Outfall Monitoring Science Advisory Panel (OMSAP) Meeting

Wednesday, June 21, 2000, 10:00 AM - 2:00 PM
Boston, MA
FINAL MINUTES

ATTENDANCE

Members Present: Andy Solow, WHOI (chair); Robert Beardsley, WHOI; Bob Kenney, URI; Judy Pederson, MIT/Sea Grant; and Jim Shine, Harvard School of Public Health.

Observers: Eric Adams, MIT; Margaret Callanan, Cape Cod Commission; Cathy Coniaris, OMSAP staff; Jim Fitzpatrick, HydroQual; Carlton Hunt, Battelle; Russell Isaac, MADEP; Ken Keay, MWRA; Christian Krahforst, MCZM; Wendy Leo, MWRA; Mike Mickelson, MWRA; Andrea Rex, MWRA; Larry Schafer, observer; Jack Schwartz, MADMF; Adam Storeygard, MWRA; Heather Trulli, Battelle; Sal Testaverde, NMFS; Dave Tomey, EPA; Steve Tucker, Cape Cod Commission/Bays Legal Fund.

SUMMARY OF ACTION ITEMS & RECOMMENDATIONS

1. OMSAP approved the minutes of the March 28, 2000 meeting as amended.
2. OMSAP approved recommendations of the Bays Eutrophication Model Evaluation Group and will forward their final report to EPA and MADEP.
3. OMSAP approved their draft letter to EPA and MADEP outlining their recent recommendations on MWRA's food web model scope of work, zooplankton, Alexandrium, and floatables thresholsds.
4. OMSAP agreed to recommend three revisions to the benthic thresholds: 1) stations considered in the threshold calculations should include the nearfield stations plus the three adjacent western Mass Bay stations; 2) the benthic threshold test for annual mean species diversity should not fall outside the central 95th percentile of the baseline data; and 3) the existing pollution opportunist threshold should be tightened to make it more environmentally protective (caution from 25% to 10% and warning from 50% to 25%).

MARCH 2000 MINUTES

J. Fitzpatrick corrected a statement in the March minutes in that the Army Corps of Engineers developed the Chesapeake Bay model and HydroQual was involved only in developing a sediment nutrient flux model with them. C. Coniaris offered to add that information in brackets. **ACTION:** OMSAP members approved the draft March 28, 2000 OMSAP minutes as amended, including the recommended revisions by Don Anderson and Jack Schwartz.

MWRA CONSTRUCTION UPDATE/1999 DISSOLVED OXYGEN (DO) & CHLOROPHYLL

M. Mickelson estimated that secondary battery C will be operational in August 2000 and the new

outfall diffuser system is scheduled to go on-line in the fall of 2000 (as early as mid-September). Presently the outfall safety plugs need to be removed. Workers had only managed to remove three safety plugs before the tragic accident that killed two workers occurred in July 1999. One plan for bringing the outfall on-line involves attaching a jack-up barge to the third riser cap using a specially built sleeve so that the tunnel can be ventilated.

M. Mickelson then described 1999 monitoring results for nearfield dissolved oxygen and chlorophyll. In 1999, there were two occasions where mean DO concentrations fell below the 6.5 mg/L caution threshold. Rocky Geyer compared the dissolved oxygen minima for each year with salinity and temperature and found that there is a correlation. He has developed a model that examines this dissolved oxygen pattern and it appears that low DO is favored by warm and more saline water.

J. Shine asked if this is an empirical observation. A. Solow replied that it is a regression model. L. Schafer asked if the anomalies are due to weather. M. Mickelson replied that the high salinity in 1999 was due to periods of drought, calmness, low river runoff, and lack of storms to stir up the bottom waters but in 1999, the salinity was higher. He added that the bottom water temperatures are largely set up by the pre-stratification winter temperature. R. Isaac thought that bottom water coming in from the boundary would be consistently cool. C. Hunt thinks that is one of the things that R. Geyer will examine next. He added that the Gulf of Maine is not deep enough to have bottom waters with a constant temperature.

J. Shine asked if R. Geyer used the data to make the model, and then used the same data to test the model. M. Mickelson replied that he used the data once, tabulated the minimum salinity, temperature, and oxygen, and then calculated the anomaly. B. Beardsley added that he is using the average near bottom dissolved oxygen for two months and the temperature/salinity variation. The hypothesis is that the more stratification, the lower the DO, and stratification can depend on both salinity and temperature. A. Solow clarified J. Shine's point in that it is not surprising that the two-parameter model fit the eight points of data, however, it may not predict future conditions as well.

M. Mickelson showed nearfield chlorophyll data for each of the baseline years of monitoring, from January to December. The seasonal threshold is the 95th percentile of the seasonal mean and the annual threshold is set at twice the baseline. If the thresholds are calculated based on only these earlier years, 1999 would have exceeded the winter/spring, fall, and annual thresholds.

M. Mickelson then compared chlorophyll and Acartia data. Current knowledge of Acartia zoology indicates that the nauplii prefer lower salinity water. In 1999, when the chlorophyll and salinity were high, Acartia was found in low abundances. This supports the notion that salinity controls Acartia abundances more than nutrient levels. J. Fitzpatrick added that there are years with high Acartia and moderately high salinities so it may be a combination of factors. M. Mickelson thinks it could also be possible that the nauplii are farther "upstream", in the inner harbor. MWRA should look at New England Aquarium and MWRA harbor monitoring data for inner harbor salinity data.

REVIEW OF THE MODEL EVALUATION GROUP (MEG) REPORT

B. Beardsley presented a summary of the draft MEG report. OMSAP tasked the Bays Eutrophication Model (BEM) Evaluation Group with reviewing the HydroQual report "Bays Eutrophication Modeling Analysis for the Period 1992 to 1994" and recommending changes to either the model or the monitoring program to improve the model's skill for water quality predictions. The MEG consisted of himself, Jeff Cornwell (U. Maryland), Don Harleman (MIT), Eric Adams (MIT), Jack Kelly (EPA, Duluth MN), John Paul (EPA, Narragansett, RI), and Jay O'Reilly (NMFS, Narragansett, RI). The MEG had an open meeting in Woods Hole in December 1999 to review the model results for 1992-4. In 1995, an earlier Model Evaluation Group requested that the model be run for 1993 and 1994 since these years included interesting features in the monitoring data including an intense fall bloom in 1993 and low dissolved oxygen in 1994.

B. Beardsley stated that in general, the MEG was pleased that the additional model runs had been done. The group felt that the simulations through 1994 provided additional experience with the model, for a variety of conditions, and further tested its ability to capture both seasonal and episodic events. The model successfully reproduced the timing and spatial pattern of the winter/spring bloom, the limiting roles of silica in Cape Cod Bay during winter and spring, inorganic nitrogen concentrations in Massachusetts Bay during the summer, the seasonal variation of dissolved oxygen, and several other phenomena. The group did note that the model did not capture the high diatom concentrations observed during the fall of 1993 and it did not completely capture the low dissolved oxygen observed in late 1994. In 1994, the dissolved oxygen prediction was lower than in previous years, but did not get as low as what was actually observed. The model also could not reproduce the large dynamic range for phytoplankton concentrations and this raises questions about how well the model can distinguish changes due to the outfall and from natural variability. Much of the mismatch between the model and observations may be associated with unresolved variability.

1. B. Beardsley then summarized the MEG's seven general recommendations: Add a third algae component to BEM for direct simulation of the fall diatom bloom (this may strongly impact the fall and spring bottom water DO).
2. Match the BEM grid to the hydrodynamic model within the BEM domain to eliminate questions of grid collapsing.
3. Make "projection" runs (i.e., model runs comparing conditions with existing versus future outfall locations, and with primary versus secondary treatment) to help assess the relative impacts of anthropogenic change versus ambient variability.
4. The nutrient flux around Cape Ann is poorly sampled by the present monitoring program, so that changes within the Mass Bays system due to real changes in the upstream boundary conditions will be missed in model simulations.
   MEG recommends three actions:
   a) Determine the sensitivity of the 1992-94 BEM runs to realistic changes in the upstream boundary conditions.
   b) Develop a plan to begin collecting measurements of currents and water properties along the upstream section of the model open boundary.
   c) Investigate recent efforts to develop a Gulf of Maine Ocean Observing System

5. The closing of the Nut Island treatment plant (NITP) in 1998 and switch to secondary treatment at Deer Island made a significant change in the distribution and forms of nutrient input to Boston Harbor (data analysis by D. Harleman). MEG recommends that the BEM be used to simulate the 1998-1999 period to see how well the model captures the increased chlorophyll observed near the Boston Harbor/Mass Bay boundary.
6. Conduct BEM mass balance studies to determine the relative importance of outfall versus open boundary sources of ammonium (NH3)within the Mass Bays system.
7. Before the addition of a benthic diatom state variable, BEM results and field data should be analyzed for the predicted/observed light levels at the sediment-water interface. The predicted areal pattern of light could be used to assist in deciding whether the inclusion of this potential source of oxygen to the model is warranted. If it is, the experimental benthic flux work should include an illuminated treatment.

J. Pederson suggested that folks let OMSAP know if anyone has specific recommendations for the GoMOOS group. B. Beardsley agreed. GoMOOS is funded and he thinks it will also be a vehicle for other activities. M. Mickelson asked how long GoMOOS is funded for. B. Beardsley believes two years. C. Hunt asked if GoMOOS will study deep circulation. B. Beardsley did not think so. One objective of GoMOOS is to look at the influx of freshwater into the system. However, there are other moorings in the Gulf of Maine that study deep ocean circulation.

B. Beardsley then described the data analysis by Don Harleman. He obtained data from MWRA to examine the effects of the closing of the NITP and the start of secondary treatment. It appears that secondary treatment increased the percentage of ammonium in the treated effluent. D. Harleman showed that about half of this increase is due to secondary treatment, and the other half is due to the increase in the input (from the transfer of the NITP flows). D. Harleman believes that this may have caused the rise in chlorophyll near the Boston Harbor/Mass Bay boundary.

J. Fitzpatrick thinks that this could also be due to better solids removal, increasing the light fields in Boston Harbor, and stimulating additional growth and higher chlorophyll measurements. J. Shine pointed out that ammonium is more immediately available to phytoplankton than organic nitrogen. W. Leo noted that this increase in chlorophyll is not seen inside the harbor between Deer Island and Hull, but outside the harbor, in western Mass Bay near President Roads. M. Mickelson suggested that the figure presented showing D. Harleman's calculations include information for Nut Island and also take flows into account to calculate loads. C. Hunt noted that the total loading into the system is not changing, it is simply the loading from one specific outfall.

1. B. Beardsley then listed MEG's recommendations for BEM report addenda: Complete documentation of the circulation and BEM parameters used in the 1992-1994 simulation.
2. Complete documentation of the boundary conditions used in the 1992-1994 simulation.

3. A comparison of simultaneous temperature, salinity, and density from the BEM and circulation model should be made for the 1992-1994 period and presented with a discussion about the skill of the BEM to capture the vertical stratification seen in the hydrodynamic model.
4. The experimental uncertainties inherent in the field data due to instrumental and methodological [replace “problems” with “errors”, see below] and the spatial and temporal scales of natural variability should be estimated as best as possible and shown in all model/data comparison figures.
5. The BEMsimulation for the nearfield bottom water DO during the 1994 stratified season was lower than in previous years, but did not reach the observed minima. The model also predicted a relatively constant decline in DO during the stratified season, while the observed rate of decline was slower in early summer and faster towards fall. If the BEMis to be used to predict bottom water DO minima on both the event and seasonal time scales, MEG recommends: a) model sensitivity analyses and b) comparison of observational uncertainties be made to help resolve the apparent model-observational differences.

C. Hunt asked what was meant in number 4, “uncertainties inherent in field data”. The statement implies that perhaps something is not being measured correctly. B. Beardsley did not think that was what the statement implied, however, the word “errors” could substitute “problems”.

J. Fitzpatrick asked for clarification on recommendation 5b. B. Beardsley replied that the MEG is asking that knowledge learned regarding dissolved oxygen be applied to the model. J. Fitzpatrick asked if this means trying to relate some of the variability of the physical processes to dissolved oxygen. B. Beardsley agreed. J. Fitzpatrick thinks they are going to be fairly limited in addressing recommendations 4 and 5 since there are not many datasets available that provide enough detail. B. Beardsley thinks if there is no observational database, earlier simulations that considered wind variability could be used to provide some sense of the variability to expect. This information could be used for the 1993 and 1994 model runs.

M. Mickelson asked if recommendation 5b verges on a statistical test of the hypothesis that the model output and the observations are the same. In other words, did the model actually do well for 1994 when error is taken into account? E. Adams thought that was a fair statement.

A. Solow listed the two parts to the question. The model is not perfect, so what kinds of things is it sensitive to, and what kinds of changes to the model would produce the observed conditions. Then there is the question of the accuracy of the observations, or if the difference is due to measurement error or undersampling. J. Pederson pointed out that both the water quality and hydrodynamic models average information over time. B. Beardsley added that changes to the boundary conditions were done monthly. J. Pederson always assumed that the model would only be able to predict when to expect a minimum or maximum and not provide absolute values that were going to be of use to the managers. B. Beardsley thinks no matter how much resolution the model has, it is still a temporal and spatial average. The point is to look at what data are available

to address this. The recommended sensitivity studies would also examine the processes themselves to make sure that the model is calculating approximately the right answer for the right reasons.

J. Shine asked what the time scale of the model output is. J. Fitzpatrick replied that it is generally 2-day averages. B. Beardsley added that the model was actually run on a much finer time scale. J. Fitzpatrick pointed out that information inputted into the hydrodynamic model is updated more frequently than the water quality model. R. Isaac suggested for added insight, loading the system until the point where changes are predicted to see what the loads are.

M. Mickelson welcomed the MEG comments. The Chesapeake Bay model may be in trouble possibly because they did not have a MEG early on. He thanked Wendy Leo who first noticed the need for independent scientific feedback during model development. He thinks the modeling efforts are in good shape, and MWRA will continue to listen carefully to the MEG. MWRA needs to think about what is a possible sequence for approaching all of the recommendations as well as its main goal in mind. He sees the preparation of an addendum to the report as something that MWRA would do early on. Some of the recommendations are related to testing the attributes of the model, and are most likely one-time tests. Others are to modify the model permanently, e.g. grid scale and adding a third algal species. Of course, these things need to make sense in relation to the extra effort needed. Others relate to better boundary sampling. The main goal is to have a suitable model, as the permit requires. It is interesting to note that the large influence of the boundary is partially due to MWRA having a lesser influence, and this detracts from MWRA interest in funding a major share of the modeling. There is now a more regional perspective and broader utility for modeling as a research tool, clearly not something MWRA intended when this exercise began.

J. Fitzpatrick thanked the MEG and believes that they establish credibility for the model. MWRA has agreed to work on the report addenda. He then discussed the other MEG recommendations. In terms of matching the grids, one of the reasons HydroQual collapsed the grid was computational burden. They can now go to the full scale grid, but that will increase the running time from 4 hour runs for one year to 25-40 hours. However, computational speed may increase by a factor of two within the next year. HydroQual will continue to do model projections. The nutrient flux around Cape Ann is certainly an important issue. The model has helped to identify the importance of the boundary condition relative to MWRA's input.

J. Fitzpatrick showed monitoring data of the nitrate long-term trend highlighting the importance of the boundary conditions. He thinks it would be desirable, at least from a modeling point of view, to have more data collected at the boundary by MWRA and/or any other members of the scientific community.

J. Fitzpatrick then discussed MEG's concerns regarding DO. In 1994, the average DO dropped to about 6 mg/L. The model picked up a good portion of this drop, but did not pick up the absolute minima, and this should be addressed. The model also had a difficult time with the reduction in DO decline between February-April possibly due to reventilation as seen in the 1992, 1993, and 1995 monitoring data. One problem is that for the early calibration analysis, there were only 2 stations that specified the boundary condition. He had made the assumption that the DO declined

gradually at the boundary, as opposed to a marked drop and reventilation. At the time of the calibration analysis, no one envisioned that the boundary would be as important. In 1994, two stations were added closer to the Cape Ann boundary to better specify the boundary conditions. Now that there is another event in 1997 similar to 1992, it may be possible to impose the boundary condition back over to 1992 and see if it improves the model for DO. Jack Kelly hypothesized that in 1994, the pycnocline may have deepened late in the summer. In effect, this would decrease the volume of the bottom waters and in turn increase the effect of the sediment-oxygen demand (SOD). The pycnocline did deepen during that period of time, and that may have exacerbated the SOD given the vertical resolution in the hydrodynamic model. Thus adding vertical resolution, at least as a sensitivity analysis for the two models, would help determine if a physical component is missing and HydroQual would consider attempting this.

J. Fitzpatrick took a little exception as to whether the model did indeed miss the 1993 bloom. Chlorophyll is a surrogate for phytoplankton biomass because particulate organic carbon (POC) measurements give no indication as to how much is algal versus detritus. Chlorophyll is a type of integrator of variables, however, it is carbon that affects the DO. In terms of the amount of carbon and primary production, the model may be doing a reasonable job of predicting the fall biomass, though it may not successfully predict the fall chlorophyll. J. Fitzpatrick showed data from the fall of 1993, when samples were very strongly dominated by the diatom *Asterionellopsis*, and the cells had a very low carbon to chlorophyll ratio. There is a lot more chlorophyll but there may not be as much carbon biomass. So the fact that the model missed chlorophyll may not be as important. Models in this country only include two phytoplankton functional groups and he is only aware of the Dutch who attempt to model species.

J. Fitzpatrick feels that in terms of POC, the model seems to be doing a reasonably good job. Models may not get the week to week variation, but can catch some seasonal variability. He then showed how HydroQual attempts to examine the temporal plots on a seasonal basis. Another way of trying to gauge whether the model is doing reasonably well is to look at primary production data. The highest productivity is usually observed in the springtime, between 1-3 gC/m2/day. August was the second highest productive period, with values averaging just under 1 gC/mm/day. The model calculates the highest productivity in the early springtime between 1-3 gC/mm/day, comparing reasonably well with the data. The model is a little late for the next peak in productivity in September, but the seasonal signal is about right. Thus the model's computation of the annual carbon production appears correct. He thinks that though the model missed the chlorophyll component if the 1993 bloom, it is capturing the carbon biomass.

J. Fitzpatrick then showed a plot of the temperature growth rate with optimum nutrients and light. The model uses temperature to control the diatom groups. A fall diatom group would need to be "grown" in a specific temperature optimum of 15-16 degrees Celsius. This same temperature range is seen in the spring, tempting the model to grow the fall group then, unless a special "finger-of-God" routine is added to control the seeding of the fall diatom, but he does not think it would be successful. He does not think that the model results are missing phytoplankton biomass in the fall of 1993.

R. Isaac asked if there are other data besides DO that suggest a ventilation event had occurred. J. Fitzpatrick did not think that the increase in DO during the spring is a top-down ventilation

event. He believes that it has to do with water entering from the Gulf of Maine, and/or a significant reduction in the oxygen-consuming processes, with possibly some side ventilation.

A. Solow thinks it is not convincing to make the argument on one missed bloom, that happened to not affect the carbon, to assume that any missed bloom will not affect the carbon. J. Fitzpatrick agreed, but does not think chlorophyll is the best indicator. It is the carbon that affects the DO, not the chlorophyll. If there is a bloom that causes high POC deposition that the model misses, then it would be imperative to consider improving the model. HydroQual is beginning to collaborate with the Dutch who have a slightly different modeling framework, untested in our coastal systems. There is a standard way that eutrophication models are built in the United States and he is not sure about adding this additional group. The only way to get the high chlorophyll values observed in 1993 would be to change the carbon to chlorophyll ratio to 20 and the problem is that this event does not occur every year. Unless the physiology is well understood, and can be added to the modeling, no model will be able to predict what years this will occur.

A. Solow said that he is not arguing that every year's variability needs to be reproduced, but the model should be able to capture a fall bloom, even a small one yet significant one. J. Fitzpatrick would like to refocus efforts to do that. M. Mickelson asked J. Fitzpatrick to explain to the group how the carbon to chlorophyll ratio works. J. Fitzpatrick explained that phytoplankton produce chlorophyll as an energy source. Under low light conditions, phytoplankton tend to produce more chlorophyll to make better use of that light. The carbon to chlorophyll ratio also adjusts under nutrient-stressed conditions. All of this is built into the model, allowing it to be able to compute the subsurface chlorophyll maximum observed in the field. He thinks if he changes the low end of the carbon to chlorophyll ratio from 40 to 20 to accommodate *Asterionellopsis*, then the model will end up calculating a lot more chlorophyll during the winter-spring bloom, that is currently calculated reasonably well by the model.

ACTION: OMSAP voted unanimously to approve the MEG report and will forward it to EPA and MADEP.

REVIEW OF DRAFT OMSAP LETTER TO EPA/MADEP

OMSAP reviewed the draft letter. A. Solow decided to submit the letter as it is currently drafted and then include the MEG report and benthic threshold revisions in another letter.

L. Schafer thinks it is extremely important that the monitoring program keeps sampling the sea surface for floatables. Though not particularly scientific, this is an issue where the public is apt to become involved. M. Mickelson pointed out that they do have sea sampling of floatables. Often they do not capture floatables but this time they did find a candy wrapper and a piece of packaging. A. Solow pointed out that this field program is mentioned in the OMSAP letter. L. Schafer thinks more information about monitoring methods and frequency should be included.

J. Shine asked how big the field sampling net is. C. Hunt replied that it is 10' long with one opening 2 m wide. The netting is 0.55 mm wide. They are sampling in the nearfield, 2 transect locations between 2 stations, for every nearfield survey, 17 times a year.

S. Testaverde asked if there is any in-plant sampling for floatables. M. Mickelson said that the in-plant study agreed to at the February 2000 OMSAP meeting has not been designed yet. S. Testaverde volunteered to help with the sampling design for the in-plant sampling.

ACTION: OMSAP unanimously approved their draft letter to EPA and MADEP outlining their recent recommendations on MWRA's food web model scope of work, zooplankton, Alexandrium, and floatables thresholds.

BENTHIC DIVERSITY THRESHOLD REVIEW

K. Keay described the benthic community thresholds, species diversity, and relative abundance of identified opportunist species. He requested that OMSAP review how MWRA plans to implement the existing narrative threshold for benthic diversity and consider tightening the opportunistic threshold. The Contingency Plan (CP) has a narrative caution threshold for benthic diversity that considers appreciable change. There was a typographic omission in the CP of the existing opportunistic species threshold. The existing threshold is detailed in the Outfall Monitoring Plan with a caution level set at 25% of abundance and the warning level set at 50%.

K. Keay described the development history of the benthic diversity threshold. In 1988, EPA attempted to predict the effect of the future outfall by modeling the deposition of solids and organic carbon from the discharge to the surrounding area. They predicted for the worst case scenario, primary effluent with strong stratification, the area later chosen for the future outfall would have modest organic enrichment that would impact, but not degrade, the benthos within only a relatively small area. Early monitoring by MWRA and USGS documented the impacts of sporadic severe storms, both on sediment transport and on the benthic communities. In addition, the monitoring data show strong year-to-year variability.

K. Keay then showed EPA model runs from the Environmental Impact Statement. EPA determined that with primary treatment, there would be an area of degradation around the outfall and then an area of less degradation a little further out due to organic carbon deposition. For full secondary, they predicted a substantially smaller area of change in habitats. Thus the first iteration of benthic thresholds considered a nearfield/midfield concept because modest changes were expected in the vicinity of the outfall, with few outfall changes further away. Preliminary hypotheses expected changes within roughly 2 km of the discharge, approximately equaling a total of 12 km2 that will not trigger the thresholds. An appreciable change at midfield communities further away, not attributable to sediment transport from a major storm, would trigger the threshold.

K. Keay then showed a map of sampling stations. MWRA collects three faunal samples at each farfield station, and three nearfield stations. A single sample is collected at the remainder of the nearfield stations. There are some shortcomings to the nearfield/midfield approach due to the fact that the outfall is to go on-line with secondary treatment with areal impacts predicted to be substantially smaller. In addition, the Bays Eutrophication Model projections predict an inshore offset (east of Winthrop) of POC deposition from the outfall discharge that is smaller than but identically located to the one predicted with the harbor discharges. Thus expecting changes to be centered on the outfall appears unsupported. J. Shine and B. Beardsley noted that the model takes treatment plants on the North Shore into account. K. Keay said that the area of higher POC

deposition has been observed in all three of the modeled years (1992-1994). J. Fitzpatrick pointed out that the Lynn treatment plant discharges in that area and that load is in the model but not the flow. He said that they will look to see if this remains a dominant feature on a finer scale.

K. Keay said that these results suggest that MWRA should be examining the entire suite of stations in the nearfield. Thus MWRA is longer pursuing the nearfield/midfield approach. MWRA is recommending that they average the data over all samples collected in the nearfield plus the three western Mass Bay stations, including the area predicted to have the maximum summer POC for all of the benthic thresholds, including the contaminant and RPD thresholds.

K. Keay then described the four threshold diversity indices used by MWRA. Two examine species richness, total species (simplest biodiversity measure, easy to explain) and Fisher's log series alpha (initial slope of the species accumulation curve). The total species measurement is somewhat sample-size dependent, i.e. samples with higher abundances tend to have higher numbers of species (log series alpha corrects for this). Shannon-Wiener is the classic diversity measure, and is sensitive to both richness and diversity. Pielou's J prime calculates evenness and is similarly widely used and relatively well understood by ecologists.

K. Keay then showed evidence in the nearfield of a 25-30% decrease in species richness between 1992-1993. There is strong supportive evidence that this is the result of the 100-year storm, the "no name nor'easter". There is some argument as to whether the increase after the storm is all recovery or whether it is part of a long-term cycle in species richness, as identified in other systems.

B. Beardsley asked if these data are total number of species per grab. K. Keay replied, yes, total identifiable species. A. Solow did not think that the number of total individuals per grab was taken into account, because that can be explained by abundance. K. Keay agreed and noted that abundance in the nearfield has increased quite a bit. Log series alpha only considers species richness, not sample size. A. Solow thinks that even though the grab size and sampling location do not change, there are still different numbers of individuals in samples. K. Keay agreed but stated that the log series alpha corrects for that. He then showed species richness with log series alpha, showing the same pattern as with total good species.

A. Solow noted that none of the measures take into account species identities. There could be the same numberof species, but completely different species, possibly a large ecological change, and none of these measures would pick that up. K. Keay agreed and pointed out that that the time pressures for reporting on thresholds do not permit for a detailed analysis the samples, that is why there is the opportunistic taxa threshold. The diversity measures available do not look at community composition. A. Solow liked the approach.

K. Keay then reviewed baseline monitoring results showing strong apparent trends in baseline diversity measures. The Mass Bay samples are much more diverse, species rich, and more evenly distributed than in Boston Harbor. It is important to note that organic enrichment can cause either enhancement at moderately low levels or a strong decrease at higher levels in faunal abundance and diversity. Thus it was decided to use a two-tailed test that viewed either a substantial increase or a decrease as a potential cause for concern. After reviewing the baseline data,

MWRA recommends a threshold test for the narrative species diversity threshold that states the annual mean diversity for any of the four (species richness, log series alpha, Shannon-Wiener diversity, and Pielou's J prime) will not fall outside the central 95th percentile of the baseline data. No exceedances were observed during baseline monitoring in the nearfield through 1999 with the thresholds calculated in this way, and more importantly, these threshold ranges appear reasonable given the variability. The nearfield results are all well above the highest values measured in Boston Harbor.

A. Solow was a little bothered by the apparent trends over time and the fact that the whole nature of this kind of threshold argument is based on the assumption that there is variability around a mean. Much of the variability is due to the observed trend. K. Keay agreed and said that MWRA has struggled with this, but he thinks if there is a pattern that is different from what was seen in the baseline, even if it does not actually exceed the threshold, MWRA will still conduct a detailed data analysis and interpretive report. The thresholds should pick up a large change and subtle changes should be discerned for the annual synthesis report. A. Solow thought that was a good answer.

K. Keay noted that if the farfield data continue to pursue an 8-year cyclical pattern in response to hydrographic forcing, but the same pattern is not seen in the nearfield, it may be evidence of some stimulation of the community. J. Fitzpatrick suggested MWRA examine the recovery of the old Boston Harbor sludge dumpsite to test this theory. K. Keay replied that they could certainly look at that, though there are not enough quantitative baseline samples. He pointed out that in 1998, species richness in Boston Harbor was a lot higher than the harbor average and was approximately twice the species richness than measured in 1991.

M. Mickelson brought up the question asked by A. Solow again. How does a trend fit in with MWRA's simple use of a normal distribution? Perhaps if one did include trend or seasonality, it would cause the thresholds to be even tighter. K. Keay thought it would be possible to at least qualitatively fit a sine wave to the data and add confidence intervals. M. Mickelson said that it would make the thresholds tighter, and they would be too easily triggered. W. Leo added that this would increase the risk of a false alarm. C. Krahforst pointed out that if this trend is real then the data are destined to trigger the threshold.

D. Tomey asked about the MWRA 301h samples. K. Keay stated that there are 301h data available from the 1987 outfall siting process. However, a detailed comparison of the old data with more recent data would require a major reconciliation of the species lists and data sets. D. Tomey asked if this was because of a difference in sampling. K. Keay replied, yes, there were several differences including sieve sizes. One example is that there were several cases where the taxonomy was not well known for the animals and thus accurate numbers of each species could not be determined. Overall, it would take quite some time to bring the 1987 data into some comparability with the 1992 data.

B. Beardsley asked if there were any ideas about what may be causing this year-to-year variability. K. Keay replied that there are several theories such as: recolonization of frequently disturbed bottoms; release from predation from increased trawling; and recovery from frequent trawling impacts. However, MWRA does not have the data to prove any theories related to

fisheries. MWRA is actively looking at some other datasets such as the Gloucester wastewater treatment plant 301h waiver program. A. Rex added that this pattern has important implications for regulators and managers when it is time to look at impacts. This looks like an oscillation, and it was fortuitously noticed during the baseline period, otherwise it may have been blamed on the outfall.

A. Solow asked which taxa are changing, which are becoming more abundant, and which are becoming less abundant. K. Keay replied that there appears to be an increase in the numbers of rare species in the average sample. The identities of the dominant taxa in Mass and Cape Cod Bays are not changing, though their abundances have increased. If it were changes seen in the 10-15 dominant species in the fauna, MWRA could hope to address that. Unfortunately there are many lesser known species that could also change. M. Mickelson asked A. Solow what he saw in the data using his statistician's eye. A. Solow thinks that though there is a lot of variability, he does agree that there appears to be a pattern.

B. Beardsley also agreed. He added that scientists have compared indices such as the North Atlantic Oscillation (NAO) database and have seen some amazing statistical relationships between, for example, zooplankton biomass on Georges Bank versus NAO. He asked if MWRA was looking at other databases outside of the monitoring data. K. Keay replied that they are. Even though MWRA cannot determine if this trend is real based on only eight years of data, if this cycle does seem to correspond with some hydrodynamic forcing function, the information would be provided in the interpretive report. R. Isaac noted that though this looks like an apparent trend, it is hard to say if there is a statistically valid pattern. K. Keay agreed. There is an apparent trend that MWRA needs to keep in mind as the data are interpreted and calculated for comparison to a threshold.

K. Keay then described the development of the pollution tolerant opportunistic species threshold. The species considered for this threshold have a good literature base in terms of being pollution-tolerant (to either toxics or organic enrichment in the sediments). MWRA is still investigating whether there are physiological constraints that might keep *Ampelisca abdita* and *Ampeliscavadorum* from building up to high abundances in the western Mass Bay sediments. The existing thresholds for opportunistic species are a caution level at 25% of mean nearfield faunal abundance and a warning level at 50%. This is determined by examining the average relative abundance within any of the 35 samples within western Mass Bay. The 1999 data continue the trend seen in previous years. These animals are present but in very low abundances throughout the entire western Mass Bay and Cape Cod Bay system, with a maximum of about 1.5% on average of the fauna in 1992, and decreasing and remaining at extremely low levels in the last few years. This is much lower than the current thresholds. In comparison, Boston Harbor has much higher values. Thus MWRA thought the thresholds should be substantially more sensitive and is recommending to tighten them to a caution level of 10% and the warning level of 25%. If as few as one third of those samples had a third of their abundance made up of the six opportunistic taxa, MWRA would be triggering that caution level threshold.

A. Solow thought that only a 5% increase would be a significant change from what has been seen up until now. K. Keay agreed but did not think that it is enough of a change, over a large enough area of the nearfield, to warrant the triggering of a threshold. He added that there are some minor

predicted impacts from this outfall that could include a modest increases in abundances. Overall, MWRA thought that 10% was both protective and appropriate.

J. Shine asked about Ampelisca. K. Keay replied that MWRA will continue to look into whether *Ampelisca* can survive in Mass Bay, and if not, there are taxa that could replace this species in the opportunistic threshold. J. Shine and K. Keay then had a brief discussion on the survivability of other opportunists in the subtidal zone.

B. Beardsley asked about how much fishing and dragging occur in the nearfield. C. Hunt replied that dragging is not very prevalent. S. Testaverde added that there is no dragging in the nearfield since it is within state waters. It has been illegal to drag in Mass Bay since 1972. MADMF would be able to speak about the laws. [Post-meeting note: dragging is not completely prohibited in Mass Bay]. J. Pederson asked D. Tomey about the barrels picked up off of Scituate. D. Tomey replied that EPA conducted a barrel study that found quite a bit of dragging by fishermen, just east of the outfall. C. Krahforst added that MADMF is allowed to drag for their trawl surveys. B. Beardsley raised this issue because of the recovery of certain species such as scallops after some areas on Georges Bank were closed.

K. Keay noted that USGS found in 1991 that the area called Rosie's Hole near the outfall was characterized by deep and frequent trawl scars. Other suitable areas nearby did not show trawl scars, presumably because it is difficult to trawl the long axes of the drumlins there. It may be useful to revisit the USGS side scan data. B. Beardsley pointed out that MWRA should want to know the trawl history of the stations. K. Keay added that MWRA could also make assumptions on which stations could not be trawled based on topography.

K. Keay asked OMSAP if they accept MWRA's recommendations. A. Solow liked the recommendation to tighten the opportunistic species thresholds. He thought that the threshold arguments on the diversity measures are weak but the fact that MWRA is going to analyze the data in detail is the best that probably can be done. He acknowledged that it is difficult to draw inferences about changes in the Shannon-Wiener measure of diversity and added that none of the measures are sensitive to changes in the actual species. However, MWRA will be looking at the opportunistic species. His one reservation is that the interpretation of the threshold calculation is very difficult. J. Shine pointed out that MWRA will still be doing other analyses to help in the interpretation. K. Keay agreed and emphasized that the thresholds are an early warning, and not meant to replace a detailed evaluation of the data.

B. Beardsley and K. Keay then discussed salinity measurements and benthic sampling. B. Beardsley asked if the monitoring program can discern the gross effects of storms. K. Keay replied yes, this can be discerned in the sediment profile imaging. B. Beardsley pointed out that internal waves generated over Stellwagen Bank and Basin during the summer months can cause resuspension and provide a mechanism for moving fine sediment out into the basin. It was previously believed that the primary mechanism for moving fine sediment in this area were winter storms. So now it seems there is a mechanism all throughout the year to carry material into Stellwagen Basin. His point is that independent of the thresholds, MWRA will be analyzing the data in detail to look for changes.

A. Solow stated that he would vote to approve these changes on the understanding that all of these other analyses will continue, making him comfortable that we are going to do the best possible job that we can in detecting changes in the benthos. J. Shine added that if there is an exceedance of these thresholds, there is other data to help determine the cause.

ACTION: OMSAP unanimously agreed to recommend three revisions to the benthic thresholds: 1) stations considered in the threshold calculations should include the nearfield stations plus the three adjacent western Mass Bay stations; 2) the benthic threshold test for annual mean species diversity should not fall outside the central 95th percentile of the baseline data; and 3) the existing pollution opportunist threshold should be tightened to make it more environmentally protective (caution from 25% to 10% and warning from 50% to 25%). OMSAP will forward the recommendations to EPA and MADEP.

GENERAL DISCUSSION

S. Testaverde requested to revisit OMSAP's recommendation to delete the floatables threshold. NMFS opposes this and if EPA and MADEP do approve of the deletion, this means a change to the Contingency Plan attached to the permit and he would then like to open up an Endangered Species Act section 7 consultation. A. Solow replied that OMSAP approved of the letter recommending the deletion of the floatables threshold and that the letter will be forwarded to EPA and MADEP.

B. Beardsley brought up the Model Evaluation Group's next step. Their formal task, to review the model results, is complete. However, the MEG would like to continue to work with MWRA and HydroQual in determining which tasks on the MEG's list of recommendations are under MWRA's purview and can be completed. He asked OMSAP if MEG may continue to have this dialogue. A. Solow agreed and thanked B. Beardsley and the rest of the MEG for their efforts. B. Beardsley added that Rich Signell (USGS), who developed and ran the hydrodynamic model is about to leave for a 3-year fellowship. The transition of the hydrodynamic model will be very critical and the MEG wold like to help see that done. M. Mickelson thinks MWRA can prepare a workplan to examine the transition of the hydrodynamic model.

INTER-AGENCY ADVISORY COMMITTEE AND PUBLIC INTEREST ADVISORY COMMITTEE UPDATES

C. Coniaris summarized the Public Interest Advisory Committee's (PIAC) last meeting on May 2, 2000. The group discussed OMSAP's recent recommendations regarding thresholds and the food web model scope of work. PIAC wanted to become more involved as MWRA developed its floatables in-plant special study as recommended by OMSAP at their February 2000 meeting. PIAC requested that Mike Mickelson (MWRA) present information on the Dutch *Phaeocystis* model and the Great Lakes zooplankton modeling at a future PIAC meeting. PIAC also requested that their next meeting be scheduled on the same day as OMSAP so that it would be easier for PIAC members to attend the OMSAP meeting and hear the Panel's discussions firsthand. There have been a few membership changes: Gillian Grossman (PIAC chair), has departed from Save the Harbor/Save the Bay and PIAC will elect a new chair at its next meeting in the fall. Susan Redlich has departed from the Wastewater Advisory Committee and thus PIAC. Both organizations would like to continue participating on PIAC.

S. Testaverde gave a brief Inter-Agency Advisory Committee (IAAC) update. The group has not met since the last OMSAP meeting. The original IAAC mission in the OMSAP charter dated October 1998 is "The committee [IAAC] will advise the OMSAP on environmental regulations." Several IAAC members felt that this mission statement was too narrow. IAAC voted to approve the following recommendation to change its mission to: "The committee will advise the OMSAP, EPA and MADEP on scientific, technical and/or regulatory matters related to discharges from and operations of the MWRA system outfalls that may directly or indirectly affect Boston Harbor, Massachusetts Bay, and Cape Cod Bay. The IAAC may review or evaluate other environmental matters as necessary." EPA and MADEP objected to that mission statement and on April 6, 2000, S. Testaverde met with Chris Mantzaris (NMFS), Ron Manfredonia (EPA), Glenn Haas (MADEP), and Cathy Coniaris (OMSAP staff) to discuss IAAC's roles and mission. There was consensus to change the mission of IAAC into a forum for information exchange among the agencies interested in MWRA's outfall permit. The forum would inform EPA and MADEP but not OMSAP directly. A. Solow said that it is his understanding that EPA and MADEP will attend a future OMSAP meeting so that we can all discuss this.

ADJOURN

MEETING HANDOUTS:

- Agenda
- OMSAP/IAAC/PIAC membership lists
- Draft March 2000 OMSAP minutes
- Draft letter of OMSAP recommendations to EPA and MADEP
- Draft Model Evaluation Group Report to OMSAP
- Copies of MWRA Presentation Transparencies
- MWRA Benthic Community Threshold Information Briefing

Summary prepared by C. Coniaris. Post-meeting comments are included in [brackets]. All such comments have been inserted for clarification only. They do not, nor are they intended to, suggest that such insertions were part of the live meeting components and have been expressly set-off so as to avoid such inference.