Report of the MWRA Hydrodynamic and Water-Quality Model Evaluation Group

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Final Report

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1. INTRODUCTION

The Massachusetts Water Resources Authority (MWRA) is in the process of building a new sewage treatment plant with offshore outfall. As part of their research effort to understand and predict the impact of these facilities on the Massachusetts marine environment (including the recovery of Boston Harbor), the MWRA contracted with the United States Geological Survey (USGS) and HydroQual, Inc to develop hydrodynamic and water-quality models for the Massachusetts Bay/Cape Cod Bay region. Specifically, in March, 1991, Dr. R. Signell and Dr. H. Jenter (USGS) and Dr. A. Blumberg (HydroQual) were tasked to develop a three-dimensional (3-D) ocean circulation model for the Bays which could be used to generate the necessary temperature, salinity, velocity, and other fields needed to run a regional water-quality model to be developed by Dr. D. DiToro and Mr. J. Fitzpatrick (HydroQual). The water-quality model could then be used to predict the impact of MWRA changes due to treatment upgrades and outfall relocation on water quality within the Bays system.

As an historical note, this modeling effort follows previous modeling studies conducted in the late 1980's by the MWRA in support of their Secondary Treatment Facilities Plan (STFP) and by the U.S. Environmental Protection Agency (EPA) in support of the corresponding Supplemental Environmental Impact Statement (MWRA, 1988; U.S. EPA, 1988). These earlier models were useful in outfall site selection but, because they were two-dimensional (2-D) (depth-averaged), they omitted important physics associated with summertime stratification. Also, their applications were limited to conservative, or simply-decaying substances characterized by first order decay/boundary loss. A fully coupled hydrodynamic and water-quality model had never been attempted for the region. The growing availability of such models for environmental management of coastal water quality made it appropriate for MWRA to take this step.

The Massachusetts Bay Model Evaluation Group (MEG) was formed in September, 1992 to provide advice to the MWRA and its two contractors (USGS and HydroQual) about their model development effort. The MEG has seven members with varied expertise (see Appendix A), and met in open session nine times between September, 1992 and May, 1995 (see Appendix B for meeting minutes and attendance). At these meetings, the MEG would review recent work by USGS and HydroQual and make suggestions to improve the overall quality of the models. The MEG held one final closed meeting on September 15, 1995 to finalize this report.

In this report, the MEG presents a brief review of the USGS and HydroQual modeling efforts and evaluates the models' ability to predict regional water quality and potential changes in water quality associated with the treatment plant upgrade and outfall relocation. The review is based on the following reports:

- "Circulation and effluent dilution modeling in Massachusetts Bay: Model implementation, verification, and results" submitted September 15, 1995 by R. Signell and H. Jenter (USGS) and A. Blumberg (HydroQual);
- "A Water-Quality Model for Massachusetts and Cape Cod Bays: Calibration of the Bays Eutrophication Model (BEM)" submitted June 30, 1995 by HydroQual and Normandeau Associates, Inc.;
- "Modeling Near Field Plume Behavior using a Far Field Circulation Model" submitted February 7, 1995 by HydroQual;

as well as discussions with USGS, HydroQual, and MWRA scientists.

This review adapts the conceptual framework for environmental model evaluation presented by Ditmars *et al.* (1987). The next section summarizes the problem that motivated the model development, followed by a section giving an overview of the models and their application to Massachusetts and Cape Cod Bays. Section 4 describes how model sensitivity was tested, calibration parameters were estimated, and the models were validated. Finally, the MEG conclusions and recommendations are presented in Section 5.

2. STATEMENT OF PROBLEM

In order to improve water quality in Boston Harbor and adjacent waters, the MWRA has started to (a) upgrade to secondary treatment at its principal facility at Deer Island, and (b) move the sewer outfall locations from their positions off Deer and Nut Islands eastward about 15 km from Deer Island into Massachusetts Bay. A key management question is, what impact will these changes in loading have on regional water quality and the recovery of Boston Harbor? Key indices of water quality include the concentrations of nutrients, particularly nitrogen, in marine environments, phytoplankton (chlorophyll concentration), dissolved oxygen (DO), and the flux of particulate organic matter to the sediments. DO is a particularly important water quality parameter since it is used to determine compliance with state and federal clean water requirements. For this reason, the primary use of the water-quality model is to predict changes in water quality associated with different nutrient loading scenarios. It is helpful to separate the problem statement into two separate, but related parts:

- develop an improved general understanding of nutrient/phytoplankton/dissolved-oxygen dynamics in the Bays, and
- predict specific effects of the new treatment plant and outfall.

A number of physical, chemical, and biological processes strongly influence nutrient/phytoplankton/DO dynamics: surface forcing (especially insolation, wind stress, and buoyancy flux), vertical stratification (especially temperature), wind- and buoyancy-driven circulation, vertical mixing, primary production (especially the spring bloom), respiration by bottom-dwelling animals, and bottom sediment/water column interactions. Since the semi-diurnal tidal currents drive the flushing of Boston Harbor (Signell and Butman, 1992) and cause significant mixing in the shallower parts of the Bays, a regional water-quality model should include and resolve currents with semi-diurnal and lower frequencies, especially synoptic and seasonal. The western Gulf of Maine is the immediate upstream source of the ocean water entering the Bays, so that the circulation and water quality in the Bays can not be examined in complete isolation from the Gulf of Maine.

3. MODEL OVERVIEW AND RELATIONSHIP OF MODELS TO PROBLEM

The USGS Bays circulation model is based on the Blumberg and Mellor (1987) three-dimensional Estuary, Coastal and Ocean Model (ECOM). This model has a free surface that allows simulation of the tides, nonlinear advection, coupled density and flow fields, and vertical mixing which is parameterized using the Mellor and Yamada (1982) second order turbulent closure scheme as modified by Galperin *et al.* (1988). The model is driven by boundary forcing (both surface and lateral) and predicts surface elevation, circulation, stratification, and mixing coefficients as a function of time. This basic model is considered to be state-of-the-art, and has been extensively tested in over 30 studies of estuarine and coastal ocean regions including Chesapeake Bay (Blumberg and Goodrich, 1990), the Gulf Stream Region (Ezer and Mellor, 1992), Long Island Sound (Schmalz, 1994), and Georges Bank (Chen *et al.*, 1995).

The circulation model utilizes orthogonal curvilinear coordinates in the horizontal plane and a sigma-coordinate in the vertical. The model grid extends east of Stellwagen Bank and north of Cape Ann to include the Merrimack River, with horizontal- and vertical-grid resolution within the Bays adequate to resolve the dominant physical processes there. While the semi-diurnal tidal forcing along the open boundary is relatively well-known (Lynch and Naimie, 1993), the lack of comprehensive synoptic hydrographic and circulation data prevent detailed description of the subtidal currents and water property fields along the open boundary. The combined use of an adjunct western Gulf of Maine circulation model, the Bedford Institute of Oceanography hydrographic climatology, observed surface forcing and river discharge, and the few current measurements available in the western Gulf of Maine represents a good approach to predict the lateral boundary conditions needed to drive the Bays circulation model.

The HydroQual Bays water-quality model takes as input the time-dependent temperature, salinity, current, and mixing coefficient fields generated by the circulation model and predicts 24 state variables, which include two types of phytoplankton, various labile and refractory components of organic matter (carbon, nitrogen, and phosphorus), and dissolved-oxygen (DO) concentration. The model incorporates a sediment nutrient flux submodel which predicts the aqueous sediment oxygen demand. The model utilizes three-dimensional mass balance equations with coefficients determined by theory, laboratory experiments, or calibration with field data from this or other sites. The HydroQual Bays water-quality model is also considered to be state-of-the-art, and has been used to examine water quality in Long Island Sound (U.S. EPA, 1994).

Both the hydrodynamic and water-quality models solve mass conservation equations which are written in conservative form and are solved on a staggered finite difference (control volume) grid which guarantees local mass conservation.

To reduce computational effort, the Bays water quality model uses both temporal averaging and spatial aggregation in the circulation model, with in general a 3x3 cluster of circulation model horizontal grid cells forming one water-quality model grid cell, vertical aggregation of the upper three sigma levels, and hourly averaging of the 1.5 to 6-minute resolution hydrodynamic model output. The resulting grid provides only coarse (about 2-km) resolution in and near Boston Harbor; however, comparisons of the flushing of a passive tracer from Boston Harbor using the circulation and water-quality models with theoretical and field data summarized by Stolzenbach and Adams (1995) indicate that the water-quality model has adequate time and space resolution to simulate the exchange of water and material between Boston Harbor and Massachusetts Bay. The water-quality model also reproduces the vertical salinity stratification predicted with the circulation model over much of the Bays, indicating that in general the effects of vertical mixing are properly accounted for in the horizontal grid aggregation.

Due in part to the severe lack of biological and chemical data along the circulation model open boundary, the water-quality model utilizes an open boundary located between Race Point (Cape Cod) and Cape Ann. The use of all available field data plus experience from other systems to develop the necessary time-dependent boundary conditions for the state variables for the water-quality model seems appropriate.

4. MODEL TESTING, CALIBRATION, AND VALIDATION

A large number of model runs were made to develop intuition about the model components, test specific model and scientific questions, estimate model parameters, and assess overall model validity. The more important of these tests are identified below under headings of hydrodynamic model, overall water-quality model, and sediment submodel. In this report, calibration means using field and other data to adjust model parameters, while model validation means comparing model results with data without changing any model coefficients to improve the model/data fit. Because of limitations in the available data, it was not possible to construct a comprehensive calibration data set and a separate validation data set for testing all of the models.

(A) HYDRODYNAMIC MODEL

The vertical sigma coordinate system used in the circulation model can lead to false flows due to finite-difference errors in computing the horizontal pressure gradient over varying topography (Haney, 1991; Mellor *et al.*, 1994), and because of simplifications in the governing equations typically used with the sigma transformation (Paul, 1994). This problem was reduced in the present calculations by periodically subtracting out the domain-averaged vertical distributions of temperature, salinity and density before computing horizontal differences, resulting in small error fields. Mellor *et al.* (1994) argue that the baroclinic pressure gradient errors will be advectively eliminated in long-time integrations like those done here. However, the work of Podber and Bedford (1994) indicates that simplifications to the governing equations commonly used with the sigma transformation may lead to inherent instabilities that could become significant, dependent on the particular application.

Horizontal and vertical diffusivities were computed by respective closure models. Horizontal mixing is parameterized using the Smagorinsky formulation, which sets the magnitude of horizontal mixing proportional to the horizontal current shear. The coefficient in the Smagorinsky formulation yielded horizontal diffusivities in the range of 5 to 20 m^2/s, consistent with literature values from Okubo (1971), and prior model calibration in western Massachusetts Bay (Walton *et al.*, 1990; Adams *et al.*, 1990). The specified minimum vertical diffusivity of 0.04 to 0.08 cm^2/s is consistent with values deduced from recent tracer studies in Massachusetts Bay (Geyer and Ledwell, 1994).

The circulation model is driven at the surface by wind stress and heat flux. The assumption of spatially uniform wind stress is appropriate for this domain. The calculation of surface heat flux based on model-computed, spatially-dependent surface temperatures is essential to obtain realistic fluxes over the Bays domain. An algorithm to incorporate this feedback was developed in the course of this study.

The hydrodynamic model was used to simulate 3-D contours of dilution resulting from the discharge of conservative tracers from the existing and future outfall locations. As expected, during winter (unstratified) conditions, the contours from the two scenarios were very similar offshore from the location of the future outfall, which is consistent with conclusions reached with the 2-D models. During summer (stratified) conditions, there were greater differences between the two outfall scenarios, but both showed high dilution offshore of the future outfall site. The 3-D hydrodynamic model showed that relatively high mean tracer concentrations (one part tracer to 200 parts seawater) were found only in a 5 to 10-km radius throughout the year, consistent with 2-D model results that suggested that the effect would be limited to a localized region about the outfall.

The model was also used to simulate chlorination failures at the existing and future outfall sites. Shoreline bacterial concentrations for the future outfall scenarios were considerably lower than corresponding concentrations for the present outfall, which is consistent with the greater flushing at the future site.

The hydrodynamic model was used to determine the mean residence time for freshwater: (1) in the Harbor from the existing outfalls and (2) in Massachusetts and Cape Cod Bays from the future outfall. In the former case, a residence time of between four and eight days was computed, with an average of five days, which is within the middle of the range of experimental and theoretical values summarized by Stolzenbach and Adams (1995). In the latter case, residence times ranged from about 40 days in the summer and early fall to around 100 days in early spring, in qualitative agreement in magnitude with field measurements provided by Geyer *et al.* (1992).

Because of the importance of temperature and light level on primary production, it is important that the model be able to represent the rise height of the future outfall plume. The hydrodynamic model replaces the vertical momentum equation with the hydrostatic approximation, and the grid spacing in the vicinity of the outfall is only about 1 km; hence the model cannot rigorously compute plume rise. Nonetheless, there is considerable evidence that the density exchange flow which is simulated by the model can lead to approximately correct plume trap heights, especially under conditions of strong stratification (X. Zhang, MIT thesis, in preparation). Analysis of model output in Massachusetts Bay suggests that predicted trap heights are in reasonable agreement with those predicted with EPA's initial mixing models. (Blumberg *et al.*, in press).

The overall hydrodynamic model was calibrated to field measurements taken in the period October 1989 to April 1991. As a result of model data comparisons discussed above, a correction was made to reduce spurious mixing resulting from the sigma coordinate transformation, the surface heat flux component was improved, and the model was coupled with a similar model of the western Gulf of Maine to provide better specification of outer boundary conditions. The model was shown to be able to reproduce seasonal and low-frequency trends in temperature, salinity and current by detailed comparison with time series data obtained at the Boston Large Navigational Buoy and additional field data. While the model reproduced most mesoscale events in winter, it tended to miss mesoscale events during strong stratification in summer. The model was then run for the period January 1992 to January 1993 using observed boundary forcing for that period but with no change in model parameters. A similar level of agreement was observed between model and data, providing a measure of model validation.

(B) WATER-QUALITY MODEL (OVERALL)

Field measurements used for model sensitivity/calibration were collected by a number of institutions between October 1989 and April 1991 and January through December 1992. Data from both periods were necessary to obtain a temporally and spatially representative data set for model calibrations. The field data were sufficient for defining major areal patterns, annual cycles, and seasonal variation in the vertical distribution of many of the state variables considered by the model. However, by using all data for examining performance and calibrating the model, there was no explicit validation step for the water-quality model in contrast to the hydrodynamic model.

During calibration, model boundary inputs and select model coefficients were iteratively adjusted until a number of ecologically important seasonal, vertical, and areal patterns that are evident in the field data were simulated. Procedures for iterative steps varied based on the aspect of the model being evaluated or adjusted, the availability of related data, and the year being simulated. For example, specification of model boundary conditions was a major effort of the calibration exercise. However, field data for the Gulf of Maine boundary was generally sparse for the two years of water-quality model calibration runs (1990 and 1992). For portions of the 1990 calibration, boundary data were unavailable; for others, lack of spatially comprehensive data dictated a simplifying assumption of spatially constant boundary conditions from Cape Ann to Race Point on Cape Cod. Vertical conditions along the boundary were specified based on patterns from limited data for some parameters. In contrast, for 1992 there were more comprehensive data, but only for stations inside the model domain. It was assumed that conditions at stations closest to the boundary were similar to the boundary, and additional adjustments were made during repeated calibration runs to improve model vs. data comparisons at these "pseudo"-boundary stations.

The accuracy or reasonableness of model results was judged primarily by inspection of annual plots of model and field data, and seasonal and annual probability distributions of observed and predicted data. Assessments generally recognized and compared sections of the Bays as distinct regions. Efforts to calibrate the water-quality model for Massachusetts Bay focused on key indicators, including concentrations of chlorophyll, particulate organic carbon, inorganic nutrients (N, P, Si), and dissolved-oxygen, as well as rates of primary production and deposition of organic matter to bottom sediments. Field data were available for comparison for all of these parameters except organic matter deposition rates.

Several features of the standard water-quality models that were developed for Chesapeake Bay and Long Island Sound were modified for the Massachusetts Bay model. Principal among these was the adoption of a variable stoichiometry formulation (Laws and Chalup, 1990) to describe phytoplankton growth and nutrient uptake. Initial model runs had a constant ratio of chlorophyll/carbon for growth, although different for each of the summer and winter groups considered in the model. Results with fixed stoichiometry were unable to capture some features of the chlorophyll, carbon, and nutrient dynamics, particularly in terms of their variations with depth in the water column. With addition of variable stoichiometry, based on theoretical considerations developed elsewhere (Laws and Chalup, 1990), simulations were better able to mimic the distribution of concentrations measured in the field. A second modification involved some fine-tuning of the submarine light field as regulated by the extinction coefficient. It was noted that the initial simulations had base (non-chlorophyll related) extinction coefficients that limited light penetration more than had been observed in the field. Using monitoring observation data, adjustments were made to better simulate light fields, by partitioning the model domain into three segments which reflected characteristically different levels of riverine and anthropogenic inputs of suspended solids (Boston Harbor, Massachusetts Bay North and South of Scituate). Further simulations then showed that the effect of changes in extinction coefficients was minor, but did improve the fit between model and data.

It is useful to classify what features are not captured by model simulations. These include some minor seasonal features of the annual cycle of chlorophyll, nutrients, and dissolved oxygen, such as the precise timing and magnitude of spring and fall blooms, but more significantly, include most "event-scale" dynamics. For example, although the seasonal spring freshet of freshwater runoff is simulated and courses through the Bay, more transient and less dramatic events which occur on time scales of days to weeks and/or at restricted locations are not well represented. It is probable that the lack of event-scale simulation is due in part to the fundamental time-scale structure of transfer rates within the model as well as necessary assumptions of constant coefficients where variable coefficients might be applicable, but data are lacking on their variability. As with time scales, finer space scales of vertical distributions and/or horizontal patchiness that are documented in nature are not simulated by the model; this, too, is a function of the basic choice of structure for the model, not a fundamental failing of theory. Also, the water-quality model was not designed to simulate species composition of the phytoplankton or unusual but dramatic biological conditions, such as blooms of certain potential nuisance species like *Phaeocystis* and *Ceratium*, which may strongly influence as well as be influenced by water quality. In terms of specific parameters, primary production seems underestimated in the model, but in contrast, dissolved-oxygen dynamics are fairly well simulated. Finally, some rather subtle details in nutrient geochemistry are not always represented in the model simulations. For example, concentrations of one species of inorganic nitrogen (NH_4) is occasionally overpredicted, suggesting that nitrification rates or assimilation rates of some nutrient species may be low in the model compared with natural rates.

On the other hand, there are numerous features that are well-captured in model simulations. The basic nature of the annual cycle and seasonal dynamics of spring and fall bloom periods are represented, even in terms of primary production rates. Relative variability among nutrients seems well-simulated and model results suggest the same quality and timing of nutrient and light limitation as are suggested by monitoring data (*e.g.*, decreases in surface dissolved inorganic silica during April–June, following spring bloom, and low, growth-limiting concentrations of surface dissolved inorganic nitrogen during the stratified season).

To a fair degree, fundamental differences in chlorophyll and dissolved oxygen in surface and bottom layers during unstratified and stratified periods are simulated. For example, the model simulates well the appearance of a subsurface chlorophyll maximum layer in the seasonal pycnocline, 15 to 20 m below surface. Importantly, the stratified season decrease in dissolved oxygen in bottom water is indicated in model simulations. Based on monitoring in 1994 that was extended to near the model boundary along the Gulf of Maine, the model seems to simulate that region well, giving more credence to the assumptions on boundary conditions and fluxes. The model operates at a sufficiently resolved spatial scale to simulate two fundamental features of the Bays that have been illustrated by intensive monitoring efforts. First, the model very well portrays the known concentration gradient radiating from Boston Harbor out into Massachusetts Bay. Second, the model captures many of the fundamental differences in nutrients, chlorophyll and production processes that occur in time and space between the Massachusetts Bay basin and the Cape Cod Bay basin.

Thus, the basic predictive range of the model seems strongly established. It is neither an event-scale nor a highly localized condition model and in general, as the time and space scales of interest shrink, the confidence in the model results diminishes. Yet, it can predict mesoscale features, both in time and space. With grid-scales on the order of 10's of km^2, the model appears to resolve many spatial features on regional scales greater than ~50 to 100 km^2 and the model, at a minimum, can produce annual and seasonal averages at given regions that compare favorably with observations.

Tests were conducted to evaluate the sensitivity to nutrient loading at the seaward boundary of the model domain. Three specific runs were suggested by the MEG: (1) a 25% reduction in the boundary nitrogen concentrations, (2) elimination of atmospheric inputs, and (3) elimination of internal sources (point, nonpoint, and atmospheric inputs), that is, to consider only the boundary input of nutrients. Projections were run using the 1992 calibration simulation, with its boundary conditions, model coefficients, and loadings, as the base case. One-year model simulations suggested that the influx from the Gulf of Maine is substantial and that fluctuations in this input can affect the average concentrations of nutrients in the Bays. In contrast, changes in the effluent load tend more to affect local conditions near the source, but not the overall Bay-wide conditions. These model results suggest that the magnitude of nutrient input from the Gulf of Maine greatly exceeds that of the MWRA loading and may control concentrations at the Baywide scale. Moreover, they are fundamentally consistent with present conditions as characterized by recent data syntheses (*e.g.*, Kelly, 1991; 1993). Although not explicitly simulated, these results suggest that tertiary treatment (*i.e.*, advanced wastewater treatment designed to remove nutrients) would have limited beneficial effect on overall water quality in the Bays.

Since the calibration and sensitivity analyses were reasonable and reproducible, it was appropriate to conduct model-based projections of the changes in water quality which might result from several waste loading scenarios. Like the sensitivity analyses, projections were run using the 1992 calibration simulation as the base case. Projections were made for three remedial alternatives: (1) relocation of the Deer Island and Nut Island treatment plant outfalls into Massachusetts Bay, about 15 km from their current locations, (2) outfall relocation and upgrading of the treatment facilities to secondary treatment, and (3) upgrading MWRA facilities to secondary treatment and continued discharge at the current outfall locations.

Projections were run using 1992 hydrodynamic model results and forcing data cycled for five years until new equilibrium was achieved. An unaggregated grid near the outfall site was used for the projections to correctly simulate plume rise.

All three projection scenarios showed improvements in water quality in Boston Harbor. The model projections for outfall relocation indicated that most effects were highly localized around the future outfall location. The greatest improvements in water quality in the Boston Harbor and Massachusetts Bays system resulted from a combination of outfall relocation and upgrading to secondary treatment.

(C) SEDIMENT SUBMODEL

Sediments play a particular role in water quality because they can serve as an important sink of nutrients (through burial and denitrification) and oxygen. In addition, the recycling of nutrients from sediments supports a substantial fraction of the primary productivity in many coastal systems. Potential degradation of the sediments and the benthic habitat are also important environmental concerns.

The Bays sediment submodel was originally developed for the Chesapeake Bay study (DiToro and Fitzpatrick, 1993). The model is driven by the input of particulate matter from the water column and calculates the fluxes of oxygen, hydrogen sulfide, ammonium, nitrate, phosphate, and silica across the sediment water interface. Since this model is less known to the community, it will be examined here in more detail.

Some monitoring of benthic fluxes in the Boston Harbor and Massachusetts Bay began in 1990, but complete seasonal data for Massachusetts Bay are not available for the water-quality model calibration period. Benthic fluxes from 1993 and 1994 were compared with the water-quality model results from the earlier model calibration period (Giblin *et al.*, 1994; 1995).

All of the fluxes except silica are driven by the diagenesis of the sedimented particulate organic carbon, organic nitrogen, and organic phosphorous to the sediment. Organic matter is assigned to one of three reaction rate classes (approximately 20-day, one-year, and inert). The rate of organic matter reaction within these classes is then modified by temperature. This diagenesis formulation is a simplification of the "multi-G" model of Berner (1980). Laboratory and field studies have supported this general model. Organic matter decomposition rates vary with the age and composition of the organic matter, with a fraction of algal material being rapidly decomposed and the remainder of the decomposition proceeding more slowly, with a small fraction assumed to be essentially inert (Westrich and Berner, 1984; Henrichs and Doyle, 1986; and references therein). The three phase model used here is a reasonable simplification and the reaction rates chosen for this model fall within the range of the literature values available (*op. cit.*).

Because much of the bulk organic matter in sediments in depositional areas consists of organic matter in the last stages of decomposition, or is inert, bulk sediment carbon concentrations cannot be used to test the model. The rate of sediment oxygen uptake compared to model predictions is a better test of the model reliability. The model rate is influenced by a number of assumptions made in water column model (primary production, settling velocity) as well as the sediment decomposition kinetics so it does not test the sediment submodel explicitly. However, benthic oxygen uptake is a process which contributes to dissolved oxygen concentration changes during the stratified period in Massachusetts Bay, and therefore represents an important inclusion in the model, one which provides greater realism and improves its potential as a management tool.

In general, the seasonal pattern of oxygen uptake observed in the Bays matches the seasonal pattern predicted by the model quite well. The model-predicted fluxes were a little lower than the measured fluxes but reasonable. This may be because the model productivity is a bit lower than the observed (see section 4B) or it may be because the oxygen uptake was measured in depositional areas where respiration is a little higher than the "average" rate predicted by the model.

Oxygen uptake at the former sludge disposal site (Station BH03) in Boston Harbor has shown large year to year variability that has been attributed to the periodic colonization of the sediments by benthic amphipods now that dumping has ceased. The HydroQual model did not capture this variation and missed the very high SOD observed in 1993 when the animals were most abundant. The modeled SOD fluxes were much closer to observed data in 1994 when the animal abundance had dropped. This suggests that the model is correctly predicting the long term response of this station but missing transients. Modelled and actual SOD rates at other Harbor stations were closer. Fluxes match fairly well at a number of the muddy stations, sometimes observed being higher and sometimes lower than predicted. The model tended to predict somewhat higher than observed fluxes at monitoring station BH08 and some of the other sandy stations.

Nitrogen diagenesis essentially follows the same formulation as carbon. However, once mineralized to ammonium, the nitrogen flux from the sediment is modelled by determining the oxidation-reduction status of the sediment. The model calculated an aerobic zone thickness from the sediment–oxygen demand and the bottom water overlying oxygen concentration. In the aerobic portion of the sediment a portion of the ammonium is oxidized to nitrate. A portion of the nitrate may enter the overlying water and a portion may enter the anaerobic zone of the sediment where it is reduced to nitrogen gas and lost through a process of denitrification. Hence the model computes a flux of ammonium and nitrate across the sediment–water interface and by the difference between nitrogen mineralization and nitrogen efflux, a denitrification flux. Additionally, nitrate from the overlying water can diffuse into the sediments and be denitrified.

This formulation of the nitrogen cycle is intended to be detailed enough to capture the fairly complicated redox reactions of nitrogen without including all the possible intermediates. The model structure was developed using data from Milwaukee River sediments and further refined using data from Chesapeake Bay. Giblin and co-workers have also run an experiment using sediments from Massachusetts Bay and found that the difference between the ammonium diagenesis flux and the observed nitrogen flux followed the same relationship as predicted by the model (Giblin *et al.*, unpublished data).

In general there is good agreement between the observed ammonium fluxes and those predicted by the model for the Bay sediments. The model also predicts nitrate efflux from the sediments in agreement with the data. There is a lot of variability in the measured denitrification data. The overall average of 1.2 mmol N m^{-2} d^{-1} calculated for the shallow nearfield stations was close to that modeled for segments 10/20 and 9/18 which are in the same region.

The nitrogen flux from the Harbor shows some of the similarities and differences between the data that are noted for oxygen above. The model missed the very high rates of nitrate efflux that were measured at station BH03 in 1993 while the data were much closer in 1994. Ammonium fluxes appeared to be in much better agreement with the model for most years. The direct denitrification measurements were higher than modeled but the difference was less in 1994 than in 1993.

Silica fluxes are driven by the chemical dissolution of solid phase biogenic silica in the sediments and are largely controlled by porewater Si concentrations. There were fairly large differences between the model predicted fluxes of silica and the observed silica fluxes in most stations. The largest differences were in the Bay; measured fluxes are 2 to 10 times higher. One hypothesis is that the difference between the model and the data is due to the sites which were sampled and the time scale over which Si dissolution takes place. All of the Bay stations are depositional and may be accumulating and concentrating material which is more evenly distributed in the model. Because much of the organic matter that reaches the bottom is decomposed within the year the movement of fine particles by winter storms does not affect the O/N/P fluxes to a great extent. However, Si dissolves more slowly than organic matter decomposes so Si fluxes may be more sensitive to sediment redistribution. There is a discrepancy between the high measured Si fluxes and a build up of Si in the bottom water. This suggests that the lower Si fluxes calculated by the model may be a more realistic average for the Bay. Silica fluxes in depositional areas of the Harbor were higher than predicted by the model while fluxes from non-depositional areas were lower, supporting the idea that redistribution of fine particles is causing the apparent discrepancy. Measurements of biogenic Si in the sediments may help resolve this question.

In summary the sediment oxygen uptake data suggests that the model is capturing the major features of carbon decomposition in the sediments. The modeled nitrogen fluxes are also in fairly good agreement with the measured data with some differences in the spatial and temporal patterns especially in the Harbor. Data taken at the former sludge disposal site in the Harbor, station BH03, would indicate that the model is capable of predicting the change in benthic fluxes which occur with changes in loading over a several year time span but does not capture transients which occur during the first few years. There are fairly large differences between the predicted and modeled silica fluxes. It is not clear whether this reflects the bias of the sediment sampling toward depositional areas or reflects some problem with the model parameterization.

5. CONCLUSIONS AND RECOMMENDATIONS

(A) The circulation model represents the state-of-the-art in reproducing the time-dependent three-dimensional stratification and circulation in Massachusetts and Cape Cod Bays. As with all model applications of this type, the major uncertainties relate to the inability to exactly specify the forcing and boundary conditions for the model simulations.

(B) The water-quality model represents the state-of-the-art in applying time-dependent, multi-dimensional water-quality models to primary productivity in estuarine and coastal systems. Because the physical, chemical, and biological processes represented in the water-quality model are more empirical than those in the hydrodynamical model, and because there are limitations in the data sets that are available, we have to expect that the model results will not completely reproduce the data. However, the model results do reproduce broad-scale space and time patterns in water quality in the Bays.

(C) The circulation model appears to reproduce the outfall plume structure when comparisons are made with state-of-the-art plume models. The plume structure and properties produced in the circulation model are specified in the water-quality model at the same horizontal spatial scale as the circulation model.

(D) Although there are some uncertainties about the nutrient concentrations at the boundary, it is clear that the Gulf of Maine is the major contributor of nitrogen into the Bays system. While the MWRA contributes substantially to the anthropogenic N inputs, it makes a relatively small contribution to the N loading of the Bays as a whole. As a result, nutrient concentrations predicted by the model are not very sensitive to the scenarios concerning treatment level or outfall locations beyond a radius of order 5 to 10 km from the outfall.

(E) The validity of the model calculations can be judged to some extent from the degree that the model has been able to reproduce the general features observed in the monitoring program. Furthermore, a model which can reproduce observations in different estuaries with essentially similar model structure and coefficients has a higher level of credibility since this indicates that the dominant physical, biological, and chemical processes are being simulated by the model. The Bays water-quality model has been successfully applied to Chesapeake Bay, Long Island Sound, and currently New York Harbor with only minor changes in either model structure or model coefficients. The degree of calibration for these cases is similar to that achieved in the Bays so that the model has demonstrated a general level of validity. In addition, a principal factor influencing water quality results, the circulation between Boston Harbor and the outer Bays region, is well represented. Evidence of this is the model's ability to reproduce average concentrations in Boston Harbor as well as the well-documented concentration gradient extending from the Harbor into the Bay.

(F) The models can be relied on to a much greater extent when predicting relative changes in water quality due to various loading scenarios as opposed to predicting absolute changes of water-quality parameters. The model predicts that, throughout most of the Massachusetts/Cape Cod Bays region, there will be insignificant changes in the concentration of nutrients, the rate of primary productivity, dissolved-oxygen levels, and the vertical flux of particulate organic matter as a consequence of either the upgrade in sewage treatment to secondary or the change in outfall location from its current location in Boston Harbor to its future location in western Massachusetts Bay. Simulations do show that water quality will improve in Boston Harbor as a result of these facilities changes, with the bigger effect being due to the change in outfall location. While a full sensitivity analysis of the model to critical assumptions has not been conducted, we nonetheless have confidence in the predictions of change because of the small differences in loading involved.

(G) As a corollary of (D) and (F), we would expect relatively small changes in the bays-wide concentration of nutrients, rate of primary productivity, dissolved-oxygen level, and vertical flux of particulate organic matter if tertiary treatment (*i.e.*, advanced wastewater treatment designed to remove nutrients) were ever to be seriously considered.

(H) Both the circulation and water-quality model results should be compared to monitoring data on an ongoing basis. This is particularly important for validation of the water-quality model since it was calibrated but not validated. Increased confidence in the range of the model to make predictions could be attained by selected model–data comparisons. Baseline monitoring in the Bay during 1992–1994 has shown that several important water quality parameters, such as chlorophyll and dissolved oxygen, vary substantially from year-to-year (Kelly and Turner, 1995); extremes of variability could be used to test the limits of predictability of the model. For example, in 1993, there was an intense fall bloom (September–October) which produced chlorophyll concentrations far above those otherwise observed in the 36-month time series used for calibration. In 1994, the annual DO minima in bottom waters fell below the state standard (6 mg/L), a result that distinguished this year from all other recent years of study. Hypotheses on the mechanisms producing these two events focus on physical forcing functions, and a strong test of the coupled hydrodynamic-water-quality model would be its ability to simulate the unusual water-quality patterns in 1993 and 1994.

(I) The MEG accepts the current Massachusetts and Cape Cod Bays models as representing the current state-of-the-art in the application of time-dependent, three-dimensional hydrodynamic and water-quality models for estuarine and coastal areas. The MEG recommends that copies of the models (including code, documentation, and appropriate data sets) be transferred from the MWRA contractors and cooperators to a suitable host site or sites within the Massachusetts Bay research community to serve as an additional "home" for research/management/policy topics. In addition to model maintenance and upgrading, the host site or sites should have the responsibility of continuing the calibration, validation, and forecasting activities by making use of ongoing monitoring data collected by MWRA and other organizations. MEG strongly recommends that MWRA and other monitoring organizations earmark 20% of future monitoring expenditures for maintaining, updating, distributing, supporting, and applying the models.

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APPENDIX A
MWRA MODEL EVALUATION GROUP

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APPENDIX B

MWRA MODEL EVALUATION GROUP

MEETING MINUTES AND ATTENDANCE

Agenda

Massachusetts Bay Model Evaluation Group
Meeting #1

September 14, 1992

10:30 a.m. - 5:00 p.m.

Conference Room
NEFCO Plant
Fore River Staging Area, Quincy

Rationale and background of Mass. Bay models (10 min)	Mike Connor, Wendy Leo
Hydrodynamic model - presentation/progress to date (45 min) Discussion - boundary conditions and turbulence closure - what to use it for? what model runs to do? - anything we've overlooked? - does it work? model validation	Rich Signell, Alan Blumberg, Harry Jenter
Water quality model - presentation/progress to date (45 min) Discussion - committee reaction to approach - does model framework match real biological processes?	Jim Fitzpatrick, Dom DiToro

MASSACHUSETTS WATER RESOURCES AUTHORITY MODEL EVALUATION GROUP

Meeting Summary
September 14, 1992

ATTENDEES:

Michael Connor MWRA
Wendy Leo, MWRA
Merryl Alber, MWRA
* John Paul, EPA
* Donald Harleman, MIT
Eric Adams, MIT
* Robert Beardsley, WHOI (Chairman)
* Charles Yentsch, Bigelow (represented by Thor Aarup)
Richard Signell, USGS
Harry Jenter, USGS
Anne Giblin, MBL
Dominic DiToro, HydroQual
Alan Blumberg, HydroQual
James Fitzpatrick, HydroQual
Rich Isleib, HydroQual
John Shipman, Normandeau Assoc.

* MEG Members

1. Introduction - Mike Connor

The modeling efforts were undertaken by MWRA to

- Assess impacts of change in MWRA outfall location and nutrient load on Mass Bay/Cape Cod Bay.
- Provide seed for future studies in the area, e.g., hydrodynamics (by USGS) and algal blooms (EPA), etc. and help GOM and MASSBAY's efforts.

MWRA is funding this modeling effort with USGS and HydroQual/Normandeau to complete 3-D hydrodynamic and water quality models, based on previous models for Chesapeake Bay and Long Island Sound developed by HydroQual. This work is a continuation of earlier 2-D model work developed by MIT; this was used by CDM and EPA to quantify farfield dilutions to make dissolved oxygen projections and evaluate eutrophication impacts in the process of the outfall site selection. The hydrodynamic modeling is halfway through on a 5-yr. agreement with USGS with several months of support left from HydroQual. The water quality modeling is expected to be completed by the end of 1993, under the HydroQual/Normandeau contract.

The MWRA historical data are being stored in an Oracle database, with only some data sets entered so far. The database is being shared with the Mass Bays program. In addition, Mass Bays and USGS are coordinating the physical data.

The MEG was organized to provide an independent review of the modeling process and to ensure that the tool is used appropriately. In general, studies in Mass Bay are being directed by the Mass Bays TAC, the MWRA outfall MEG (Model Evaluation Group) and technical workshops which provide guidance to MWRA and others on the regional efforts.

2. Hydrodynamic Model - Rich Signell, Harry Jenter, Alan Blumberg

- A review of the physical structure of the Bay and exchange with waters beyond the bay (i.e., Gulf of Maine, etc.) was presented. Wind is an important forcing function in this region (particularly inshore), operating on about a one-day time scale. The Merrimack River and other rivers in the Gulf of Maine (i.e., Kennebec, Penobscot) appear to be quite an important hydrographic influence coming from above Cape Ann; these will be looked at further. This influence is important to offshore (Stellwagen Bank) boundary conditions. The channel between Stellwagen Bank and Provincetown acts as an "exit valve" for waters in the Bays; surface water residence time in the bay is on the order of 3 to 6 weeks. Low frequency variability is important at the offshore outfall because there is no preferred mean flow there.

- The Ecom-Si 3-D model was described: the 70 x 70 grid varies from 0.6 km square in Boston Harbor to 6 km on a side at outer edge and 1.0 km at outfall; it has 10 levels in the vertical. Only unstratified conditions have been run so far; the discussion was based on these runs. The USGS model is similar to the Blumberg-Mellor model except that the model code is implicit in its treatment of free surface elevation.

- The M2 barotropic tide ellipses were presented and discussed. The model matches measured values quite well and showed a 2 km excursion at the offshore outfall; currents off Race Point are not well resolved yet, however. Tidal ellipse variability was discussed and Rich was interested in knowing what level of variability is acceptable.

- How NW winds drive shallow water southward alongshore, causing a deep return flow was demonstrated; also how SW wind brings cold water to shore and warmer water offshore was shown (i.e., coastal upwelling). This simulation included an imposed outerflow in the Gulf of Maine.

- Establishing outer boundary conditions for temperature and salinity remains a challenge; this needs to be resolved for runs for stratified conditions. While the open boundary of Mass Bay is the biggest issue, it is hoped that internal conditions dominate, at least in western Mass Bay. Heat flux calculations were discussed, including their role in helping to set up the baroclinic case. Climatological data are needed as well as a characterization of input at the outer (northern) boundary in order to characterize the salinity field.

- USGS's video presentation (of a 51-day projection) showed that the modeled behavior of the discharge plumes were generally similar "downstream" (i.e., primarily along the South Shore) far from the discharge, for the existing outfall versus the offshore discharge. There were differences locally. That is to say, the 1000 dilution line for both scenarios (Deer Island versus the offshore outfall) had a generally simlar distribution from Hull to Plymouth Harbor, but simluations showed greater differences in the Boston Harbor and Northshore areas.

- Since the equilibrium run for the water quality model will be on the order of one to two years, it was suggested that the hydrodynamic model be run over the same period (i.e., one year). Seasonal runs for the WQ model are not that helpful unless we're looking to answer a specific seasonal question (e.g., dinoflagellate blooms, etc.).

- USGS is examining 20 year salinity records as well as discharge records from the major rivers in Gulf of Maine in order to help better define the salinity field along the northeast boundary.

- The resolution of horizontal grid size between WQ and hydrodynamic models was discussed. Essentially, hydrodynamic model segmentation is too fine for the WQ model. Options were discussed, with a resulting suggestion to collapse the hydrodynamic 4:1 and to test further collapses until the model "blows up". It was also suggested that time averaging occur over 1-2 hour periods instead of 10 minutes in order to reduce computations (i.e., model run time).

3. Water Quality Model - Dom DiToro, Jim Fitzpatrick

- An overview of water quality model key components and interactions was presented by Dom DiToro. The presence and rates of incorporation of depositional sediments was discussed in relation to shear stress and benthic invertebrate influences. Chlorophyll level in sediment may be a way to quantify this movement and tell a lot about G_1 (labile) Carbon. The % G_3 (conservative or slow refractory) Carbon for Boston Harbor sediments was calculated at about 20% by Kelly (MWRA Tech. Report 92-2). The percent of sandy bottom in the area may make the sediment flux calculations for Mass Bay much different from Chesapeake Bay; sediment deposition is expected in the nearfield outfall area but deposition rates in the farfield are uncertain.

- The water quality model-set up and preliminary information was presented by Jim Fitzpatrick. The model Phase I grid includes 32 horizontal and 5 vertical segments. Initial estimates of bay circulation were provided using spatially aggregated output from the hydrodynamic model under a non-stratified, average wind condition. The greatest physical gradient is in the vertical; the horizontal shear flow is a dispersion calculation. To mimic stratified conditions, the surface temperature was forced to match data and the vertical mixing coefficients were seasonally modified. Water quality model input includes yearly mean nutrient loading from the Menzie-Cura report and monthly exogenous data (rainfall, solar irradiance, etc.); the model is essentially monthly driven. Initial calibration analyses were made by comparing model computations against MWRA 1989-1990, (Bigelow/UMASS) cruise data for water quality and Anne Giblins sediment data. HydroQual made initial runs to test initial input including robustness of the sediment model. In initial runs, model results generally matched observed data but adjustments are clearly necessary. For example, the model currently overestimates concentrations in the outfall region because of tidal averaging (i.e., underestimates horizontal transport). Furthermore, the model currently overestimates sediment oxygen demand (SOD) in Boston Harbor and nitrate and phosphorus fluxes are off.

4. Discussion

- Since the current hydrodynamic grid is unworkable for the WQ model, tests in the near-term will include 2:1, 3:1 and/or 4:1 averaging for salinity in the hydrodynamic model to see what happens. Also, we will need to do temperature compensation for stratification. We will re-run the hydro model with one hour averaging and then collapse the grid 2:1 and beyond to see what happens. Vertical collapse will probably not be used because of concerns about fairly representing the euphotic zone and the pycnocline.

- Discussion revolved around issues related to development of the baroclinic model and how to set boundary conditions. Suggested that a "pseudo"-summer hydrodynamic run be developed just so that a summer condition can be tested in the WQ model.

- Issues discussed included:

    - How are the two models dealing with nearfield conditions and how is the nearfield defined? The discharge mixes within one grid cell since the grid size is too large to resolve the immediate discharge plume characteristics. To get around this problem, the hydrodynamic model can be run to determine what depth the plume will rise to and in turn, the water quality model can then be made to reflect an input at that point.

- Harmful algal blooms (especially red tides) seem to bloom every three years or so; Don Anderson is tracking this issue. There is a long-term Pilayella (free-floating benthic brown algae) bloom problem in Nahant Bay which may need attention.

- First order model questions summarized by D. DiToro:

  1. In the short-term, what is the difference between the offshore and present outfall location? The long-term involves operational questions, like total loading effects.

  2. If there is a harmful algal bloom (or D.O. or PSP problem, etc.) can it be demonstrated that MWRA "triggered" the problem or that they can do something about it? This assumes these processes can be effectively modeled.

  3. What is Mass Bay's assimilation capacity for nitrogen?

- A review of satellite data during the Bigelow cruises was suggested to look at larger scale temperature conditions and help verify model results.

- Evolution/de-evolution of the thermocline needs to be tested using the salinity/temperature data.

- M. Connor discussed the upwelling issue and its importance; it would be helpful to show the number and extent of upwelling events. We could look at historical satellite data for temperature and chlorophyll to evaluate this issue.

5. Summary of Modeling Evaluation Group (MEG) Comments

Hydrodynamic Modeling Findings

The MEG was pleased with the progress to date. They recommended the following priorities:

1. The highest priority is getting the baroclinic case working. The heat flux calculations need to be done differently so that the natural air/sea interaction feedback is retained and then a comparison made from model predictions to satellite data. At least a two month summer case is needed quickly. Two demonstration examples should be run to determine if the numerical model is giving realistic simulations. The first focuses on the evaluation of the spring thermocline (pycno-

cline) by increased surface heat flux and reduced wind-mixing; the second focuses on the breakup of the seasonal thermocline (pycnocline) by reduced heating (even cooling) and increased wind-mixing. At that point, a 6-month continuous period should be run, from spring through summer stratification, through fall overturn.

2. The boundary condition issue needs to be solved for salinity and elevation along the open boundary.

Water Quality Model

1. The MEG was pleased with the initial progress on the water quality model. The model for the Chesapeake provides a convincing simulation of events. The effect of river plumes on Mass Bay are so variable - will that be a problem?

2. The most immediate priority is to evaluate the sensitivity to grid size. The MEG suggests re-doing the model calculation based on the hydrodynamic grid, evaluating grid collapses until the model falls apart for the instantaneous salinity anomaly. There needs to be a coarse and fine grid comparision for the same hydrodynamic forcing and boundary conditions. The baroclinic test case is of most importance.

3. The MEG would like to see some verification of the choice of sediment parameters, particularly the effect of the temperature coefficient on SOD, burial rate, and denitrification rate. It is recommended that some colder water data sets (e.g., North Sea or Baltic) be examined to see if they agree with parameter choices from the Chesapeake region. Since the Gulf of Maine lies outside the range of observed parameters from Chesapeake Bay and LIS, there is concern that we may be extrapolating beyond the range of observations.

4. The problem of specifying water quality state variable fluxes across the open ocean boundary should be addressed. Do the data exist to do a reasonable job?

Other Issues

The MEG expressed the following questions they would like the consultants to address:

1. What is the vertical resolution in the sigma coordinate system.

2. How sensitive is Mass Bay to fluxes along the boundary? Also, how significant are the spurious circulations?

3. Is the grid spacing sufficient to resolve internal waves coming over Stellwagen Bank?

4. What are the major data gaps that need increased measurement programs? Are the consultants aware of the UNH-Maine Sea Grant - hydrographic data off Portsmouth, NH?

5. Can tidal differences off Race Point be resolved?

6. Is there the ability to successfully simulate river plume dynamics?

7. What kind of numerical dispersion is implicit in the formulation used in the model?

8. What is the effect of reflection off the outer boundary?

MEETING NOTICE

MASSACHUSETTS WATER RESOURCES AUTHORITY
MODEL EVALUATION GROUP MEETING

JANUARY 25, 1993

10 am - 3 pm

Charlestown Navy Yard, Bldg 36, (100 First Avenue)3rd Floor Board Room

AGENDA

1. Opening remarks - Bob Beardsley/Mike Connor

2. Hydrodynamic model update - Rich Signell/Alan Blumberg/Harry Jenter

   - Analysis of baroclinic simulations - transition to spring
   - Discussion of long-term simulations

     - Analysis of 10.5 month case
     - Status of 18-month run (updated inputs)

   - Selection of boundary condition
   - Inclusion of Gulf of Maine rivers
   - Influence of model resolution

     - Boston Harbor
     - Race Point
     - New outfall

3. Water Quality Model - Dom DiToro/Jim Fitzpatrick

   - Status of Grid collapse work
   - Further evaluation of sediment flux model
   - Resolution of boundary conditions
   - Toxic Dinoflagellate Blooms
   - Future work/direction

     - appropriate data set for model validation
     - other

4. Remaining MEG Committee questions not resolved by above discussion

NOTE: The MEG Committee will meet in executive session immediately following the above presentations/discussions.

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MWRA MEETING SUMMARY

MASSACHUSETTS BAY MODEL EVALUATION GROUP

JANUARY 25, 1993

HYDRODYNAMIC MODEL - Rich Signell

Work is continuing on the 18 month (January 1990 - July 1991) hydrographic simulation that HydroQual will use in their water quality model. A one-year simulation has been completed for the period of January 1990 to January 1991. This effort was generally successful, providing an accurate prediction of the timing of thermo/pycnocline evaluation and breakdown. However, an area to improve on is the inability of the model to predict the magnitude of temperature and salinity stratification. Rich hypothesized that this weakness results from the northern boundary of the model not accounting for enough warmer, fresher surface flow into the study are (e.g., water originated from the Gulf of Maine).

The following suggestions were made in a discussion of possible strategies to improve the model:

1) Set vertical mixing equal to zero to diagnose the stratification problem

2) Evaluate sigma surfaces
   a) use a finer grid at Bay entrances
   b) subtract out area-averaged density as a diagnostic tool

3) Run Gulf of Maine model to generate northern boundary conditions

4) Evaluate the importance of deep water exchange between Gulf of Maine and Stellwagen Basin

5) Evaluate AVHRR satellite composite (4/90-9/90)

WATER QUALITY MODEL - Alan Blumberg

In order to reduce the run-time of the water quality model, HydroQual evaluated the effects of collapsing (aggregating) the grid network and increasing the time step. When both models were run on the same grid and with the same time step, the results produced were in full agreement. When the time step is increased to one-hour averages, the results of the hydrodynamic model cannot be reproduced for Race Point or Boston Harbor. The results improve if you step back to half-hour averages. Using a one-hour time step and an aggregated grid network (4:1 or 2x2), the water quality model predicts a greater degree of temperature and salinity stratification than the hydrodynamic model.

Dissolved oxygen and nitrogen simulations were run in order to look at variables that have locally-induced vertical variation (which salinity does not have). The oxygen source is the atmosphere and the sink is sediment oxygen demand (SOD); the two models (full grid vs. aggregated grid) tracked well for surface layers; bottom dissolved oxygen was lower in the collapsed grid model than in the full grid model, because the former produces a stronger pattern of vertical stratification. The nitrogen source is the MWRA outfall and the sink is the sediments; again, surface layers track well, but bottom layers do not.

HydroQual intends on continuing these types of tests as new, longer hydrodynamic simulations become available. A possible compromise approach is to collapse 2x1 in harbor and harbor entrance, 2x2 elsewhere, and use ½ hour time step.

SEDIMENT MODEL - Dom DiToro

Cold water data from the MERL/Narragansett Bay work was used to do a "stand alone" calibration of the Chesapeake Bay model. The model predicted ammonia flux at low loadings (0-4x), but could not reproduce the negative ammonia flux observed at high loadings (8-32x). The model predicted the MERL oxygen flux data well, but was not able to accurately predict phosphorus or silica flux. A few changes were made to the sediment model that improved these areas:

1) Temperature dependencies were increased for diffusion and particle mixing

2) Diffusion coefficient for pore water mixing was increased (5x)

3) Phosphorus partition coefficient was changed (lowered)

4) Detrital silica (Si) source (i.e., the Susquehanna River) was eliminated

5) Changed the carbon/silica ratio slightly

Boundary conditions were revisited and the following points/suggestions were made:

- More monitoring stations in the area of northern boundary would be useful

- A sensitivity analysis on boundary conditions needs to be done

- It would be better to use the same boundary conditions in the WQ model as in the hydrodynamic model, since the cross-boundary flow is smaller

- Boundary conditions would be better if produced from the Gulf of Maine submodel developed by Rich Signell.

The next MEG meeting will be in late May. During the interim, Rich Signell will produce the 18-month hydrographic simulation (by March 1st) for HydroQual. HydroQual will then collapse the grid network and run the water quality model, then develop the eutrophication model. This work should be ready for evaluation in April. Some interim results will be distributed to the MEG Committee prior to the May meeting.

MWRA MEG COMMITTEE RECOMMENDATIONS

MASSACHUSETTS BAY MODEL EVALUATION GROUP

JANUARY 25, 1993

HYDRODYNAMIC MODEL

Two work priorities were established by the Committee:

1. Turn off the vertical mixing to see if sufficient stratification then occurs into the spring and summer. At the same time correct for heat flux at the surface. Results should be submitted to the Committee before the next meeting.

2. Add riverine input at outer boundary; run Gulf of Maine model to generate northern boundary conditions;

Other priorities included:

1) Check for horizontal heterogeneity by examining composite satellite images (from NOAA or other sources) and compare with modeled conditions in January and April 1990;

2) Compare the results of an outfall nearfield model and compare the results with the 100:1 dilution grid "box" at the outfall;

3) Examine local wind data at Gulf of Maine input area; and

4) Check the model sensitivity to the Gulf of Maine (boundary conditions) input.

Further questions discussed included the possibility of putting tracers in the model to see where suspended material settles, particularly in reference to Stellwagen Basin. The need for upstream boundary monitoring station was discussed by HydroQual should check the model sensitivity to this input before determining the level of need.

WATER QUALITY MODEL

The Committee felt that the grid collapse looked good but cautioned not to lose the connection between Boston Harbor and the outfall area and don't add significantly to model inaccuracies.

The Committee recommended running the sediment model without the outfall to check depositional areas and see how important transport is, particularly in the area of Stellwagen Basin. Furthermore, validating the model with boreal data was felt to be important. The importance of the bottom layer of the WQ model is a critical component of the model was emphasized.

With respect to particular model parameters the following questions/comments were made by the Committee:

1) Is there enough observational data in Stellwagen area to compare the model?

2) Spatial differences in sedimentation rate is important so account for this by examining substrate type within each grid;

3) It is necessary to keep the phosphorus component of the model for now;

4) It was agreed that biogenic silica data was needed for the model and

5) The importance of nutrient input at the northern boundary should be determined and the appropriate levels added at that point.

The next meeting will be scheduled for late May.

NORMANDEAU ASSOCIATES

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Massachusetts Bay Model Evaluation Group

January 25, 1993

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MEETING AGENDA
MWRA MODEL EVALUATION GROUP

TUESDAY, JULY 20, 1993
10:00 AM - 3:00 PM

MWRA BLDG 36, 3RD FLOOR BOARD ROOM
****

1. INTRODUCTION - M. CONNOR

2. HYDRODYNAMIC MODEL - R. SIGNELL, H. JENTER, A. BLUMBERG

   - Model Improvements

     - control of excessive mixing along sigma surfaces
     - better parameterization of Gulf of Maine boundary conditions
     - better heat flux parameterization
     - Shapiro filtering to suppress 2 grid length energy along open boundary

   - Model Results

     - comparison of year-long run (including stratified conditions) with data

3. WATER QUALITY MODEL - J. FITZPATRICK, D. DITORO

   - Progress since last meeting
   - Brief review of WQ Model Kinetic Framework
   - Review of Hydrodynamic/WQ Grids and Collapsing Algorithm
   - Review of WQ Database - As applied to boundary conditions
   - Calibration results/projection for outfall relocation
   - Future model validation with 1992 data sets
   - Other future modeling directions

     - scenario runs/sensivity analysis
     - dinoflagellates

4. MEG COMMITTEE EXECUTIVE SESSION - R. BEARDSLEY

MWRA EVALUATION GROUP MEETING
JULY 20, 1993
MEETING SUMMARY

1. INTRODUCTION

Mike Connor discussed the fact that the lawsuits related to the offshore outfall have been turned aside; additionally, a recent EPA report found that the outfall would pose no threat to Threatened or Endangered Species in Massachusetts Bay.

Data from the monitoring study show that the zone of influence from Boston Harbor has been reasonably predicted by the model, and that the impact areas are similar to the initial estimate. Also, the issue of nitrogen export from Boston Harbor now appears to be a closed issue, as a result of Battelle's (Jack Kelly's) report. Since sludge removal began in early 1992 there has been evidence that benthos in the Harbor are improving; Ann Giblin's video showed some of this evidence.

2. HYDRODYNAMIC MODEL

Recent adjustments to the hydrodynamic model were discussed by Rich Signell, which included: resolving outer boundary problems (by specifying temperature and salinity in that area); including Merrimack River and Charles River flows and an estimate of freshwater input from Cape Cod groundwater; and tidal elevations from Dan Lynch's fine resolution tidal Gulf of Maine (GOM) model. Resulting statistics showed that the model is adequate to show continuous discharge of material, even if time series plots are not great.

Some problems with temperature stratification were discussed. Essentially, there were some sigma coordinate artifacts which, upon adjusting, yielded improved (within 2°C) temperature modeling; adjustments for year-long trends are being made. There were also some problems at the lower outside edge of the model grid (east of Cape Cod). These were fixed with a Shapiro filter (energies under 2 grid lengths were removed). There are still some problems in the middle of the modeled region and some questions about heat gains in the bottom water, indicating possible surface heat flux calculation problems. GOM input will help surface conditions at the outer boundary. Improved temperature prediction is a first priority.

A discussion ensued about ability to predict the model's specific events. The climate versus weather prediction analogy was used; given the complexity of the system it was not clear that event-level variability can be adequately forecast, given the available data base for model forcing.

A second priority is to make appropriate salinity corrections. This includes tying in a sub-model for the major GOM rivers; this will affect the northern 1/3 to 1/2 of the outer boundary. The southern half of the model presently uses climatological data to correct for salinity, a situation which will probably not change.

Again, event-by-event modeling is difficult because of the complex spatial structure of Mass Bay - surface salinity structure can change every few days and over relatively few kilometers. The goal is to predict seasonal trends and events as accurately as possible, without grid-by-grid adjustments. Methods to check model precision and goodness-of-fit were suggested. Sensitivity simulations using long-term (annual) forecasts and comparisons of one-month periods (e.g. May and August) against monitoring data were also suggested.

Current model vs data comparisons were discussed on a seasonal basis. There is generally a good fit between the model and surface data observed during the winter at Buoy B; at depth (-23 m) comparisons were not as good. There is high variance between the model and data collected during the summer, and general events are not well represented in some cases. Basically, the statistics of events are right but events are not well predicted.

For the current ellipses: in spring, the outfall area is about right although more remote areas (Stellwagen, Cape Cod) are off somewhat; the same is true in summer. An improvement may result from running with Otis AFB or Provincetown winds. In the fall, the variability at the outfall and near shore is ok, but not as good at Stellwagen; in winter, the outfall and Cape Cod are ok, but direction is off at Provincetown and Stellwagen.

The seasonal behavior of the modeled offshore plume (at 500 dilutions) versus the existing outfall was discussed. In March the plume shows an irregular sphere which reaches the surface and then moves (south) along the coast toward Plymouth Harbor, but further offshore than the existing outfall plume. In May the 500-dilution contour forms a "blob" and stays submerged, intersecting slightly with the thermocline; the existing discharge plume is modeled to go south at the surface to Scituate. In July the surface water quality conditions at the new outfall were predicted to actually improve over existing conditions because the contour stays below the surface (while the current discharge goes to the surface). In September the 500 dilution contour is confined to bottom and mid-depth and doesn't go to the surface, as in the other stratified conditions noted above.

3. WATER QUALITY MODEL

Jim Fitzpatrick began his presentation with a review of the water quality model's kinetic structure, loading information and a summary of the available data to be used in calibrating the water quality model. The review of the model's kinetic structure included a listing of the water quality state-variables and a number of viewgraphs showing the relationships between phytoplankton biomass nutrients and dissolved oxygen. The summary of nutrient loadings being delivered to the system was based on the 1991 Menzie-Cura report to the EPA and on information provided by MWRA and included estimates of inputs from point, non-point and atmospheric sources.

The review of the available water quality data base noted a number of deficiencies including:

- the relatively sparse nature of the water column data from both a temporal and spatial point of view,
- the relative paucity in the number of water quality parameters sampled relative to the number of state-variables in the water quality model,
- the lack of a comprehensive data set with which to specify the boundary conditions for the model,
- the available sediment nutrient flux data was primarily restricted to Boston Harbor and the area in the vicinity of the proposed outfall; no data was currently available for other regions of mass Bay and Cape Cod Bay.

It was further noted that, while none of these deficiencies would preclude a successful calibration effort, the data collected as part of the current MWRA Mass Bays monitoring effort would alleviate a number of these data limitations.

An overview of the grid-collapse scheme used to aggregate the hydrodynamic results for use in the water quality model was presented next. It was noted that a three-to-one aggregation was used throughout most of the grid, except in areas where it was necessary to maintain land segment versus water segment integrity. This was required in areas such as the tip of Cape Cod, wherein a two-to-one aggregation was used. The hydrodynamic input file provided by the USGS only included a twelve month span of the eighteen month calibration period, i.e. January 1990 through December

1990 versus October 1989 through March 1991. HydroQual took the hydrodynamic model output for October-December 1990 and used it for October-December 1989 and took January-march 1990 and used it for January-March 1991. They have also been manning the hydrodynamic model at HydroQual to alleviate the problems associated with massive data file transfers.

A review of the calibration status of the water quality model was presented, showing a number of viewgraphs comparing time-series of model computations versus observed data for both the water column and sediment for the 18 month calibration period. HydroQual noted that while the initial calibration results are encouraging (i.e. the model approximately reproduces the annual cycle observed in algal chlorophyll-*a*, inorganic nutrients and dissolved oxygen and approximately reproduces observed nutrient gradients between Boston Harbor and northern Mass Bay) additional calibration work is required. It appears that the water quality model is presently under-estimating daily and annual primary productivity as judged by comparisons to observed surface layer dissolved oxygen concentrations and estimates of productivity made by Jack Kelly (Battelle) using recent monitoring data.

During the course of the water quality model calibration it was noted that the model did not use the same sigma-level correction for diffusive exchange in the horizontal plane that was employed in the hydrodynamic computation. HydroQual speculated that it was not required for water quality but agreed to address the issue further by developing the appropriate code in the water quality model and performing long-term model runs.

4. STATEMENT OF WHAT'S NEXT

HYDRODYNAMIC MODEL

- provide an October-April model run with new heat flux calculations.
- look at one month period and diagnose affects; look at stick plots to diagnose differences.
- display temperature and salinity plots, looking at observed versus modeled variance; generally look at more quantitative data comparison.
- display low frequency data.
- check sea level difference effects - Boston Harbor to Provincetown.
- try to plot new data and compare with model data.
- could contour model differences in two outfall locations; could also run no outfall scenario.
- since vertical mixing is the key to the results of the model, could pick an area around the outfall, hold the surface "flat," and run the model to examine the degree of vertical mixing.
- could display some vertical plots.
- should include some plots at an intermediate depth (i.e. within the thermocline) to pick up peak primary production areas.

WATER QUALITY MODEL

- use hydrodynamic data back to October 1989 and forward to March 1991 (add 3 months, either end).
- incorporate updated loading from MWRA.
- look at freshwater input from Cape Cod.
- make a calibration run in next 1-2 months.
- examine possibility of using new WQ data (since earlier data is more sparse), but would require extending hydrodynamic model out more (next run should include heat flux changes).
- look at affects of grid collapse on temperature and salinity for a full 18 month period.
- examine effects for sigma correction on upslope mixing.

5. EXECUTIVE SESSION RECOMMENDATIONS

HYDRODYNAMIC MODEL

The modelers need to think of better ways to display model results, both for characterizing/understanding what the model is doing, and to compare (quantitatively) with data. For example, they should contour kinetic energy (or variance), and plot profiles and vertical sections as well as surface values. 3-D plots may also be useful.

Implement heat flux based on local SST. See if this fixes "Cape Cod Bay too warm" problem. Also look at spatially varying wind stress, and at the Provincetown-to-Gloucester elevation difference, to see if those help improve model-data comparability.

With local heat flux in place, produce new 18 month run, starting in 10/89, for water quality modelers.

Perform two sets of sensitivity studies, for a month or so each:

(1) peak of stratification
(2) salinity minimum,

to see if the model can reproduce freshwater runoff events and spatial gradients.

Exchange between Boston Harbor and Mass. Bay is important to model correctly. Compare the model's harbor residence time with TRIM, ELA, and residence times estimated from data. Maybe compare a modeled tracer distribution with 1992 monitoring data showing a nutrient gradient from Harbor to Bay.

Discrepancy between modeled and measured currents at Race Point, possibly due to tidal rectification: fix it, or demonstrate why it doesn't matter.

How well does the present approach to determining the outer boundary condition work? Do data from the Gulf of Maine toxic dinoflagellate study validate the western GOM model?

Put in drifters (maybe clusters of them) and see how they disperse, and how they compare to real WHOI drifters.

An unresolved item from the last set of MEG recommendations is to compare initial mixing to initial mixing models; for example, compare initial mixing to trapping height predicted from initial dilution models (trapping height under various conditions is given in Volume V, Appendix A to the Secondary Treatment Facilities Plan).

WQ MODEL

It is not clear why the numerical tactic of subtracting the domain-averaged profile is not necessary for the water quality model, since it seems critical to getting the stratification right in the hydrodynamic model. Add this step to the water quality model and compare the results. It if makes no difference, what does that mean?

Compare the hydrodynamic model and water quality model results for both temperature and salinity, in a region near Stellwagen Bank (instead of nearshore well-mixed areas) under the following scenarios:

- with the water quality model grid collapsed
- with the sigma-grid upslope mixing "fix" left out of the water quality model

We need better ways of displaying the model outputs. In addition to time series plots at a few points, the MEG would like to see profiles sections, and (more) contour plots. In particular consider how to assess spatial coherence in the thermocline in the nearfield of the discharge. Use 3-D displays to give the whole-domain context.

Choose more levels to display and to compare data, especially the pycnocline and/or chlorophyll maximum. Also show model results for Stellwagen Basin, which is the likely sink for labile organic carbon.

and is known to have experience dissolved oxygen depression in late summer.

Compare to 1992 monitoring data, even though it is from a different year.

Other items to plot are the two classes of phytoplankton, depth-integrated production (is model production lower than measured production, or is loss too high?), and depth-integrated chlorophyll.

Use the hydrodynamic model for a full 18 month run. Calculate the carbon-to-chlorophyll ratio from the 1992 data, including seasonal variations, and use that in the model.

It was not clear how boundary conditions are arrived at/applied. The data along the boundary are sparse, and many of the model parameters were not measured at all. Related question: how close to the boundary should we have confidence in the model predictions? This is something to test.

The preliminary present/future comparison showed some odd behavior that should be investigated; it may indicate that the model is not working correctly. For example, why is the plume at the new outfall site trapped all year? If there is any denitrification in Boston Harbor (even 10%), why is there no perceptible difference in Cape Cod Bay? Other methods of displaying the data should be used along with the probability plots to perhaps bring out some information that would explain these results.

MWRA MEG MEETING - ATTENDANCE LIST 7/20/93

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Jack Kelly	Battelle Ocean Sciences	617-934-0571

Agenda

Massachusetts Bay Model Evaluation Group
Meeting #4

October 13, 1993

10:00 a.m. - 4:00 p.m.

Board Room
Building 39 (100 First Avenue), 3rd floor
MWRA

Introduction	Mike Connor
Water quality model	Jim Fitzpatrick

- Analysis of water quality result with/without sigma-level corrections
- Analysis of water quality model boundary conditions
- Water quality model calibration results
  - vertical profiles
  - surface layer contours
- Comparison of 1989-91 data to 1992 monitoring data

Hydrodynamic model	Rich Signell, Alan Blumberg

- Additional hydrodynamic model calibration results
- Circulation based on model SST instead of data SST
- Characteristics of future outfall plume

Future priorities/schedule	Wendy Leo, Mike Connor

- Criteria for model acceptance
- Model projections for treatment scenarios
- Structure for model follow-up
  - toxic dinoflagellates
  - sediment transport model
  - long-term model "home"

MEG Executive Session

MWRA MODEL EVALUATION GROUP MEETING
~~SEPTEMBER~~ OCTOBER 13, 1993
MEETING SUMMARY

1. INTRODUCTION

Mike Connor indicated that MWRA would like to use the mid 1993 to late 1994 construction "gap" to reevaluate the CSO and secondary treatment system options. Part of the evaluation would be to use the water quality model to assess BOD_5 and nutrient removal effects of the alternatives.

2. WATER QUALITY MODEL

Jim Fitzpatrick discussed model comparisons, modifications and data displays that were undertaken since the 7/20/93 MEG meeting. When comparing a non-sigma level corrected 30-day simulation, for the hydrodynamic and water quality models using an uncollapsed grid with one-hour averaging, the graphics showed similar results. There was a 1-2°C difference between the models in surface-bottom differences near Boston Harbor. This is likely due to the upwind differencing scheme in the water quality model; the hydrodynamic model uses a central differencing algorithm. With the sigma-level correction in place the delta-t error gets somewhat worse between the two models; however, this correction is in-place to help establish the appropriate level of stratification in the hydrodynamic model. Bottom line, the sigma correction makes little difference in the water quality model once the advection scheme is changed.

Using a collapsed grid to compare sigma-level differencing shows some differences in the model (over a 30 day period) for the temperature/salinity results also, but these differences are due mostly to grid collapse and not using the central differencing scheme in the WQ model. This held even for the 12 month projections. Basically it was found that the numerical diffusion differencing schemes caused the greatest differences between the models while sigma level corrections were felt to be insignificant.

An additional MEG request was to graphically examine model versus actual data for most parameters in the 1989-91 period. In surface contours in August, several parameters were similar, (dissolved oxygen, dissolved silica, phosphate, ammonia, PON) while some parameters showed differences. Modeled salinity was higher (1 ppt) offshore but lower than observed data in Boston Harbor; DIN was about 50% higher in Boston Harbor but similar offshore; and both chlorophyll *a* and POC were higher in the model, particularly just outside Boston Harbor. The MEG observed that these latter two parameters are important to the treatment plant and that carbon; chlorophyll ratios in the model should be higher. Gradient direction was also somewhat different for several parameters: observed gradients went toward the southeast (from Boston Harbor area) while modeled gradients had a more northeasterly axis. The model showed gradients for many parameters in the Wellfleet (Cape Cod) area but no observations were available for comparison. The results will be compared to historic satellite ocean color observations.

Vertical comparisons were made for five-point (depth) instantaneous (observed) data versus a modeled seven-day average. In June 1990, several parameters were underestimated in the surface: POC, DIP, chlorophyll; because of low chlorophyll, nitrogen in the surface was inadequately estimated. Silica was reasonably close. In summary, the spring bloom occurred 1-2 months late in the model but the summer stratification, once set up, was good for several parameters, but not as good for some parameters (chlorophyll, nitrogen).

Due to modeled differences, the value of adjacent temporal (weeks) and spatial (grid) comparisons was discussed to see if there is simply an "offset", since general conditions are predicted by the model. The value of plotting the percentage of data points that fall within the range of the model results for certain time periods was discussed, to help evaluate the amount of data coherence. A model comparison against 1992 (or 1993) data was also discussed. the MEG would like to see a summary of what comparisons could be made (to determine model "adequacy") in an "active" document which MEG and the modelers could work on. Initial test cases would be a place to start.

Questions about where to make model adjustments arose. Heat flux calculations need to be updated to the WQ model as a first step. Boundary conditions were also discussed, where timeseries showed reasonably good fit, although POC, silica and certain nutrients are off somewhat. Boundary area data from D. Townsend (UME) and T. Durbin (URI) was suggested as a source of help since it is not clear that present data are adequate to see the gradient of nutrients. Differences in 1990 versus 1992 observed data were also discussed; use of these later data would help validate the model.

Priorities at this point are seen as: 1) Fixing stratification is the first priority (heat flux adjustments may take care of this); 2) Determine how well heat transfer is picked up in intermediate layers (vertical transport/stratification is very important to this model); 3) Get 1992 hydrodynamic data input into WQ model; 4) Understanding boundary condition effects on the nutrient budget would help. It would be helpful to examine other "boundary area" data available (i.e. 1993 D. Anderson/T. Loder data), as monitoring data are not available from far enough east and north to fit this model grid.

3. HYDRODYNAMIC MODEL

Rich Signell discussed model corrections made since the last MEG meeting. The sigma-level correction demonstrated the appropriate level of summer surface-bottom stratification at Buoy B and modeled versus observed data were well correlated at 5, 23 and 33 m depths. The exception was Cape Cod Bay where surface temperatures are ok but bottom waters are modeled as being too warm.

Matching specific time periods for modeled versus observed data corrected much of the variance observed earlier in the low frequency ellipses. The modeled mean flow is still somewhat low, except at the outfall where observed mean flows are low anyway. In some cases there is much spatial variability in the model's mean flow direction in the regions near certain current meters. This contributes to the perceived error in the model when comparing against data. For example, this was demonstrated just north of Race Point where M2 tidal residuals were high in one grid cell (near shore) but low in the next (offshore) grid. Seasonally, no further model changes will be made in summer or winter; in the spring period corrections to mean flow will be sought; while corrections to the predictions at Race Point will be addressed.

The heat flux problem is being addressed by Alan Blumberg; progress has been made but the solution was not available in time for this meeting. The bottom waters in Cape Cod Bay are still too warm and this may be occurring elsewhere. Basically, there is too much mixing since surface temperatures are ok but bottom temperatures are too warm. The production of energy in the vertical mixing will be turned down to see what happens.

Priorities are seen as: 1) Work on the spring Gulf of Maine boundary flows; work on modifying mean boundary elevations; 2) Work to understand flows in the fall at Race Point; 3) Undertake process oriented tasks - examine winter storms, upwelling; 4) address excessive heating and plume issues.

4. MEG PRIORITIES

Water quality

- Supply information to MEG as to where the model could most readily be improved

- Improve the spring bloom predictions
- Explain degree of spatial/temporal variability
- Compare X-Z transects of model vs cruise data
- Compare model vs observed temperature/ salinity at all 9 sites
- Examine boundary condition issue, including new/other data as available
- Look for strong correlations between some parameters, then only concentrate on key parameters (to save time)
- Try to include more nearfield data
- Look at physical events to see if they are reflected in biological events

Hydrodynamic

- Demonstrate that stratification is being modeled appropriately at all layers
- Incorporate heat flux modifications
- Increase Gulf of Maine flows in spring
- Work on spatial resolution at Race Point
- Get 1992 data up and available for WQ model
- Continue to look at plume characteristics.

5. FINAL COMMENTS/THOUGHTS

- The model is needed at this point to examine some treatment scenarios and their affects on BOD_5 and nutrient loading.
- The value and precision of the models depends on by what criteria it is being judged: carbon loading on the bottom; nutrient concentrations; chlorophyll estimates; dissolved oxygen concentrations, etc.
- MEG would like some sample scenarios to see how the model is behaving; look at some monitoring data to see what the match is like
- An exchange of what comparisons the MEG/modelers would look for was requested
- Getting an areal estimate of primary productivity would help
- Make more effort to get EPA Region I (regulatory) at the next meeting.
- The next MEG meeting is tentatively scheduled for the week of January 17; date to be fixed upon consultation with MEG members. It is anticipated that the model development (and therefore MEG) oversight will end next summer (1994).
- The modelers should propose criteria for model acceptance and submit them to the MEG for review and comment.

MWRA MODEL EVALUATION GROUP MEETING

~~SEPTEMBER~~ OCTOBER 13, 1993

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MEETING NOTICE

MWRA MODEL EVALUATION GROUP MEETING
ON
MASSACHUSETTS BAY/CAPE COD BAY HYDRODYNAMICS AND WATER QUALITY MODEL DEVELOPMENT

WEDNESDAY, JANUARY 19, 1994
10:00 am - 4:00 pm
FORE RIVER STAGING AREA, QUINCY, MA
BUILDING 10, CONFERENCE ROOM 10

MEETING AGENDA

HYDRODYNAMIC MODELING

- Calibration and Verification of Circulation Physics
  - Model/data comparisons
  - Influence of heat flux
  - Race Point Resolution
  - Sensitivity to Gulf of Maine flows
  - Impact of 18 vs. 10 vertical layers

- Skill of Outfall Plume Dynamics
  - ECOMsi vs. ULINE
  - Plume characteristics

WATER QUALITY MODELING

- Qualitative Analysis of model calibration with 1989-1991 (18 mo.) dataset
- Demonstration of model projections and discussion
- Discussion of statistical methods to judge model adequacy (for 1992-1993 data) based on examples from other sites.

DIRECTIONS

FROM NORTH: South on I-93 through Boston (or Rt. 128 around Boston) to Route 3 South

Take Exit 17 in Braintree - East on Union St.
Left on Rt. 53 (Quincy Avenue)
Right on East Howard Street

You should be able to see the General Dynamics goliath crane at the FRSA from some distance away.

FROM SOUTH: Same, except take Rt. 3 N to Exit 17 in Braintree

MWRA MODEL EVALUATION GROUP MEETING

JANUARY 19, 1994

MEETING SUMMARY

WATER QUALITY MODEL

Jim Fitzpatrick of HydroQual presented a broad overview of the calibration status of the model:

• Yearly averaged phytoplankton chl *a* as computed by the model was compared to yearly averaged estimates derived from satellite data (from Amy Michelson's 1990 Master's thesis), with favorable results: both model and data showed elevated yearly averaged chl *a* in Boston Harbor, Plymouth Harbor and in the arm of Cape Cod.

• The model reproduced the decline in silica concentrations in Cape Cod Bay before Massachusetts Bay, as observed in the data.

• The model reproduced the annual cycle of nutrients: high concentrations of inorganic forms occurred in the late fall/early winter, low concentrations in the spring and summer.

• Model-data comparisons for salinity, temperature, dissolved oxygen and chl *a* from Boston Harbor were generally favorable, with some discrepancies that may have resulted in part from use of the older version of the hydrodynamic model with unresolved heat flux problems, and an underestimation of freshwater (riverine and storm water) flows.

• The model did not fully reproduce some vertical features of Mass Bay observed in the 1992 data, which may also have resulted from the use of the older hydrodynamic model.

• The observed early phytoplankton bloom in Cape Cod Bay did not appear in the model until March-April and did not shut down in May.

• The calibration run showed bottom layer dissolved oxygen supersaturation in shallow areas, which was not observed in the monitoring data.

• Although some mis-calibrations were observed, the model was able to reproduce several seasonal and spatial elements of the monitoring data.

In addition to the calibration runs, a projection scenario was modeled by moving the effluent location to Mass Bay from Boston Harbor; the model was run for a three and one-half year period to permit sediments to come to a new equilibrium:

• The projection predicts the average DO in the bottom layer to increase in Boston Harbor by approximately 0.5-1.0 mg/L and to decrease near the new outfall by approximately 0.3-0.6 mg/L.

• Dissolved inorganic nitrogen concentrations, sediment oxygen demand and fluxes are predicted to decline in Boston Harbor but to rise slightly in bottom layers near the outfall.

• The projection predicts that an area of low dissolved oxygen (< 8 mg/L) in Broad Sound increases in size and moves offshore. The area of bottom water undersaturation in Stellwagen Basin is also predicted to increase in size.

• The projection predicted surface chl *a* concentrations to decline relative to calibration results.

• In general, projection results indicated that most water quality impacts would likely be localized to the vicinity of the future outfall.

Discussions then turned to an examination of the criteria that could be used to evaluate model adequacy. Different methods were illustrated with examples from the Chesapeake Bay, Potomac River, and Lake Ontario studies. The following methods were presented:

• Student's t-test: used to compare mean parameter values from monitoring data against mean values computed by the model; the "goodness" of the comparison is typically based on a 95% level of confidence.

• Relative error test: comparison of modeled results with observed values (observed-predicted/observed), expressed as a percentage difference.

• Linear regression: evaluate R^2, slope and intercept of observed vs. modeled values.

There are enough 1992 monitoring data from the nearfield to test model performance in this way; there are not enough data available from farfield areas. A reasonable expectation is 20-25% relative error, or 80% agreement between observed and predicted mean values. It was suggested that the tests focus on parameters of greatest interest.

As this discussion evolved, a question was raised about the value of these types of tests, since test outcome is dependent on the quality of the monitoring data as well as on the quality of the model. These tests indicate how well a model reproduces observed data, but they do not suggest why it might not. A qualitative "does it make sense" evaluation was therefore suggested:

• Are limiting factors correctly specified?
• Do nitrogen concentrations look right for the time of year?
• Do parameters expected to behave conservatively do so?
• Are boundary conditions properly specified?

Other suggestions included looking at narrower, more critical ranges of the data, focusing on parameters of primary interest, and conducting a sensitivity analysis. The first goal, however, is to calibrate the model with 1992 data. Once the model has been "fine-tuned," a sensitivity analysis could be undertaken.

Several other suggestions, comments and questions arose during this discussion:

- To maintain consistency with standard oceanographic practices, nutrient concentrations should be reported in micromoles/liter (µM).

- Fluxes should be reported in millimoles/m^2/day.

- Superimpose the 1% light level on transect plots.

- Prepare difference plots for observed vs. modeled results, once results are "final."

- Several sediment parameters are computed; which should be displayed?

- Evaluate sensitivity to natural load variability, i.e., "wet" vs "dry" vs "average" year loadings.

- Map the existing point and nonpoint sources and define a "base load" scenario: define a "pristine" version of the model by removing Boston Harbor loadings; compare with- and without-harbor scenarios.

- Focus on time-series plots in data presentations instead of statistics, at least until model is "final."

One last "food for thought" question was raised: how do you balance the range of anthropogenic factors against the range of natural phenomena, i.e., those factors that can be regulated vs. those that cannot?

HYDRODYNAMIC MODEL

Rich Signell from USGS discussed progress made in the improvement of the hydrodynamic model:

- The defect in heat flux has been fixed, such that Cape Cod Bay is no longer too warm, and Boston and Plymouth Harbors are no longer too cold.

- Comparisons between the 18- and 10-layer models indicate that the only advantage of the 18-layer model is its ability to discern the surface layer that occasionally develops.

- At the next meeting, spring profiles will be shown to examine whether or not the spring pycnocline is stronger; at present, the model does not have as sharp a pycnocline as the data.

• Efforts to try to reduce the level of mixing in the model will be continued.

• The mean flow of the model was judged to be too weak, particularly around Manomet and Scituate: in May, just after the occurrence of the salinity minimum, the model predicted a northward flow, although 1991 data showed southward flows for this period.

• Problems specifying mean flow may have resulted from insufficient detail in wind data, as upwelling- or downwelling-favorable winds can affect the direction of currents.

• One possible solution would be to vary winds across the model domain; an attempt will be made to collect data from several USCG stations along the coast to fine-tune this aspect of the model.

Another issue in determining circulation is the specification of freshwater flows. Using 1991 data the model predicts too high a salinity, indicating that one or more sources of freshwater have been missed. In 1990 the Kennebec and Androscoggin River flows were larger but temporally similar to those of the Merrimack River. In 1991, however, the Kennebec and Androscoggin Rivers exhibited large discharge events that did not appear in the record of Merrimack River flows. Doubling Merrimack flows or boosting the coastal current did not fix the flows or increase the Manomet and Scituate flows; therefore some work appears to be necessary on how the western Gulf of Maine is specified in the model.

PLUME DYNAMICS

Alan Blumberg of HydroQual discussed the ULINE and ECOMsi models, indicating that they compared well in predictions of plume rise height and thickness, although ECOMsi gave somewhat lower dilutions than ULINE. It was noted that the ECOMsi model accounts for background concentrations, while the ULINE model does not. A question was raised about how plume rise height relates to stratification; Alan will evaluate this for the next meeting.

ISSUES FOR NEXT MEETING

• Rich Signell will finish his 1992 model run in early February; two months after the 1992 run is completed the water quality model will be completed.

• Jim Fitzpatrick will investigate the loading issues and primary productivity while waiting for Rich's results.

• A sensitivity analysis will be conducted with the water quality model, beginning with a scenario with no loading from Boston Harbor, strong stratification, and lots of upwelling; boundary conditions may also be adjusted.

• Efforts will progress to modeling to the full build-out with secondary treatment. The present conditions vs. future conditions comparisons will be evaluated in light of these results.

• The effects of spatially variable winds on the hydrodynamic model will be evaluated.

• Boundary conditions of the western Gulf of Maine model will be reevaluated with 1993 and 1994 data.

• Water mass residence times will be evaluated through a comparison of Rocky Geyer's report and Susan Becker's thesis.

• Ideas will be collected for a decision on the structure and scope of the final report on the hydrodynamic model; what is the desired product?

MEG EXECUTIVE SESSION SUMMARY

The MEG is pleased with the significant progress made since the last meeting. With respect to both models, the following specific issues should be addressed:

• Do the models reproduce the amount of natural variability?

• Use 1994 as a model validation year.

• The models should be used to predict a "worst case" defined as the combination of forcing which gives the largest swing in environment result, e.g. 1976 in the NY Bight. For example, looking at wind conditions, one could model the following scenarios:

- upwelling-favorable summer
- quiescent summer
- "alternating conditions" summer in which the pycnocline alternately forms and breaks down.

The goal is to determine what synergy is needed to make the small impact area bigger.

With respect to the hydrodynamic model, the 3-week freshet period in 1991 needs to be resolved, as this type of event is important to the red tide issue.

The following items concerning the water quality model need to be addressed:

• The subsurface chlorophyll maximum (SCM) is there in the data, why is it not there in the model? There are too many competing theories about why the SCM forms to test them all, however, HydroQual should determine whether the absence of the SCM is related to the fact that the model pycnocline is too weak.

• MEG would like to know what exactly was done to "calibrate" the model. Besides changing the dependence on temperature in the sediment submodel, were any other model coefficients varied from their Chesapeake Bay values? Doesn't the Chesapeake Bay model have three functional groups, rather than two?

• The many available data should be used to check the carbon-to-chlorophyll ratio, and the "blend" of phytoplankton with different ratios.

• Look at the water quality monitoring data to determine the form of the nitrogen entering from the open ocean boundary, and the importance of the boundary in the budget for different nitrogen forms. Check out whether the large amount of nitrogen at the boundary is in an unavailable form (as in Chesapeake Bay).

• A closer look at light would be helpful. Some specific MEG requests:

- Plot the 1% light level position relative to the pycnocline. In the data, the pycnocline usually falls between the 10% and 1% light levels.
- What is HydroQual using for alpha, beta and P_{max} ?

• Monitoring/modeling relationship needs to be better defined; also, the question of the long-term model "home" will need to be resolved. In Long Island Sound, the model is used as the monitoring framework. This indicates a need for regional support for the model. In any case, the MEG encourages the outfall monitoring group to use the model results to help redesign the monitoring program. The modelers should use the monitoring data to validate the model.

MWRA MODEL EVALUATION GROUP MEETING

January 19 1994
~~SEPTEMBER 15, 1993~~

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MEG meeting #6 - 9/1/94 - agenda

Introductory remarks — **M. Connor**

Hydrodynamic model — **R. Signell**

- 1992 model/data comparison
- 1992 *vs.* 1990
- boundary conditions
- sensitivity analyses
- schedule/next steps
- discussion

Water quality model — **J. Fitzpatrick**

- changes to algal stoichiometry/subsurface chlorophyll maximum

- LUNCH -

- methods for model calibration
- 1992 model/data comparison - including new types of plots
- 1992 *vs.* 1990
- boundary conditions
- sensitivity analyses
- schedule/next steps
  - how should model projections be presented and evaluated?
  - report schedule
  - next meeting - October 1994?
- discussion

Initial dilution modeling (if time permits) — **A. Blumberg**

- unresolved issues from ECOMsi/ULINE comparison

MEG Executive Session

SUMMARY OF MASSACHUSETTS/CAPE COD BAYS MODEL EVALUATION GROUP MEETING

MEETING #6
SEPTEMBER 1, 1994

Mike Connor - Opening Comments

It is important that the model be completed soon, to be available to the public debate process for the new outfall.

The project goals are to:

- run the build-out projections in October,
- produce a draft report in late October-early November
- produce the final report in December.

The final product should be two separate reports, one for the hydrodynamic model and one for the water quality model.

Important events occurring this fall include:

- MWRA will propose the final build-out plan for Deer Island
- EPA is planning to release draft permit, possibly in November.

Rich Signell - Hydrodynamic Model

The open ocean boundary is driven by *low-frequency* water surface elevations, not tidal elevations. The low frequency elevation was held at zero for the southern two-thirds of the boundary; a separate western Gulf of Maine model was used to specify elevation along the northern one-third of the boundary.

Bogden's results, using "hindcasting," indicated that interior circulation is not sensitive to mean sea level tilt in the southern end, but is sensitive to *local* windstress.

Two problems in the calibration run were that Cape Cod Bay was too warm and too "salty." The heat flux problem was solved by using a model-predicted surface temperature rather than monitoring data; an additional beneficial outcome was that the model was better able to represent the freshwater plume originating in the GOM.

Vertical temperature comparisons between the model and 1990 data showed that surface (-5m) temperatures tracked reasonably well.

Similar comparisons with 1992 data also showed good agreement with surface temperatures, but the model missed some specific events in bottom temperatures, particularly in the fall.

Temperature differences could be lessened by artificially adding freshwater at the boundary, but this is not important to the hydrodynamic model itself; it needs to be determined how important this is to the water quality model.

For temperature, salinity, and particle excursions, the model statistics reproduced monitoring data statistics very well, even though specific extreme events were not always matched.

A model experiment was run to evaluate harbor flushing; dyed effluent was released from Deer and Nut Islands, and tracked over a month or so. Flushing time calculated at about 7 days, which lies within other estimates of 2-10 days.

The biggest problem in the model is predicting exactly when and where the GOM plume enters the bay; if boundary conditions were perfectly specified, possibly this could be predicted. Two possible reasons why the model has difficulty describing the GOM plume are that the plume itself is very event-oriented (something the western GOM model attempts to address), or possibly the model physics can't adequately describe plume movements.

Two different horizontal mixing regimes were examined: Smagrinski vs. constant mixing (1 m^2/sec); no real differences were obtained.

"Worst case" scenarios of weak winds and river flows were modeled, with forcings reduced by a factor of 10; the model "blew up" because of heat flux problems created by the lack of mixing. A more realistic scenario based on historical data may involve a reduction in forcings by a factor of 2-3.

The presentation was closed by asking the MEG what type of management issues the model is suited to address in its present form.

Jim Fitzpatrick - Algal Kinetics

Algal growth rate is a function of temperature, irradiance and nutrients. The conventional model, which used winter and summer functional groups, had nutrient ratios fixed either to the Redfield ratio or according to nutrient limited conditions.

Another approach based on Chalup and Laws (1990) and Shuter (1979) specifies algal cell carbon as a function of structural carbon, carbon in storage, and carbon produced by light and dark reactions (C = S + R + P + E). This approach allows for variable stoichiometry.

The model assumptions were as follows:

- Under nutrient saturation, carbon storage (R) = 0.
- Carbon produced by light and dark reactions are equal.
- Respiration rate = basal respiration + growth.
- Net growth = carbon produced by dark reactions - respiration rate.
- Nutrient uptake is uniform between structural and light and dark compartments (no nutrients in the storage compartment).
- Each compartment shows balanced growth.

These assumptions mean that there is no luxury uptake of nutrients.

Model equations showed the following:

- The N:C ratio is dependent upon the ratio of the nutrient limited growth rate (μ) to the nutrient saturated growth rate (μ_s).

- The N:C ratio equals the Redfield ratio under nutrient saturated conditions, but exceeds the Redfield ratio when nutrients are limited. An F factor determines how far beyond Redfield the ratio can go.

Chalup and Laws showed the following:

- For one algal species in the lab, N:C and Chl *a*:C ratios are higher under low light than under high light, under nutrient saturated conditions.

- N:C ratios do not differ between low light and high light conditions as a function of the relative growth rate (μ:μ_s), but Chl *a*:C is still higher under low light compared to high light.

The bottom line of this discussion was that algae are shade-adapted.

The Chl *a* maximum is typically not associated with the biomass maximum, but occasionally is associated with biomass and DO maxima at depth. This, however, is more a function of shade adaptation.

The model has self-shading built in, but the system is more nutrient limited than light limited. The model does not presently account for photoinhibition, and this is supported by available data; if, however, the MEG thinks it is important, it can be added in.

Using the Chalup and Laws assumptions, the model's growth-dependent respiration is ≈ 0.28 at the surface; this drops to 0.03 at depth, so the vertically averaged value is ≈ 0.15, which is what is observed in the data.

At steady state, it should not matter whether the effluent is 1° or 2° since the cycling rate is much higher than the flushing rate.

A sensitivity analysis comparing 1° and 2° effluent (making the nitrogen in the effluent all nitrate) could be run to test this idea.

At end of this discussion, Mike Connor noted that Chl *a*:C is critical in determining if surface and bottom effluent discharges perform differently; how the phytoplankton growth is specified is critical.

Rich Isleib - Loadings and Calibration Data

Loadings were estimated from four sources of information:

- Treatment plant loading records.
- 1990 Menzie-Cura report (separates North Shore/Boston Harbor from South Shore/Cape Cod).
- Updated MWRA report on Boston Harbor loading.
- Atmospheric deposition report by Dr. Steve Zemba (some questions were raised later about the quality of this information).

For both the calibration (1989-1991) and verification (1992) periods, relative loading magnitudes were as follows (noting that there was no MWRA sludge during the verification period):

- Carbon: MWRA WWTP > Non-MWRA WWTP > Atmospheric > Non-Point Source ≈ Riverine Source > MWRA Sludge

- Nitrogen: MWRA WWTP > Atmospheric > Non-MWRA WWTP > Riverine Source ≈ Non-Point Source > MWRA Sludge

- Phosphorus: MWRA WWTP > Non-MWRA WWTP > Non-Point Source ≈ Riverine Source > MWRA Sludge > Atmospheric.

HydroQual will calculate the boundary load, and try to discern how much of the load is labile vs. refractory.

Some 1992 boundary conditions had to be specified using UNH/UMB data from 1989-1991 because no other data were available.

The model state variables for which there are data include temp/sal/DO, carbon measurements, nitrogen measurements, phosphorus measurements, and silica measurements. Biogenic silica and POP measurements are not made.

Comparisons of the calibration and verification data sets:

- Both data sets show similar temperatures; there is no stratification in the President Roads area, but there is in the other three areas plotted (Northern Mass Bay, Central Mass Bay, Eastern Cape Cod Bay).

- Verification salinities are fresher than calibration salinities, particularly in bottom waters, although the vertical structures are similar.

- The two datasets are similar for DO, Chl *a*, N, and P; N concentrations are higher near the outfall; N appears to be the limiting nutrient based on PO_4 and NH_4 data.

Jim Fitzpatrick - Verification Model Results

The data and the model are supersaturated with DO through the spring and summer due to 1° productivity; undersaturation occurs only in the fall due to burn-off of organic carbon (in data and model). The model does, however, underestimate DO most of the time.

It was reiterated that the model tracks carbon, and other nutrients and Chl **a** are predicted from that according to assumptions from Laws and Chalup.

Model results from the different stations were discussed, going from Boston Harbor outward into Cape Cod Bay. In general, POC and Chl *a* are overpredicted in Mass Bay: a late March/April freshet brought Si into the system; the model and data both suggest that biomass decreases with distance from the harbor, and that the system slowly becomes nitrogen-limited with distance. Summer Chl *a* is underpredicted in Cape Cod Bay, possibly because of the settling of a *Ceratium* bloom.

Vertical profiles at specific stations on specific dates were discussed. In general, Chl *a* and DOC track the data fairly well, as does DO. However, POC and DO are underpredicted in deep water in Cape Cod Bay. The model fails uniformly at depth, as the *Ceratium* bloom was missed.

There was some discussion about nitrification: organic carbon and nitrogen need to be oxidized more so than they are currently; there is too much ammonia and not enough nitrate, so the nitrification needs to be increased in the model.

There was a discussion of the types of phytoplankton used in the model, and the use of only a few taxa to represent the entire assemblage; more detail is not possible due to a lack of knowledge about the details of each species. Some discussion occurred about temporal resolution (5 day averages vs. monthly averages, and what kind of information each provides, i.e. the "climate vs. weather" analogy). The main point is that the Chalup and Laws algal kinetics are able to show the peak at depth. It was noted that Jim needs to resolve units problem with respect to light inhibition.

A question was asked about how the model specifies the phytoplankton assemblage in June; the model uses a 50:50 split between the winter and summer group; it was suggested that in reality it is probably 100% of one group.

Several figures showing concentration contours in the water column were discussed; these figures showed when data and model predictions matched, and gave approximate differences where they didn't match.

A question was raised about percent composition of bottom substrates; the model assumes uniform composition.

The sediment flux model was discussed; the model generally had difficulty predicting the high SOD and nutrient fluxes in Boston Harbor, but improved with increased distance from the harbor.

The sediment model was rerun for calibration data using the Laws and Chalup assumptions; these assumptions generally improved the ability of the model to reproduce the data. The rerun also improved vertical differentiation, showed the phytoplankton peak at the pycnocline, and helped to pick up some instances when the Chl *a* maximum and higher carbon concentrations occurred away from the surface.

Some further improvements to be made include work on denitrification (DIN won't change, but it may help resolve differences between ammonia and nitrate) and adjustments to C:Si, although these will not alter the *major* features of the model.

Discussions

Questions that were asked about the model were:

- ~~Are limiting nutrients correctly specified?~~
- Does the amount of nitrite look right for the time of year?
- Do parameters that are expected to behave conservatively actually do so?
- Are boundary conditions correctly specified?

Responses to these questions were:

- Since the DIN plots are right on the data, the nitrogen specifications appear to be correct.

- For conservative parameters, TOC is high in the vicinity of Boston Harbor; phosphorus and DO are acceptable.

- There is not much more that can be done to improve boundary conditions without additional data.

- Since there was no 1992 boundary data available, boundary data will be back calculated from data coming from inside the model grid.

A lot of discussion followed on Amy Michelson's data (CZCS); would this fill in some of the current data gaps? HydroQual will examine the data if MWRA can provide it.

The suitability of the model for running projections was discussed. More information on short term (diel, storm response) DO changes was requested

The calibration of the water quality model was asserted to be acceptable; algal groups and kinetics were adjusted. A specific description of what was changed and by how much, and what the implications were, was requested.

Changes to the sediment model included temperature changes and phytoplankton light formulation; the refractory components were made higher than that in the Chesapeake Bay model.

Further emphasis was made on the need to increase nitrification.

A sensitivity analysis needs to be run to further examine boundary conditions; how much nitrogen comes from the boundary and atmospheric sources and how much comes from Boston Harbor? A general means of examining this is to turn off internal loads and see what happens, then turn off boundary and atmospheric loads and see what happens.

For the calibration demonstration, MEG members would like to see a letter from HydroQual documenting the correctness of primary production estimates, some comparisons with CZCS data, and documentation of changed parameters.

Discussions on the structure of projection runs and sensitivity analyses continued, with the following scenarios identified:

- Change boundary loads (N, P, Si) by 25%.
- Turn off shoreline C, N, P, and Si loads (but not freshwater flow).
- Turn off atmospheric loads.
- Move existing loads to new outfall location.
- Move load to new outfall location and change treatment levels (1° to 2° to 3°), i.e. BOD, TSS, N, P all change.

Discussions concluded by setting the upcoming agenda:

- New runs should be completed by mid-October.
- Finalize the hydrodynamic model.
- Next meeting in late October-November.

MASSACHUSETTS/CAPE COD BAYS

MODEL EVALUATION GROUP

MEETING AGENDA

NOVEMBER 17, 1994

Introductory Remarks	M. Connor
Water Quality Model Projections Sensitivity analyses Initial reactions to final report draft	J. Fitzpatrick
-LUNCH-	
Hydrodynamic Model Sensitivity analyses The next generation: sediment transport modeling	R. Signell
MEG Executive Session* MEG Product MEG Future	R. Beardsley

* This is intended to be a focussed discussion among Drs. Beardsley, Adams, Giblin, Harleman, O'Reilly, and Paul about what the next steps are for the MEG. If you want to contribute to the discussion, please talk to Wendy Leo before the meeting.

SUMMARY OF MASSACHUSETTS/CAPE COD BAYS MODEL EVALUATION GROUP MEETING

MEETING #7
NOVEMBER 17, 1994

Mike Connor - Opening Comments

• This meeting's goal is to move along as much as possible towards completing model calibration and verification.

• The draft NPDES permits are due to be released soon, and the model could be an important tool for the public hearing process. The model should also be used to evaluate the reasonableness of the NPDES permit conditions; the MEG itself should have a voice in this process.

Jim Fitzpatrick - Updated Water Quality Model

• Modifications to algal kinetics (i.e., increased nitrification) that were discussed at the September meeting were made, and the model was rerun for both the calibration and verification periods.

• Projection runs are not yet complete, but should be available by the end of November.

• The basic model framework was reviewed, with the following important points reiterated:

a) Most of the available data come from the nearfield area; there are many more gaps (spatially and temporally) in the farfield dataset.

b) Salinity is the key to connecting the hydrodynamic model to the water quality model.

c) The water quality model grid represents a 3 x 3 collapse of the hydrodynamic model grid over most of the model domain; the aggregation protocol is slightly different in one farfield area. Hourly averages are computed for each cell in the model domain. The mixing coefficient for each aggregated grid cell was the lowest from any of the 9 grid cells within it.

d) While there are 12 vertical sections in the hydrodynamic model (an extra 3 in the upper layer to help correct the early heat flux problem), there are only 10 in the water quality model.

• The specification of boundary conditions was reviewed. For the 1989-1991 period, data were available for only 11 of 19 months, so a "simplifying assumption" of a spatially constant boundary was made; the exception was the period of February to April in 1991, when some spatial gradients were apparent in the data.

• Boundary conditions were specified differently for the 1992 period; data were available for PO_4, NH_4, NO_2+NO_3, DSi, and DO; all other parameters had to be estimated from the 1990 dataset.

• Revised calibration and verification runs were discussed for several grid cells (15,16 at the northern boundary; 10,15 east of Hull; 11,18 near the future outfall; 8,18 just outside of entrance to Boston Harbor; and 6,4 in the southwest corner of Cape Cod Bay). Some improvement in results was shown, but in general the model misses the mid-late February phytoplankton bloom (model shows this occurring in March, missing the *Phaeocystis* bloom); the model also tends to underestimate DO at depth, and is high on NH_4 and low on NO_3, especially in bottom layers.

• The high degree of intraseasonal variability in Chl *a* was discussed; since similar variability is not seen in physical parameters, what's causing this? Possibilities include solar radiation or tides. Dom DiToro and Rich Signell both agree that this is probably driven by hydrographic variability; Chl *a* data plotted over a conservative tracer might help elucidate this. The important point here is establishing whether or not this variability is real, or if it represents a limitation in the model (i.e., establishing model credibility).

• The results of the sediment model were discussed:

a) The model tracks SOD data in cell A (grid cell into which sludge was discharged) reasonably well, but overpredicts SOD in cell B (Deer Island outfall) and underpredicts in cell C (near the future outfall).

b) Ammonia flux is underpredicted at A, about right for the one data point at B, and underpredicted at C.

c) Nitrate flux is about right in A for April, May and June data points (flux about equal to zero), but is high for August, September and November data points. The model predicts a small flux out of the sediments for the one point available in cell B (which describes a small flux into the sediments), and accurately reproduces the very small outward flux at C.

d) The model captures the seasonal PO4 flux at A, although fluxes in April, May, June and November are slightly overestimated. The small September outward flux at B is slightly overpredicted by the model, while the flux at C is accurately reproduced.

e) In general, the sediment model reproduces spatial and seasonal trends fairly well in Boston Harbor and the nearshore area of Massachusetts Bay, but more data are needed to evaluate its performance elsewhere.

- Vertical profiles from the water quality model were reviewed. Nutrient data show a homogenous profile in November, but the model shows stratification; vertical mixing was increased by a factor of 10 across the model domain. Rich Signell noted that the problems with stratification are likely not due to model structure, but to the boundary conditions applied to that structure. These problems occur mostly in deep waters in the farfield area.

- Chl *a* satellite photos from March-June in 1979 showed high levels in Cape Cod Bay, without correspondingly high levels in upper Mass Bay; some blooms were evident near Plymouth and Boston Harbor. Since this spatial variability varies from year to year, the model has difficulty picking it up.

- Bay-wide color displays of model-predicted Chl *a* results showed the following:

a) Bloom starts in January and February near Cape Cod and in March near Boston Harbor; by April, the bloom lies more in Boston Harbor than near the Cape.

b) Chl *a* increases through August in Boston Harbor, but by less than 1-2 $\mu g/L$ in the rest of the Bay.

c) Chl *a* declines between September and December.

- Sensitivity analyses were discussed, with the following results noted:

Nitrogen: In August, a 25% reduction in boundary loading resulted in only a small change in nitrogen concentrations, since there is not much nitrogen in surface waters in August; turning off atmospheric loadings did not produce any changes; turning off all loads other than boundary input caused a sharp reduction in surface and bottom water nitrogen in and near Boston Harbor, and a smaller decrease in eastern Cape Cod Bay.

Chl *a*: In March, there is some reduction in Chl *a* accompanying a 25% reduction in nitrogen loading at the boundary; in August, eliminating atmospheric input reduces levels in Cape Cod Bay; with input from the boundary only, reductions occurred in Boston Harbor but not in Cape Cod Bay.

Dissolved Oxygen: A 25% reduction in boundary loading produced a small increase in bottom DO in March and October; little change in DO was produced by shutting off atmospheric inputs; eliminating all loads produced the greatest changes in DO.

DOC Flux (August): A 25% reduction in boundary loadings produced a small change in Cape Cod Bay; removing atmospheric inputs had a more pronounced effect in Cape Cod Bay than elsewhere; eliminating all loads except the boundary produced a reduced flux in Boston Harbor and a small change in Cape Cod Bay.

POC: Each of the scenarios produced about a 20-30% reduction.

- Relocating the outfall produced the following results:

a) Nitrogen in surface waters moves offshore; concentrations in Boston Harbor decline.

b) August surface nitrogen declines under primary treatment with the outfall relocated; secondary or "100%" treatment do not produce much difference since the plume traps below the thermocline.

c) Reductions in August bottom nitrogen concentrations in Boston Harbor after 5 years are similar under primary and secondary treatment.

d) Looking at the section from Cohasset east to the boundary, inshore nitrogen concentrations improve under secondary treatment, although nitrogen is somewhat trapped in bottom waters in March; it is unclear why this is so. In August, the plume is trapped offshore in bottom waters, primarily due to density stratification.

- The need to model a conservative tracer at the outfall in the water quality model, as was done for the hydrodynamic model, was discussed. The reason for the plume apparently being trapped during March was also discussed. One possibility is that adding nitrogen to the bottom waters while simultaneously removing it from surface waters gives the appearance of a trapped plume in the difference plots. It also appears that the plume is trapped too low in the water column in August.

- The following conclusions were drawn about the projection runs:

  a) Changes in organic carbon resulting from moving the discharge should not have a significant effect on the benthos (i.e. 500-1000 mg/m^2 maximum over the year is about 125 mg/m^2 above background, and the increase under secondary treatment is only half of that). This reflects an order of magnitude decline from earlier EPA projections.

  b) Worst case dissolved oxygen conditions occur in October; the maximum change should be 1.3 mg/L under primary treatment, about 0.1 mg/L under secondary.

  c) Increases in nitrogen concentrations will be limited to the area around the outfall; there would be no impacts to Cape Cod Bay (an increase of $< 0.1\ \mu g/L$), and definite benefits to Boston Harbor.

OPEN DISCUSSIONS

Comments on the NPDES Permit:

- Mike Connor asked if those present from Mass DEP were satisfied with what they have seen; they indicated that in general they were, but that they would like to see more information about background concentration build-ups over time.

- Since the NPDES permit focusses on the nearfield, is the model useful? The permit assumes that if nearfield conditions are acceptable, farfield conditions will be also. Interest groups concerned about Cape Cod Bay do not necessarily agree; the model should be useful in addressing their concerns. It is implicit that MWRA will eventually go to secondary treatment; it is possible that this may happen before the outfall tunnel is completed.

Comments on the water quality model:

- A concern about the reliability of the DO results was raised; Dom DiToro suggested that a comparison be made between probability distributions of the data and of model results. There's a comfortable margin of error when looking at secondary treatment with the relocated outfall, but this margin decreases when looking at future conditions with no change in treatment level.

• Jay O'Reilly asked that the precision capability of the model be clarified; Jim Fitzpatrick responded by asking if there is really enough data available to do this in the farfield areas. Bob Beardsley suggested that a determination be made about how representative the three years of monitoring data really are; this might put some level of confidence on the model's capabilities.

• Anne Giblin would like to see 1993 bottom SOD data compared to the model; Wendy Leo will provide this information to HydroQual.

• Dom DiToro noted that stratification is what most affects DO concentrations in the bottom of the water column; worst case conditions occur when stratification sets up early in the year and lower in the water column. Low river flows and low winds contribute to this.

• Jim Fitzpatrick suggested that the model be run for hydrographic conditions that represent ± 25% of worst case.

• Jay O'Reilly believes that sensitivity runs on environmental forcings would be useful; Jim Fitzpatrick believes that this could be troublesome since this is really a hydrodynamic issue.

• Don Harleman is concerned that the plume is "over-trapped" in bottom waters; Jim Fitzpatrick will address this concern with a conservative tracer. Eric Adams suggested that this problem may be the result of grid collapse, since 3 x 3 averaging may make the plume disperse sideways, when in reality most of the velocity is in the upward direction.

Comments on final reports and schedules:

• Information on nearfield results should be included in the final draft water quality model report; projection results should also be included.

• Rich Signell expects to write the hydrographic model report in January; this may take the form of a peer-reviewed article, with interpretation about the final model incorporated into it.

• Mike Connor suggested the following schedule:

> a) A full draft final report from HydroQual should be available sometime during the first two weeks of December.
>
> b) MEG members will forward comments to Bob Beardsley within 2-3 weeks following receipt of the report.

c) Comments will be incorporated into a final report to be made available sometime in January.

Highlights of the MEG Discussion

MEG members agreed that the most serious issue is whether the new outfall plume is properly trapped (or not properly trapped) in the water quality model as it is in the hydrodynamic model. The grid collapse could average out local vertical circulation differences and cause effective rise height to be lower. To check this, HydroQual can put a tracer in the water quality model. Running the tracer for just a month may be sufficient to test the plume trapping. The grid may need to be un-collapsed, at least locally. Another less elegant solution would be to inject the effluent into the water quality model at the rise height indicated by the hydrodynamic model.

Maximum use of all available monitoring data is very important. HydroQual may not be able to look at 1993 and 1994 data by the end of the year. However, the model results should at least be compared to frequency distributions of 1992, 1993 and 1994 sediment flux and water column data to see whether variability is similar.

The MEG discussed the context of predictions of "no effect;" it is not clear how the model will be used in management decisions. If before/after differences are of interest, then interannual differences in environmental (weather) forcing probably don't matter (before minus after will not change). Conversely, if one is interested in absolute values, it will be necessary to see whether or not weather varies much from conditions in the 1990 and 1992 calibration periods. Rich Signell will look at this, but will not finish by the end of the year.

The MEG thinks water quality modeling should not stop in December 1994; the HydroQual project should be extended by six months to allow them to complete the report and address MEG issues. If possible the modeling project should be continued (by some organization) and the model kept as a viable tool. The MWRA may want to run the model in the future to analyze anomalous environmental events. Modeling and monitoring should be better melded.

If a model run of secondary treatment in Boston Harbor is done, the suitability of the grid for Boston Harbor predictions should be tested by comparing modeled and observed harbor flushing rates, similar to what was done for the hydrodynamic model.

MEG comments on the report include:

- Add a section or chapter discussing the sensitivity runs presented at the MEG meeting.

- Discuss interannual variability and model accuracy. Include 1993 data where possible, particularly for SOD/bottom water DO.

- List conclusions for the MEG to agree or disagree with.

- Plot dissolved inorganic nitrogen.

- October rather than August is the minimum DO month, so show sensitivity and projection results for October.

MEETING NOTICE

MWRA MODEL EVALUATION GROUP MEETING
ON
MASSACHUSETTS BAY/CAPE COD BAY HYDRODYNAMICS AND
WATER QUALITY MODEL DEVELOPMENT

TUESDAY FEBRUARY 14, 1995
10:00 am - 4:00 pm
FORE RIVER STAGING AREA, QUINCY, MA
NEFCO SLUDGE-TO-FERTILIZER PLANT
LARGE CONFERENCE ROOM

MEETING AGENDA

Introductory Remarks — **M. Connor**

Water Quality Model — **J. Fitzpatrick**

Revisions to grid
Projections with revised grid
Calibration parameters compared to Long Island Sound; sensitivity
Conclusions re: outfall effects

MEG Outputs — **R. Beardsley**

Assessments of MWRA's suggested conclusions
Regulatory agency's needs
MEG letter report on suitability of model
MEG written review of final report

-LUNCH-

Hydrodynamic Model — **R. Signell**

Overview of final report

MEG Executive Session* — **R. Beardsley**

*This is intended to be a focussed discussion among Drs. Beardsley, Adams, Giblin, Harleman, O'Reilly, and Paul about what the next steps are for the MEG. If you want to contribute to the discussion, please talk to Wendy Leo before the meeting (617-251-6501).

#8
(corrected)

MASS BAY/CAPE COD BAY MODEL EVALUATION GROUP (MEG)
February 14, 1995 Meeting Summary
Minutes

Opening Comments - Mike Connor

The (January) draft WQ model report was sent to the Cape Cod Commission for their information; they are interested in having their consultants review the model.

Water Quality Model Review - Jim Fitzpatrick

I. Model/Data (1992) Comparisons (probability plots)

1. <u>Chlorophyll *a*</u>

- Based on probability plots (report Fig. 5-19) the annual modeled variance encompasses field data within the 86th percentile level, indicating a generally good comparison with the data overall; differences tend to occur in the upper 14th percentile.

- However, modeled chl *a* is too high (compared to 1992 data) in the spring and too low in the fall; the summer extremes are not totally reflected as is the timing of the 1992 spring bloom. Nuisance blooms are not dealt with.

- Potential sources of problems identified include:

  - field data are daytime values; model integrates over 24 hours
  - two phytoplankton population types (winter/summer) over-simplifies real conditions, but constraints arise from lack of research knowledge on species kinetics and conditions which affect population dynamics
  - patches may be smaller than grid size; scales "maybe unmatched"; Tow-YO data may help discern this
  - model has inertia which reduces response to extremes; both spatial and temporal response may be "smoothed"
  - *Chl <u>a</u>* not a performance standard per se, but indications of high respiration below the pycnocline and higher deposition are of concern as they affect dissolved oxygen

- Potential adjustments could include matching the spatial scales better, by averaging the field data to the model grid scale. "Adjusting" the model to fit all data points by fiddling within coefficients segment by segment is not advisable, per HydroQual.

2. Dissolved Oxygen

- Based on probability plots (report Fig. 5-20) of bottom DO, annual model matches data reasonably well (up to the 86% percentile), and it matches well in the critical fall period.

- Potential adjustments that could help the model include:

  - the model base extinction coefficient may be too high. The actual 1% light level may be 2 x the value in the model, but this is affected by on-shore/off-shore differences in TSS, phytoplankton, etc.
  - the spring bloom die-off is possibly not picked up in model
  - are the GOM riverine nutrient inputs missed?

- It is unclear why there is a DO reduction in surface after stratification - some ventilation may be occurring due to windstorms or upwelling.

- Nutrient/light interactions (as they affect phytoplankton growth) at/below the pycnocline are recognized as important, since DO limitations are more of a potential issue there. Therefore, phytoplankton growth/suppression needs to be examined carefully in the model with respect to the above parameters, as it relates to the new outfall location for representative grid locations.

II. Grid Collapse Correction

Finer (i.e., hydrodynamic grid) around outfall resolved the vertical mixing problem in the WQ model; the mixing (coefficient), was not changed only the number of grids (with concommittant flow data) were expanded to match the hydrodynamic model. There was still some plume trapping in August, but this was consistent with the hydrodynamic model

III. 1992 Temporal Calibration Results

- 12 Month projection and seasonal (Apr., Jun., Aug., Oct.) vertical plots were presented for key parameters at grid cells where there were field data, near the new outfall (grid 11, 18) and in Cape Cod Bay (grid 6,4)

- 12 Month plots showed reasonably good agreement, with the exception that some of the peaks in the field data were not projected. The vertical plots also showed good agreement for most parameters.

- A hard copy of the plots should be provided.

IV. Comparison of current outfall with future outfall, primary vs secondary treatment - color plots

- DIN: At the future outfall there might be 1-2 months where it is higher at the primary treatment level

  <u>Surface</u>: In Boston Harbor, DIN stays higher in the spring; in the summer, it is gone
  <u>Bottom</u>: A "Bloom" occurs in Cape Cod Bay first; but not in deeper waters

- Chl *a*: The projected bloom starts in Cape Cod Bay, with little difference between treatments. There is a projected ~ 5mg/l decrease in Boston Harbor with the new outfall; this reduction (DIN and Chl *a*) is the biggest benefit to the Harbor; there is some reduction in lower Cape Cod Bay as well. Some increase in DIN in the spring are projected at the new outfall, but only small differences are projected in summer and fall. However, light levels in the Harbor are not changed with secondary treatment; how much light penetrations results due to reduced TSS and/or Chl*a*, is not known.

- Dissolved Oxygen: With the new outfall there is an improvement in bottom DO in Boston Harbor while it is somewhat lower at the new location, and essentially unchanged in Cape Cod Bay. While lowest DOs are in August, they are projected as being lowest in October offshore. There is a 0.4 - 0.5 mg/l DO projected improvement with secondary treatment; the minimum (15 min.) value projected in bottom DO is 6.6 mg/l. DO levels in inner Boston Harbor (i.e., Chelsea River) is projected to improve a little (min. 4.63 mg/l) with the future outfall (with secondary) but CSOs, Charles River and other inputs are still there - why the improvements are not better is not entirely explained.

- Chlorophyll *a*: There is a 30-50% Reduction in surface Chl *a* projected in most areas with the new outfall (with secondary); there is a good reduction even with primary. Generally there is a inverse correlation between DO and Chl *a*.

- POC FLUX: An increase in the maximum level is projected at the new outfall - 12% (to 207 $mg/m^2/d$). Decreases on the order of 50% are projected in Dorchester Bay: to 402 $mg/m^2/d$ at the max. level and 207 $mg/m^2/d$ on an average annual basis. Overall relative changes in parameters appear fine for comparison of operating scenarios, but the absolute value may be off somewhat (e.g., 0.1 to 0.3 ppm DO).

V. Further discussion

1. Light extinction

- The suggestion was to put in the model an on-offshore gradient with seasonal fluctuation; (e.g., change coefficient in the model to 1.5 m^{-1} or a value worked out from empirical data).

- Concerns were expressed about identifying the correct level of light and amount of phytoplankton growth that can occur at pycnocline depths. It may be worth looking at 1992-93 data to examine Chl *a* max. and light levels.

- Look at C. Yensch's paper on spatial changes in extinction coefficients.

2. DO

- Look at DO minimums from year to year by comparing them on probability plots.

3. Calibrations/Annual Variations

- The coefficients were optimized with 1990 and 1992 data; a verification with 1993 data could be done, but it is not clear that there is time for this effort presently. Year to year variability including 1993 water column and sediment flux data should be checked (probability plots).

- Will more data be useful? It would show greater variability because of ye to year differences. An examination of cumulative (all data) probability plots, spatially separated (nearfield, farfield) could help. A comparison of 1993 and 1994 data with 1992 model run and data could be done to look at variability issue. However the 1993 or 1994 data can't be integrated into the calibration because of contract/deadline issues.

- HydroQual could show plume dynamics with physical data to show forcing funtion effects on biological/WQ data.

4. Phytoplankton - A look at seasonal/spatial differences in winter/summer groups to understand what's driving Chl *a* levels and what seasonal/spatial resolution there is was requested.

5. Pathogens - These could be added to the Hydrodynamic model with dilution projects (with die-off rate) to get a general picture.

VI. Additional needs

- produce cumulative frequency plots of DO, Chl *a* for surface and subsurface (Pycnocline)
- adjust the extinction coefficient as discussed; re-run model if necessary
- Show 1993 sediment data
- Report
  - Add chapter on February MEG recommendations
  - Add conclusions, with revisions
  - Look at why inner Boston Harbor doesn't improve more with offshore outfall
  - can treatment vs dilution affects be quantified?

Hydrodynamic model final overview - Rich Signell

An overview of model development and corrections made to improve model was presented, including:

- corrections to horizontal and vertical mixing and matching (grid size) to appropriate spatial data for model comparison (i.e., low frequency elipses)
- Sigma - coordinate fix ~~correct heat flux parameterization~~

* Correction to heat flux parameterization.

- nearfield grid size influences and nearfield modeling analysis by A. Blumberg.

- Windstress by season (stick plots) to show spatial differences

- Flushing time estimates which showed higher values in the summer (unclear why) and although at both site it is rapid, flushing at future outfall is projected to be lower than at current location.

In final report, would like to summarize what has been learned from model; also want to expand hydrodynamic - related conclusions in WQ report.

#8

M E G MEETING

February 14, 1995

Action Items/Recommendations

For Water Quality Model/Report

1. Examine the base extinction coefficients in the model and compare them with seasonal/spatial (on-offshore, including in the Harbor) variations from field data. If they are different enough, model may need to be re-run. Mike Mickelson will compile field data.

2. Check the variability of data more closely by matching specific field data location(s) with corresponding grid cell(s) in model. If the comparison shows that data-model variability are reduced, variances in model are likely due to small scale patchiness which the model doesn't reproduce (this may or may not matter).

3. Examine certain dissolved oxygen questions:

   3a) Check to see if minimum DO is always in deepest segment.

   3b) Is DO pattern similar to that of temperature (plot minimum percent saturation)

   3c) What causes the low DO at the mouth of the Charles River; i.e., why doesn't it improve when the outfall is moved offshore? It could be BOD from rivers, CSOs, or stormwater; a less likely possibility is the lack of sediment, COD, SOD equilibrium in five year's time. Wendy Leo will look at BOD loads in Harbor and determine their reasonableness for present and future conditions.

4. Make some figures which focus on Boston Harbor and Western Mass. Bay (out to the new outfall area).

5. For phytoplankton/chlorophyll *a*:

   5a) Plot results along a transect from Boston Harbor, to try to get at how the export of Boston Harbor water (forced by wind or whatever) affects the interseasonal variability in chlorophyll.

5b) See if any of the scenarios change the relative importance of the two functional phytoplankton groups. Perhaps show the ratio of the functional groups during summer at a few depths and locations, or monthly stacked bar charts of the winter and summer group, or an animated map of a summer group portion.

5c) To compare projections with respect to chlorophyll, calculate areal average chlorophyll down to the 1% light level (rather than just surface chlorophyll calculations).

6. Demonstrate that the (new) grid resolution is sufficient for Boston Harbor by calculating the residence time for Boston Harbor with both the "full hydrodynamic" and the "new collapsed" grid. Rich Signell has (we think) already demonstrated that the hydrodynamic model gives the correct residence time.

7. Put the 1993 sediment flux data on the sediment flux plot.

8. Calibrations and interannual variation:

8a) Clarify the calibration procedure in the report; initial calibration was done with the 1990 data then recalibrated (with minor adjustments) to the larger 1992 data set, then 1990 was re-run as a check.

8b) Look at interannual variability in the model using the whole model run (10/89-12/92). (Rich S. notes that the main hydrodynamic difference observed at B buoy in 1989-94 data set is the timing and size of the spring freshet; there is little statistical difference).

9. Put all the field data from 1993 and 1994 on the calibration and probability plots. It is legitimate to "lump" data across years in order to see if the model reproduces the broad seasonal features. Look for observed features that the model "misses" in all years: for example, did 1993 have the same rebound in DO in June as 1992 did? If 1993 or 1994 are very different from 1992, may need to do 1993 or 1994 model run -- but put this off for now.

10. On the probability plots, instead of model minimum and maximum values, plot probability bands, and compare them to probability bands of the data (this request may need to be clarified).

11. Determine how different the 1990-92 environmental forcing was from the following proposed "worst case": Stratification sets up as early as ever observed (viz. 1994), and is prolonged as long as ever observed; quiescent currents; high incident radiation through September (?) and low incident radiation thereafter. (Should we use a low (1990) or high (1993) runoff year? Does it matter?) Use the B buoy data and the old lightship data to find the worst case stratification [get Rich Signell to help].

    11a) If the difference is very great, adjust the 1992 forcing and re-run the model. Run both "calibration" and projection, and see if the relative difference is the same.

    11b) Jack Kelly will extrapolate the observed respiration rates to see if delayed fall turnover would cause DO problems.

12. Was the time of fall turnover adjusted in the water quality model relative to the hydrodynamic model?

13. Compare the model predictions to the 301(h) waiver conclusions [Wendy will send them to Jim].

14. Produce a revised draft. This may include new runs, depending on the answer to the light question (2) above. Send it to the MEG, and to MWRA, Normandeau and USGS.

    14a) Discuss in the report what we have learned about Mass. Bay from the model, maybe a chapter on "how the system works".

    14b) Rewrite "proposed conclusions" based on MEG comments and any new insight; caveat all of them "assuming that environmental forcing is the same as in 1990-92" [Wendy will draft some new ones]. Add caveat about toxics and pathogens. Bring in comparison to 301(h) waiver conclusions and mention conclusions based on best possible effort with two years of data for calibration.

For Hydrodynamic Model/Report (R. Signell)

1. Remember to add to the report that the sigma-coördinate fix and the initial dilution comes out approximately right; ref. Alan Blumberg paper.

2. Help figure out what the stratification and currents "worst case" observations were for the WQ model (see item #11 above)

3. Compare model and data spectra

4. Include the report the analysis (already done?) that the grid resolution is sufficient for Boston Harbor because the model gives the correct residence time.

5. Review previous models in report; discuss how is ECOMsi different?

6. Discuss in the report what we have learned about Mass. Bay from the model.

7. Send a draft report to Wendy Leo to distribute to MEG.

8. Possibly model the effects of a chlorination failure on bacteria counts under various scenarios, assuming first-order decay.

MEG ACTIONS

MEG members will send comments to jimf@hydroqual.com and cc: meg@hestia.whoi.edu

Model evaluation group meeting
2/14/95
Attendance List

Eric Adams, MIT
Robert Beardsley, WHOI
Jim Bowen, ENSR
Michael Connor, MWRA
Jim Fitzpatrick, HydroQual
Anne Giblin, MBL
Donald Harleman, MIT
Jack Kelly, Battelle
Brian Kubaska, MWRA
Wendy Leo, MWRA
Michael Marsh, EPA
Michael Mickelson, MWRA
Jay O'Reilly, NOAA/NMFS
John Paul, EPA
Judy Pederson, MIT Sea Grant
John Shipman, Normandeau
Richard Signell, USGS
Nick Yannoni, MWRA

Agenda - MEG meeting #9

May 25, 1995
10:00 - 4:00

NEFCO Sludge-to-Fertilizer Plant
Large Conference Room
Fore River Staging Area
Quincy, MA

Introductory remarks — **M. Connor**

Water quality model — **J. Fitzpatrick**

1) revisions to light extinction
2) calibration results using new light extinction coefficients
3) comparison of 1992 to 1993 data
4) comparison of 1990 to 1992 model computations
5) analysis of some aspects of model variability

- LUNCH -

6) projection results using revised extinction coefficients
7) conclusions re: effects of outfall relocation and secondary treatment

MEG Outputs — **R. Beardsley**

assessment of MWRA's suggested conclusions
regulatory agencies' needs
MEG letter report on suitability of model
MEG written review of final report

MEG Executive Session — **R. Beardsley**

MEETING MINUTES: MEG MEETING #9
MAY 25, 1995
FORE RIVER STAGING AREA, QUINCY, MA

Introductory Remarks - Mike Connor

• The EPA permit is due out this summer, so a MEG report in the summer would be timely.

• Review of the project and the final report by the MEG would be very timely; Bob Beardsley thinks that the MEG could draft its report by June 30th. Jim Fitzpatrick considers the report to be essentially complete, with the exception of adding the final conclusions.

• Where should the model be "kept," and to whom should it be distributed? MIT and UMASS Boston are suggested locations. John Paul recommends against wide distribution of model code. HydroQual is concerned that they will be asked in the future to support model code. Since the MWRA has an ongoing relationship with USGS, they might be the appropriate "keepers."

• The hydrodynamic model code and input files, and the water quality model and input files require more than one gigabyte of memory.

• Discussions turned to the Ray Canale report, which provided a somewhat controversial and critical review of the model. The MEG had raised some similar issues on the January draft report, and HydroQual's new draft has responded to these issues.

• Does the report need to be made more understandable to the layman? Some details that were available to the MEG could be included, for example: include a table showing how coefficients were derived from other systems; explain by reference to Alan Blumberg's report that the hydrodynamic model adequately represents plume dynamics; and note that new data collected at the boundary corroborate earlier assumptions (J. Kelly will send draft report to the MEG).

• Adding a disk or possibly a CD-ROM that includes the model animations would be helpful for managers interested in using or reviewing the model; possible items to include might be surface, mid-depth and bottom contours of DO, DIN, chlorophyll and POC flux. It should be possible to stop the graphics at any point during a simulation. HydroQual will look into this.

• For graphics in general, limit each page or screen to two panels, with consistent format and scale on each panel.

• Data to be included with model: MWRA observational data and model inputs (rain, runoff, etc.). These data could possibly be included on CD-ROM along with the model. HydroQual can copy tape archives.

Water Quality Model Updates - Jim Fitzpatrick

New Light Extinction Coefficient

• The coefficient varies with water depth; Ke_{base} ≈ 0.2 m-1 in the Bay. Stations near Boston Harbor had a different relationship, with a higher Ke_{base}. The Boston Harbor Ke_{base} ≈ 0.6 m-1 based on historical data (there was no outfall monitoring data for the Harbor). If it is assumed that MWRA solids are 50% of total solids loading, Ke_{base} ≈ 0.4 m-1, but model results are not sensitive to this. The two runs (at 0.6 and 0.4) probably bracket the actual values.

• HydroQual needs to check that the text and figures in other sections of the report are consistent with changes made to extinction coefficients, e.g., page 5-16 needs to be updated to make it clear that this incorporates the new Ke_{base} determination.

• As expected, increasing light penetration reduces DIN at depth and increases chlorophyll at depth. Increased light penetration can sometimes reduce pycnocline chlorophyll because the algae there compete with sub-pycnocline algae for nutrients.

• Overall, chlorophyll increased by ~ 15%, and model calibration improved slightly.

• Primary productivity is still underestimated in the nearfield area. Although productivity is higher in the other years, as shown in Figure 5-24, the match of the 1992 model to 1992 data is not as bad.

• The lower Ke helped the model reproduce the occasional summer inversions of DO (i.e., bottom DO > surface DO).

• The probability plots showed that with the new Ke, there is a better prediction of mid-depth chlorophyll. The model always seems to under predict the fall bloom. John Paul asked if the model under predicts the DO depression, but Jack Kelly noted that there is not a clear relationship between fall bloom size and DO depression. Jay O'Reilly noted that this occurs because the fall bloom is confined to the surface, while the spring bloom occurs throughout the water column.

Comparison of 1992 Model Results and 1992 and 1993 Data

• Dom DiToro brought up Figure 5-32 to try to explain the high May denitrification value at Station 1 in Figure 5-34, which he does not think is a realistic value. The differences between model predictions and data at Station 1 are a concern to everyone in the group, but everyone is happy with the farfield match.

• Some of the figures shown (e.g., Figure 3-26) highlight the variability in the field data, which the model cannot reproduce. A lot of the variability in the model is caused by the variable hydrodynamics in Boston Harbor.

• Jim Fitzpatrick noted that the water quality model will "do as well" as the hydrodynamic model. The MEG requested that the modelers check that both the water quality and hydrodynamic models are correctly representing Harbor flushing. Eric Adams provided some ideas on calculating residence times based on salinity, and will provide Jim with a table of Harbor residence times from the literature.

• A question was raised about Chapter 6 of the report; sensitivity analyses were done with the "old" model. Should this be kept in? Plume dynamics are not applicable to this, but changes in light limitation could have minor effects on primary productivity. Sensitivity to Si is not addressed, even though Cape Cod Bay is Si limited. However, there's no anticipated outfall effect there. It was concluded that the chapter should stay, but should be appropriately caveated.

Projections

• The assumed DIN under secondary treatment is sometimes a little higher than the actual DIN under primary treatment. It should be made clear in the report that "splits" into forms of N, P and C were changed between primary and secondary treatment. It should be made clear in Table 7-1 and in the figures that the same primary and secondary treatment loads were used for current and future outfall scenarios.

• The existing (primary) phosphorus load is probably too high, giving an apparent secondary phosphorus removal as high as 50%. However, the model results are not sensitive to phosphorus. Nutrient loads should be double checked, especially phosphorus. The existing conditions scenario is based on 1988 data and the secondary treatment scenario is based on 1994 data. A 30% nitrogen removal may also be too high.

• After the outfall is moved, the sediment (G2) carbon burns off in about three years. Boston Harbor still has lower DO and higher chlorophyll due to CSOs, riverine input and restricted flushing.

• Secondary treatment reduces POC flux at the future outfall, to below 500 mg C m^{-2} d^{-1}; POC flux declines at the existing outfall also, below this level of possible concern.

• Some more comments on figure structure were made, concerning resolution of contours in some of the figures in Chapter 7; either the contouring protocol needs to be improved - some software that Jay O'Reilly has may help - or the contours need to be numbered.

• The point that the most recent data from the boundary region support earlier assumptions based on 1990-1992 data was made again. It was also reiterated that a table giving the history of the coefficient development needs to be made for the final report. The important thing is, however, that the model is stable, so it does not matter that there are so many coefficients and variables. It would also be valuable to provide a history of the calibration efforts (e.g., Jim's log of the "100s" of runs).

Conclusions: MWRA's and MEG's

• At the center of this discussion was a list of eleven draft conclusions prepared prior to the meeting.

• Should these be MWRA and HydroQual's conclusions or the MEG's? Does the MEG have any objections to conclusions drawn by MWRA?

• The MEG and MWRA should prepare their own separate conclusions; it is not the task of the MEG to restate model results or agree/disagree with model predictions, especially since the MEG has been involved in the process all along. The MEG's role in this process has been as catalyst and advisor. The MEG's task should be to state whether or not the model can serve the purpose for which it was developed - to make predictions about future water quality conditions after the outfall is moved and after treatment levels are upgraded.

With this in mind, the conclusions could go as follows:

MWRA:

"In all of the projection scenarios that were conducted, no adverse changes from present conditions (calibration simulation) were predicted for Cape Cod Bay.

There were no substantial changes in Cape Cod Bay concentrations of dissolved oxygen , DIN, DIP, or chlorophyll, or in POC flux. Sensitivity analyses indicate that Cape Cod Bay and most of Massachusetts Bay are relatively unaffected by changes in wastewater inputs into or near Boston Harbor."

"The water quality in Boston Harbor will greatly improve with any of the projection scenarios. The dissolved oxygen standard should generally be met in the Harbor with secondary treatment plus relocation of the outfall, although there may remain localized problems not resolved by this model."

"The effect of the new outfall on water quality is predicted to be limited to the nearfield. Effects include improved bottom dissolved oxygen, and an increase in POC flux to the sediment; the latter remains well below the deleterious effects levels."

"The combination of secondary treatment and new outfall location meets all water quality standards with respect to oxygen and benthic enrichment (there is no standard for the latter). Therefore it is unlikely that further treatment will be necessary. Specifically, the dissolved oxygen standards of 6 mg/L in the Bays and 5 mg/L in Boston Harbor are predicted to be met, and the POC flux is well below the 301(h) level of concern of 1.5 $g/m^2/day$."

"Algal production is limited by nitrogen and/or light, not by phosphorus."

MEG:

"The hydrodynamic model represents the state-of-the-practice in reproducing the time-dependent, three-dimensional stratification and circulation in Massachusetts and Cape Cod Bays. As with all model applications of this caliber, the major uncertainties relate to the inability to exactly specify the forcing and boundary conditions for the model simulations."

"The water quality model represents the best attempt at being able to model the principle processes of primary productivity in the Bays. The model represents the state-of-the-practice in applying time-dependent, multi-dimensional water quality models to primary productivity in estuarine and coastal systems. Because these models are not true depictions of what occurs in natural systems and because there are limitations in the data sets that are available, we have to expect that the model results will not completely reproduce the data. However, the model results do reproduce the major processes that occur in the Bays."

"The hydrodynamic model appears to reproduce the plume dynamics when comparisons are made against state-of-the-practice plume models. The plume dynamics produced by the hydrodynamic model are specified directly in the water quality model, at the same spatial scale as the hydrodynamics."

"The validity of a model can be judged to some extent from the degree that it reproduces the observations to which it is calibrated since it is not always possible to reproduce spatially variable magnitudes with constant parameters. A model which can reproduce observations in different estuaries with essentially similar structure and coefficients has a higher level of credibility since this indicates that it can properly scale between these quite different locations. The Massachusetts Bay model has been successfully applied to Chesapeake Bay, Long Island Sound, and currently New York Harbor with essentially no changes in either model structure or model coefficients. The degree of calibration for these cases is similar to that achieved in Massachusetts Bay so that the model has demonstrated a general level of validity. Therefore the model can be relied on to a much greater extent when making projections. It is this feature of the Massachusetts Bay model upon which we base our opinion that the model projections can be relied on as the best available estimates."

"Model results should be compared to monitoring data on an ongoing basis."

• Don Harleman expressed concern over the conclusion that the combination of secondary treatment and the new outfall location would be sufficient, and additional treatment would not be needed in the future. He suggested that a nitrogen-removal scenario be run; the sensitivity analysis may not be sufficient. Similarly, nitrogen loads in primary or secondary effluent are uncertain. Mike Connor noted, however, that the values used in the model are reasonable, given the most recent actual data available for the current MWRA effluent.

Several items that need to be added to the report or expanded were discussed:

1. Loading

2. Flushing

3. This model vs. history and other models; additional calibration results (possibly in a separate bound appendix, and including the time series plots).

4. Boundary conditions; data added to figures, plus the 1994 data collected by Battelle; Anne Giblin's nutrient flux report.

5. Plume behavior in the hydrodynamic model; include Alan Blumberg's report as a separate bound appendix; refer to this in Section 7.2, and note that the original grid is acceptable for calibration.

6. Make it clear that the model does not generally deal with "events," but can deal with seasons. For example, the DO drop in spring is not reproduced by the model; some events are, however, captured. List the features that are not reproduced (sometimes because the hydrodynamic model does not capture all events). Page 5-50 of the current draft includes "understanding of how the system works;" these bulleted summaries are helpful, but they should be moved to the actual report summary. With respect to "weather vs. climate:" are the extremes (e.g., lowest temperature, lowest chlorophyll) captured by the model? The model does capture the minimum DO. Are the high chlorophyll values that are missed by the model high enough to be a concern? Temporal performance (events) may be mismatched to vertical gradients (e.g., inversions of DO) because the model does not capture enough chlorophyll "events" of short duration. What kinds of environmental scenarios are suitable for prediction with the model?

7. Tertiary treatment run, with total N set at 5ppm.

8. Bacterial simulation run (to be completed by Rich Signell).

9. Resolution of the gray scale problem.

10. CD-ROM vs. color plates.

11. Put satellite comparison figure in the report (color or gray?).

Summary of MEG Executive Session

• The MEG decided to invite Jack Kelly to be an official member of the MEG, as his participation in MEG discussions to date has provided helpful perspective.

• The MEG discussed its report on the modeling projects, which will be in the form of a report from the MEG to Mike Connor. The following outline was discussed:

Introduction: History of MEG, original charge to MEG, membership, the review process, and objectives of the MEG report.

Assessment: Hydrodynamic and water quality models. Eric Adams suggested this be organized following the format discussed in a paper in *J. Hydraulic Engineering* on evaluating model performance. The steps included would be something along the following lines:

1) problem statement
2) process identification
3) choice of model and modification for Mass Bay
4) model response studies (sensitivity)
5) calibration (strengths and weaknesses)
6) verification

Recommendations

Appendix: membership, minutes of meetings

• The MEG then discussed the "assessment" section for both hydrodynamic and water quality models, and tentative recommendations. The MEG plans to meet in early July to discuss its report.

Attendees of Model Evaluation Group Meeting #9

May 5, 1995

Eric Adams, MIT
Robert Beardsley, WHOI
Jim Bowen, ENSR
Anne Canaday, MWRA
Holly Carson, Normandeau
Michael Connor, MWRA
Dom DiToro, HydroQual
Jim Fitzpatrick, HydroQual
Don Harleman, MIT
Russ Isaac, MADEP
Jack Kelly, Battelle
Wendy Leo, MWRA
Michael Marsh, EPA
Michael Mickelson, MWRA
Jay O'Reilly, NOAA/NMFS
John Paul, EPA
Nick Yonnoni, MWRA
Ling Tang, MIT

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MWRA Model Evaluation Group Meeting

Minutes

The Model Evaluation Group (MEG) met on Friday, September 15, 1995 to complete its report to the MWRA.

In attendance were:

MEG Members:

Bob Beardsley	John Paul
Eric Adams	Don Harleman
Anne Giblin	Jay O'Reilly
Jack Kelly	

Also in Attendance were:

Mike Mickelson	Dave Taylor
Wendy Leo	Jim Bowen
Mike Connor	Jennifer Chiapella

- Wendy Leo handed out meeting minutes from the previous MEG meeting and the Department of Environmental Protection's comments on the water quality model report.
- Bob Beardsley handed out the latest MEG report draft and Rich Signell's latest draft of his report.
- At Bob Beardsley's suggestion, the reports were read by all and then discussed.
- Discussion on the structure and content of the report included the suggestion that an introductory statement of what would be found in the report be added. Also discussed was the addition of a conclusion recommending that the model be used on an ongoing basis by making it available through the local academic community.
- The MEG group members discussed sections requiring further detail or clarification and were then assigned report sections to revise and finalize. Finalized sections were handed in to Bob Beardsley and Eric Adams to update electronically.

ENSR

- The meeting concluded with the understanding that all sections were essentially complete and that final comments would be exchanged by email for incorporation in the final version.