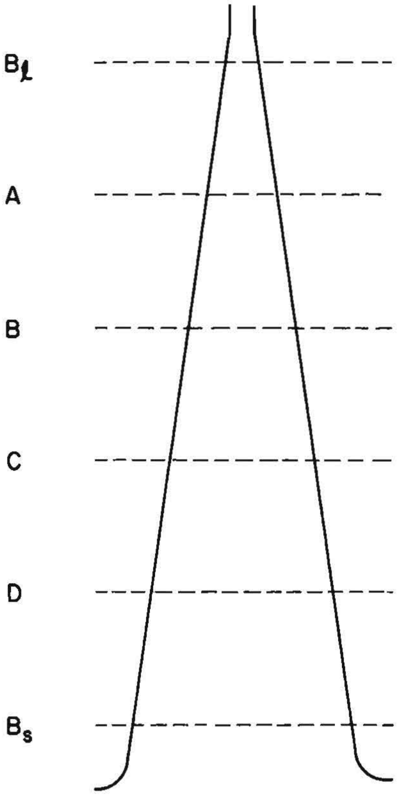
The analyses of FLUX 1 and 2 will give us the forms, partitions, and relations necessary for constructing, verifying, and testing an improved three-dimensional estuarine model.

5.2 *The estuarine model experiments*

The brief name for the large-scale experiments which will give us verified and tested three-dimensional models for three estuaries will be EMEX 1, 2, 3, 4, 5, and 6.

If we use the Delaware for FLUX 1 and 2, then the Delaware will be used for

EMEX 1 and 2. The estuary will be divided into five reaches by six sections along which the instruments used in FLUX 1 and 2 will be deployed and operated continuously for a month at a time, the month chosen not being critical. The landward and seaward sections B_ℓ and B_s, must be occupied during each data collecting period but all the internal sections need not be occupied simultaneously. During EMEX 1, for example, we could occupy B_ℓ, A, B, and B_s for one month followed by B_ℓ, C, D, and B_s for another month.



On section B_ℓ, which will be located above the limit of sea-salt intrusion, the primary measurement will be of the fresh water volume flux. At all other sections, surface elevation, salinity, temperature, and velocity will be measured. Meteorological measurements will be made during all periods of operation.

With the data in hand from EMEX 1 a three-dimensional model of the Delaware will be constructed and verified against the data. We can then proceed to EMEX 2.

EMEX 2 will repeat EMEX 1 and the data used to test the model.

For EMEX 3 the data acquisition pattern of EMEX 1 will be repeated in Long Island Sound and the data used to test the model from EMEX 1. One of two outcomes may be anticipated:

(1) The model of EMEX 1 for the Delaware performs satisfactorily for Long Island Sound without modification.

(2) The model of EMEX 1 for the Delaware requires adjustment of its coefficients for satisfactory performance in Long Island Sound.

Should outcome (1) materialize, there will be no need for EMEX 4. The data from EMEX 3 will test the model. The effort planned for EMEX 4 could be avoided or, perhaps invested in another estuary even further removed from the type of the Delaware, say Corpus Christi.

Should outcome (2) materialize, the data from EMEX 3 would be used to adjust the coefficients and to verify the adjusted model.

EMEX 4 would then be similar to EMEX 2 and the data used to test the adjusted model for Long Island Sound.

EMEX 5 and 6 would repeat the pattern of EMEX 3 and 4 in the Duwamish, a strongly stratified estuary.

At each stage from EMEX 1 through EMEX 5 computation and analysis will be completed before the next stage is undertaken and the results used to guide the ensuing stage.

In the end we will have three-dimensional models tested and verified for the Delaware, Long Island Sound, and the Duwamish which represent a wide range of estuarine conditions. We will have also learned what adjustments are necessary to adapt such models to particular estuaries. It is likely that the full weight of the field data necessary for EMEX 1 through 6 will not be required. We can hope to be able to say with some confidence what field measurements are enough for model transfers.

5.3 *The means*

The trained men, the supporting services, the means of inter-institutional cooperation, and the interest of the scientific community are all available for this experiment. What is lacking are the instruments and the money.

5.3.1 Instruments and Hardware

Required by both FLUX and EMEX.

Number	Description	Estimated Unit Cost ($)	Total Cost ($)
100	salinity-temperature-velocity sensors	8,000	800,000
10	tide gages	2,500	25,000
1	meteorology buoy station	10,000	10,000
4	meteorology shore station	5,000	20,000
20	mooring systems	750	15,000
2	ENDECO-type readers	5,000	10,000
1	paper-tape reader, tide gage	3,000	3,000
1	positioning system, electronic	26,000	26,000
			909,000
	For the FLUX Central Tower		
6	high resolution 3-component velocity instruments		
6	compatible high resolution instruments for salinity and temperature		
1	Attendant barge with living and working space for a technician, tape recorders, power supply, monitoring and data logging devices for all instruments of the array, and repair facilities		
		Estimated cost:	100,000
		Total:	$1,009,000

5.3.2 Schedule, Salaries, and Ship Time

Year	Stage	Ship Time (days)	Senior Scientists No.	Senior Scientists Base Salary ($×10^3)	Assoc. Scientists No.	Assoc. Scientists Base Salary ($×10^3)	Scientific Support No.	Scientific Support Base Salary ($×10^3)	Administrative No.	Administrative Base Salary ($×10^3)	Totals No.	Totals Base Salary ($×10^3)
1	Planning, Instrument acquisition testing and calibration	10	1	30	3	20	3	15	2	11	9	157
2	FLUX 1 & 2	60	2	30	5	20	4	15	3	11	14	253
3	EMEX 1 & 2	60	2	30	5	20	4	15	3	11	14	253
4	EMEX 3 & 4	60	2	30	5	20	4	15	3	11	14	253
5	EMEX 5 & 6	60	2	30	5	20	4	15	3	11	14	253
		250	9	30	23	20	19	15	14	11	65	
				270		460		285		154		1169

If we allow a factor of 1.8 as reasonable for overhead and fringe benefits, the total for gross salaries will be	$2,104,200
250 days of ship time estimated at $1000 per day gives	250,000
Total:	$2,354,200

5.3.3 Other Costs

It is estimated that computer services during the first year will cost about $10,000. Thereafter they are expected to run at $50,000 per year.	$ 210,000
Travel associated with the planning and execution of the experiment is estimated at $10,000 per year	50,000
Total:	$ 260,000
Grand Total:	$3,623,200

We are thus talking about an experiment that will take five years to complete, require 65 man-years of work, and cost on the order of 4 to 5 million dollars.

6. CONCLUSION

What needs to be done is clear. The key to adequate prediction of estuarine transport processes is in the nonadvective flux terms; those not specified *a priori* by the physics of the flow. We must develop more adequate forms for them empirically. We must relate them to the advective fluxes. We must parameterize them properly and relate them to the bulk properties of estuaries. A successful attack on these questions will require careful attention to methods of averaging and to the choice of instrument, measurement, and observation scales in both space and time. Completion of these tasks will be a major step forward in our scientific understanding of estuarine transport processes.

However, increased scientific insight is only the first requirement. The second is to use our better grasp of estuarine transport processes as a basis for the construction of improved three-dimensional estuarine models. These models must be both verified and tested if we are to have confidence in what they tell us about the real world. Only such models give the factual foundation that is a sure aid to management decisions.

The third requirement is to explore model transferability. Under what conditions can a model fitted to one estuary and successful as a predictor in that estuary be used for another estuary? What adjustments are necessary for transfer? How much field data and of what kinds must be gathered to make the adjustments?

This, then, is the job to be done. We know how to do it. With proper support, the men, ships, and institutional capacity are available to do it. It will take something like five years and the support required will be between 4 and 5 million dollars.

Will it be worth doing from a practical standpoint? On past experience the answer is an unequivocal "Yes." "Guidelines for Evaluating Estuary Studies, Models and Comprehensive Planning Alternatives" issued by the U.S. Department of the Army, August 1969 lists eight projects in which hydraulic model studies whose costs were comparatively small guided management decisions which improved project design and saved millions of dollars. They were

(1) The Columbia River Entrance,
(2) The Columbia River Entrance, Jetty B,
(3) The Columbia River, Wauna-Lower Westport Bars,
(4) The Delaware River Dikes,
(5) Narragansett Bay,
(6) St. Johns River, Florida,
(7) Galveston Harbor Entrance,
and (8) Lake Pontchartrain, Louisiana.

Hydraulic models are highly specific; a separate hydraulic model must be built for each estuary studied. They are cumbersome and expensive and, most important, not easily transferable. Mathematical models are much more flexible. For a good one, only the computer input needs to be changed to move it from one estuary to another. Mathematical models do not replace hydraulic models; they complement them. As a tool for management, improved mathematical models have the potential of extending the benefits already realized from hydraulic modeling to a much wider range of estuaries at much less effort and expense.

Another management area to which improved, reliable mathematical models could make an important contribution is water quality monitoring. Much effort is currently expended on gathering water quality data. Unfortunately much of it may be of little value; even for the purposes of the agencies which collect it. Good mathematical models could provide guidance for water quality monitoring that would make it both more effective and less costly.

Our conclusion is that the problems

discussed in this report should be taken in hand as soon as possible. We feel that the benefits, both in increased scientific understanding and in more powerful management tools, far outweigh the costs of the undertaking.

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APPENDIX A

Organizations Supporting the Workshop on Transport Processes in Estuaries

United States Environmental Protection Agency
United States Energy Research and Development Administration
Office of Naval Research: Geography Branch
National Oceanic and Atmospheric Administration: MESA, New York Bight Project
United States Fish and Wildlife Service: Office of Biological Services
Stony Brook Foundation

APPENDIX B

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APPENDIX B
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