OUTFALL MONITORING SCIENCE ADVISORY PANEL (OMSAP) MEETING
Monday, April 29, 2002, 10:00 AM to 2:00 PM, WHOI Carriage House
FINAL MINUTES

ATTENDANCE

Members Present: Andy Solow, WHOI (chair); Bob Beardsley, WHOI; Norb Jaworski, retired; Bob Kenney, URI; Scott Nixon, URI; Judy Pederson, MIT/Sea Grant; Mike Shiaris, UMass Boston; and Jim Shine, Harvard School of Public Health.

Observers:
Bruce Berman, Save the Harbor/Save the Bay; Peter Borrelli, Center for Coastal Studies; Mike Bothner, USGS; Todd Callaghan, MCZM; Cathy Coniaris, MADEP; Mike Delaney, MWRA; David Dow, NMFS; Patricia Foley, Save the Harbor/Save the Bay; Tom Fredette, USACoE; Maury Hall, MWRA; Carlton Hunt, Battelle; Russell Isaac, MADEP; Chris John, MWRA; Ken Keay, MWRA; Matt Liebman, EPA; Lisa Lefkovitz, Battelle; Steve Lipman, MADEP; Stormy Mayo, Center for Coastal Studies; Mike Mickelson, MWRA; Jerry Neff, Battelle; Jeff Reade, MWRA; Andrea Rex, MWRA; Steve Rhode, MWRA; Larry Schafer, retired; Jack Schwartz, MADMF; Dave Taylor, MWRA; and Steve Tucker, Cape Cod Commission.

SUMMARY OF ACTION ITEMS & RECOMMENDATIONS

1. OMSAP approved the October 16, 2001 minutes with no amendments.
2. OMSAP accepted the March 2002 MEG report. OMSAP recommends the following: MEG continue its review of the model output; MEG membership should rotate; and that MWRA develop a plan to maximize the use of this model (e.g. publish in peer-reviewed journals and graduate thesis work).
3. OMSAP recommends that MWRA continue mussel monitoring using the same protocol as summer 2001.
4. OMSAP will convene during summer 2002 to begin discussions on developing a process for the review of the Monitoring Plan.

MINUTES

WELCOME, APPROVAL OF MINUTES

ACTION: OMSAP approved the October 16, 2001 minutes with no amendments.

FLOATABLES

L. Schafer suggested that any planned sampling for floatables consider what sizes of material should be sampled, and what volume of effluent should be screened since floatables are rare in secondary-treated effluent and difficult to quantify. N. Jaworski asked why there is a floatables threshold if the floatables coming out of the treatment plant cannot be measured quantitatively. B. Berman replied that the Clean Water Act includes aesthetic criteria.

A. Rex pointed out that MWRA has already come to an agreement with EPA and MADEP as to how MWRA will to sample floatables. A flow-paced sampler has been designed and will soon be ready to sample at the plant. In addition, MWRA is sampling in the nearfield with nets.

PUBLIC INTEREST ADVISORY COMMITTEE UPDATE
P. Foley reiterated one important comment from the fall 2001 OMSAP workshop, the importance of continuing to monitor the effects of the outfall. She then thanked the members of OMSAP as well as those from the regulatory agencies who were enormously helpful in providing outreach to the public as we planned these meetings.

MWRA UPDATE
M. Mickelson described the bacterial exceedance in December 2001. The chlorine level was too low, coinciding with a rainstorm and the problem was quickly corrected. The second exceedance, mussel bioaccumulation, will be described in greater detail later. Also, this spring, there was a bloom of *Phaeocystis*, with levels up to one million cell/L (not as high as the 14 million cell/L measured in 1997).

S. Mayo said that the spring 2002 concentrations of right whales are probably the lowest seen in 17 years of study, probably even lower than 1997. ~15 individual right whales have compared to the 80-100 individuals normally seen. The residency time, though difficult to calculate, seems to be exceedingly low. In February, the only time that whales were present, a brief burst of *Pseudocalanus* was measured and this was followed by the lowest concentrations of *Calanus* that we have ever seen. He is not inclined to think that this is due to the *Phaeocystis* bloom. It does appear to be a very unusual year for right whales and zooplankton. Water 20 miles east of the Cape has abundant right whales and *Calanus*. The nitrogen isotope monitoring program is continuing and he has asked Joe Montoya to present to OMSAP at an upcoming meeting.

D. Dow mentioned that he heard that there were concentrations of right whales on Stellwagen Bank and he asked if they could sample there for *Calanus*. S. Mayo replied that they sampled near there (east of the bay), and they have seen almost exclusively late stage *Calanus*, but the patches are not as close to the surface. As we have seen in the past, right whale distributions are tightly tied to the availability of their food resource. B. Beardsley asked if the aerial survey goes outside of Cape Cod Bay. S. Mayo replied that the Center for Coastal Studies, with MADMF funding, is working principally in Cape Cod Bay. Occasionally when we have extra time, we travel outside the bay. NMFS is flying aerial surveys outside of Cape Cod Bay.

BIOACCUMULATION OF CHLORDANE AND PAH IN MUSSEL TISSUE: A CAUTION LEVEL EXCEEDANCE
C. Hunt reviewed the progress made in analyzing data related to the mussel tissue exceedance for chlordane and polynuclear aromatic hydrocarbons (PAHs) sampled during summer 2001 at the outfall site in Mass Bay. Caution level thresholds for the two parameters are set a twice the baseline. The caution level for chlordane is 205 ppb (lipid-normalized) compared to the 250 ppb measured during the summer of 2001. The caution level for total PAH is 2160 ppb (lipid-normalized, using the NOAA list of 24 PAHs) compared to the 3024 ppb measured. The values are well under the FDA thresholds for human health. The Deer Island Treatment plant was operating very well during the entire mussel deployment period. Chlordane and PAH values in the effluent were low, even lower than the water quality criteria.

C. Hunt related the levels measured in mussels and effluent using standard bioaccumulation equations, and evaluated the potential health effects to human and mussels. [For more details, see MWRA Report entitled "Evaluation of 2001 Mussel tissue contaminant threshold exceedance" located at: http://www.mwra.state.ma.us/harbor/enquad/pdf/2002-05.pdf]

J. Shine suggested that desorption (not quantified) would increase the dissolved phase and cause more bioaccumulation. S. Nixon mentioned a possible bias due to use of log vs. log plots in these analyses. D. Dow highlighted the assumption that the concentrations found in mussels result from equilibrium partitioning rather than from the food sources such as phytoplankton and detritus.

N. Jaworski asked why the mussels from the outfall had double the concentrations of chlordane than those from Deer Island and Boston Inner Harbor. J. Neff suggested that there may be a larger flux of atmospheric chlordane to Mass Bay. Also we are dealing with extremely low concentrations that are not only hard to measure but also hard to interpret environmentally.

B. Berman suggested that caged mussels be deployed more often than just in summer. M. Hall replied that the deployment must be synchronized with the mussel spawning cycle which affects the mussel lipid content and thus contaminant concentrations. B. Berman asked if longer deployments would give higher concentrations, but C. Hunt replied that this is an equilibrium process.

B. Beardsley asked why the Rockport mussels are so much cleaner than Cape Cod Bay mussels. M. Hall replied that the sampling location is at a more exposed site with less sediment deposition where they are thoroughly cleansed. C. Hunt continued with his presentation on the evaluation of the exceedance. He explained that predicted values for chlordane were not higher than the measured. J. Shine assumed that the equations used did not specifically account for variations in DOC (dissolved organic carbon). C. Hunt said they will try to factor in DOC. M. Shiaris noted that there seems to be more uncertainty for chlordane than PAHs. C. Hunt said that it is because they are measuring extremely low levels of chlordane in the mussel tissue.

A. Solow said that since predicted exceeded observed and observed exceeded the threshold, we would have predicted that there would have been an even larger threshold exceedance. He asked C. Hunt what he thought about that. C. Hunt replied that when these thresholds were developed in the mid-1990's, based on the best science available, they used bioaccumulation factors for fish, which have different metabolic pathways than mussels. As a result, two times baseline seemed like a good threshold at the time. J. Shine added that it could be that the predicted levels are too high for accounting for factors such as DOC differences. A. Solow agreed and said that it could be due to many different factors. B. Beardsley added that another factor could be what the actual dilution is in the ocean. C. Hunt said that the cause of the disparity between the measured and predicted chlordane values is in part due to the susceptibility of the chlordane method to analytical interferences and the fact that a more recent chlordane bioaccumulation regression coefficient was not used. The difference between measured and predicted PAH values may be due to the fact that 80% of the PAHs in mussel tissue are high-molecular weight (HMW). The HMW PAHs are slow to come to equilibrium and thus deployment time may be too short for these compounds to reach equilibrium, causing an underestimate in measured relative to predicted concentrations. For both chlordane and PAHs, uncertainty in the various factors used in the predictions could contribute to the disparity between measured and predicted concentrations.

J. Shine asked if there are standard reference materials (SRMs) for chlordane as a quality check. If they run those materials and obtain results comparable to the known standard, then they can believe the results from the unknown samples. L. Lefkovitz replied that they have in the past done some mass spectrometry in their tissue analysis for some of the pesticides because with ECD, it is difficult to know whether there is a false positive. There were slight variations, but they do not have identified co-elutions as they do with some of the PCBs, DDTs, and dieldrin, but there is the potential for some

interference. C. Hunt added that given the analytical method that they use and the quality control checks that are included, they believe that the results are relatively accurate. They can improve those numbers by using another method with greater accuracy, but it is much more expensive. He then described the sources of error in bioaccumulation factors and their potential contribution. Next, he discussed caged mussel exposure to the effluent plume. The effluent plume was in different locations over the course of the mussel deployment. Mussels were exposed to both background contaminant levels and varying levels of dilute effluent. This depends on effluent flow, current speeds, rise height of the effluent (which is stratification driven), and the background build-up of effluent (i.e. far field dilution).

C. Hunt summarized by saying that the predicted PAH and PCB concentrations in mussel are fairly good with predicted values 2-3 times higher than measured. Predicted chlordane concentrations are also good with predicted values 2-3 times lower than measured. Variability in measured data and assumed literature values may cause some of the discrepancy. Reducing uncertainty in level of contaminant concentration in the effluent and in the partitioning between dissolved and particulate phases are important factors in predicting bioaccumulation potential.

C. Hunt then went on to discuss the predicted PAH concentration in water [see handout]. Looking at the full suite of PAHs measured, they wanted to look at whether there is the potential for any kind of health impact on those mussels. He listed the types of information examined and it appears that even considering a cumulative basis, the mussels are not being affected by the contaminants that are being measured.

T. Callaghan asked if mussels can live at the depth at which the cages were deployed. C. Hunt replied yes, though the species living around the outfall in 100 feet of water is different from the test species. T. Callaghan asked what this monitoring is designed to look at, whether it is a signature of the outfall, or the health of the mussels. C. Hunt replied that the reason why we do this monitoring is to see if there are bioaccumulating compounds that are taken up at levels of concern. J. Neff added that we are using the mussels as a surrogate for a valued ecosystem component (e.g. fish). If the mussels remain healthy, then we assume that it is safe for other organisms too. C. Hunt mentioned that MWRA also samples flounder and lobster from around the outfall and analyzes their tissues for contaminants, and there were no exceedances in 2001. J. Shine added that since mussels do not metabolize these compounds very well, they can be considered a "worst-case scenario" for bioaccumulation.

D. Dow asked if they have looked in the literature for whether biomarkers of effects and what the levels of PAH would be associated with those as potential health effects as opposed to chronic/acute toxicity. J. Neff replied that the criteria they used was from mussel work in Great Britain that looked at scope for growth, which integrates many factors. They developed a level above which this biomarker is effective. We used that, as well as acute toxicity data. D. Dow said he was thinking of molecular, as opposed to scope for growth. J. Neff said that there are no unique molecular biomarkers for PAHs in mussels. C. Hunt added that there is not that much information available on different effects vs. contaminant levels on this species.

B. Berman thinks that to discuss the potential effects on mussels, then the length of exposure is important. He wondered how long it takes for chlordane concentrations in mussel tissue to reach equilibrium, whether there are good data that show particulate-bound vs. dissolved. J. Pederson said that mussel scope for growth studies in New Bedford Harbor (high load of PAHs and PCBs) and a clean site found that you would have some effect with high PCBs, but not as much as you would

expect. Studies have also been done on reproductive output and as far as she knows, those are the only biological data on effects of these contaminants on reproductive output. With the PCBs in New Bedford Harbor, reproduction was cut in half, but the levels are so much higher than in Mass Bay. So it is hard to prove that there are any effects on the mussels using the literature. M. Hall added that the concentration that the mussels would be exposed to would be partially driven by the dilution. These mussels were placed in the thermocline. As the thermocline begins to breakdown, around September, the dilution is going to go up, and the concentrations that they are exposed to should go down.

T. Fredette asked if even though HMW PAHs are increasing, do we have some sense where they are on that curve. J. Neff replied that there are some ways to estimate that using a polynomial regression to predict how close to the equilibrium they may be. The problem with long-term exposure is that the offshore environment is probably not ideal for mussels because they may not be getting enough food. T. Fredette agreed that the mussels seem to be approaching equilibrium, but that seems to be a lingering question. S. Nixon asked what the cost of the mussel monitoring program is. K. Keay replied ~$100K/year.

S. Mayo asked if they have looked at the mussels that normally live near the outfall. C. Hunt replied that they have not looked at the horse mussels that are down at that depth. S. Mayo thinks that certainly that would be a good example of an animal that is exposed to the effluent over the long term and is not starving. C. Hunt agreed that that was a good concept but what they do not know is what the effluent concentration is right at the bottom. All of the data from summer 2001 shows that the plume is up in the water column and it is not right at the bottom. The effluent does rise, as predicted, to the pycnocline area in the summer and then is diluted, so the exposure at the bottom might be much lower. S. Mayo still thinks that would be interesting and would not cost as much as the caged mussel studies. C. Hunt said the bottom line of his presentation is that they do not see a potential for an effect. J. Schwartz asked if the mussels have had the chance to all spawn. C. Hunt replied that they are taken from Rockport at the end of June and by then they have spawned. J. Schwartz noted the difference in fat content between mussels that have and have not spawned. M. Hall said that they have looked at the spawning season in the past, which is why they have chosen the dates of deployment, because presumably the animals have already spawned.

C. Hunt then listed the open questions and uncertainties: (1) what is the actual average concentration of contaminants to which the mussels are exposed at caged mooring locations; (2) what fraction of Deer Island effluent, as well as the diluted effluent at the exposure locations, is dissolved and therefore most bioavailable to the deployed mussels; and (3) are the present threshold values or deployment locations appropriate for the intended purpose of the fish and shellfish program. S. Nixon asked if another question was, has the program fulfilled the purpose for which it was designed, and so the $100K/year could be better utilized elsewhere.

J. Pederson asked if they are measuring the lipid concentration of each of these samples or is it an average. M. Hall replied that it is measured with each replicate and each replicate is 10 mussels. R. Isaac asked if chlordane is in limited use, or banned. C. Hunt replied that it has been banned for quite some time, but there are still some homes that may have these chemicals and still use them, and there also is an atmospheric input. N. Jaworski added that chlordane is also probably in runoff.

J. Schwartz suggested that MWRA forgo further study and use the $100K/year toward source reduction efforts. C. Hunt thinks that is one option that could be considered. He then presented possible studies for 2002: (1) field measurements of PAHs and pesticides in the offshore water; (2)

better characterizing effluent concentrations of the organic compounds; and (3) analyzing mussels from more than one deployment at a constant distance (60m) from the outfall. Field measurements were rejected on the basis of technical challenges and high cost relative to the utility of the data for describing the average exposure duration and concentration. More precise estimates of contaminant levels and forms discharged by MWRA in concert with a modified analytical plan for the 2002 caged mussels are recommended to help evaluate if the Contingency Plan thresholds for PAH and chlordane are overly conservative [see handout for details].

N. Jaworski thought that mussel bioaccumulation could studied in a laboratory using various dilutions of effluent. C. Hunt said that unfortunately they did not set up the mussel monitoring that way. T. Fredette asked if the levels that were measured in the effluent different than the levels that were used to make the initial predictions. If they are comparable, then perhaps the wrong equations were used. M. Hall thinks that for some of the estimates, pilot plant data were used in ~1995. Some contaminant concentrations are now higher, and some are lower, however, there is not a large difference. T. Fredette thought he heard that using the old way of predicting, they would have not predicted a 2x baseline difference. Using the new, better equations, now that we understand the science and the kinetics better, now we would predict the 2x baseline difference, so did we set ourselves up unknowingly for failure, and if we had used this better knowledge, would not have set the same threshold, is that really the question, instead of going out and trying to do longer deployments and other things, you are still going to come up with the same thing. C. Hunt does not know if we would have set the same threshold but the numbers would say that it would be higher than what the 2x baseline is, if we believe the forward calculation.

J. Shine thinks what we are getting at is that we had a nice discussion about why chlordane might have exceeded the 2x baseline threshold. But the real question is, what threshold should we have, what are the goals, so we want to protect human health, organism health, or detect change. Maybe the 2x baseline threshold is flawed from the onset and we need to come up with a better strategy for setting thresholds. S. Nixon thinks that 2x baseline was the best threshold that could be developed at the time, a program was put in place and we have one year of post-discharge data. Everything that C. Hunt has said is that all the evidence suggests that this is not a problem. People checked it out, it is not a problem, and it would be best to put the resources into looking at other potential problems. A. Solow feels that there was a threshold that was set, it has been exceeded, and it is not exactly clear why. There is a proposal to spend more resources more detailed studies to understand it, he is not in favor of that, since it seems like it is a waste of money. On the other hand, just to stop the mussel monitoring, even if it is the right thing to do scientifically, someone has to pay attention to the confidence that people will have in this program. He agreed with J. Shine that the mussel monitoring should continue because we only have one year of post-discharge data. He thinks the sampling should continue for a few more years. M. Shiaris suggested also monitoring PAH and chlordane in effluent more closely so that the models can be more fine-tuned. J. Shine thinks continued monitoring of the mussels is the best approach because they are bioaccumulating and hopefully representative of other bioaccumulating components. Obviously we have a worst-case scenario because these mussels are hung in the effluent.

A. Solow listed the three options for OMSAP to consider: (1) recommend that MWRA conduct the monitoring activities C. Hunt listed; (2) recommend to stop mussel monitoring; and (3) recommend that MWRA continue mussel monitoring as in 2001 at least for a few more years. OMSAP could also decide to revise thresholds.

J. Shine thinks part of a monitoring program is to demonstrate compliance, that they are protecting the environment so we still need to demonstrate this, year in and year out. S. Nixon thinks the first option should be rejected in favor of using those resources for more important and pressing questions. OMSAP agreed. J. Shine does not think that over these thresholds is a top priority. He does think that it is important to consider whether we looking for "meaningful change" or "significant change". By hanging mussels right at the effluent, we have seen that there is some significant change, we have made it so we are definitely going to see that, it still may not be meaningful and so he does not see it necessarily being worth getting more spatially resolved sampling where at our worst case scenario we have seen a change. It is worth knowing but it is still below some meaningful level where we would have more concern.

N. Jaworski asked if the additional effluent data would help reduce the uncertainty C. Hunt presented. C. Hunt said yes, it would produce data that are locally relevant. N. Jaworski thinks then that one more year of sampling plus the additional effluent monitoring makes sense. A. Solow pointed out that these are going to resolve uncertainties about something that is not all that important.

S. Nixon noted that the data do not suggest that bioaccumulation is anywhere near as significant as you would expect it to be based on your conservative physical assumptions in the laboratory work. And you have 5 compounds that have been tested for in a worst-case environment with the cage right over the diffusers. How much more evidence do you wan to have before you say bioaccumulation is not an issue for these compounds.

N. Jaworski said that since this is a combined sewer system, there is a lot more discharge in a wet year vs. a dry year. S. Nixon still not think there would be a big difference between a wet and a dry year. A. Solow does not think that we have to prove it year in and year out in perpetuity, but at least twice. J. Shine pointed out that annual monitoring would make it possible to spot trends in bioaccumulation.

J. Pederson thinks that there should be at least another year of monitoring for the mussels and she does not think that MWRA needs to do too much more in terms of the effluent plan. This is leading nicely into the next discussion. When we developed the Monitoring Plan, we realized that there were questions to be answered and that we had almost no good monitoring data for Massachusetts to give us any clue as to what was important. It was understood that after the outfall went on-line, we would come back to revisit the monitoring plan to see if the frequency and spatial distribution were correct.

P. Borrelli agreed that the monitoring should continue. He liked J. Pederson's comment about the big picture. Just as how we are concerned about what comes out of the pipe, we should be concerned about what goes in it. A better understanding of what the source is would be good. MWRA has the responsibility to run the outfall and see what it can do about source reduction and public education.

S. Nixon said that since the evidence is showing us that bioaccumulation is not a big problem he will vote against continuing the mussel monitoring. N. Jaworski does not want to stop the mussel monitoring because even though the values were not high, PAH and chlordane in mussel tissue did increase from the previous year.

B. Berman asked about the high PAHs values in mussel tissue from in the Boston Inner Harbor. S. Rhode replied that the ratio of the high molecular weight to the low molecular weight indicates that the source is road runoff of combusted fuels (i.e. soot) so it seems unlikely that it is the oil operations in Chelsea Creek. B. Berman asked if the Inner Harbor farfield monitoring is required in the permit. A.

Rex replied no, only the nearfield is required in the permit. MWRA is monitoring in the harbor to measure changes.

A. Solow does not think it seems right to stop monitoring for something that has a threshold after only one year. Perhaps the threshold needs to be revised, but we will monitor next year to see if there is evidence of a trend or if this was an anomaly.

J. Shine asked what would happen if this threshold were exceeded again. A. Rex replied that this is a caution threshold which means something is different from the way it was before, and we notify everyone. What we try to do is look to see if it meant anything, and in this case, we do not think it does, except that perhaps we set our threshold too conservatively. If this happens again next year, we will not go through this same calculation exercise.

ACTION: OMSAP recommends that MWRA continue mussel monitoring using the same protocol as summer 2001 (7 members yes, 1 member no).

MODEL EVALUATION GROUP UPDATE

B. Beardsley described the recent activities of the Model Evaluation Group (MEG). The MEG was established a few years ago by OMSAP to review a report by HydroQual, "the Bays Eutrophication Modeling Analysis for the period 1992-1994". Members are: Bob Beardsley (WHOI), Eric Adams (MIT), Jeff Cornwell (U. Maryland), Don Harleman (MIT), Jack Kelly (EPA), Jay O'Reilly (NMFS), and John Paul (EPA). The group meets periodically to review modeling reports and evaluate model performance. The Bays Eutrophication Model consists of two models, the hydrodynamic model (predicts the flow and dilution of the effluent in the bays) and the water quality model (predicts parameters such as dissolved oxygen).

B. Beardsley reviewed the recent modeling reports and MEG review [for details see March 2002 MEG report]. The MEG met in March 2002 to review the following reports:

- Calibration of the Massachusetts and Cape Cod Bays Hydrodynamic Model: 1998-1999. Report 2001-12.
- Addendum to "Bays Eutrophication Model (BEM): modeling analysis for the period of 1992-1994." Report 2001-13.
- Boundary sensitivity analysis for the Bays Eutrophication Model (BEM). Report 2001-14.
- Analysis of the addition of a third algal group to the Bays Eutrophication Model (BEM) kinetics. Report 2001-15.

B. Beardsley then summarized the March 2002 meeting and listed MEG's recommendations:

- The hydrodynamic model results for 1998-99 are acceptable. HydroQual should run the water quality model for this period with both two and three algal groups.
- Additional documentation about the HydroQual hydrodynamic model should be added to the "Calibration of the Hydrodynamic Model: 1998-1999" report, so that this report when completed will be the definitive report on this model.
- The higher resolution hydrodynamic model grid should be used in all new runs.
- All water quality runs should be done with the same spatial resolution as the hydrodynamic model.
- Recent plume tracking data should be compared to hydrodynamic model predictions.
- Recent ideas about how to embed a more accurate nearfield model should be considered.

- There is a need for more *in situ* water quality measurements.
- The MEG would like to encourage that interesting events captured by the model be analyzed further, written up, and published because it would provide much more understanding about how the bays work. It also would give the program much higher visibility.

S. Nixon asked if it is safe to assume that the bottom water dissolved oxygen in the nearfield around the diffusers is influenced by the dissolved inorganic nitrogen input across that northern boundary. B. Beardsley replied yes. S. Nixon asked if there has been any systematic comparison of the Chesapeake, Long Island Sound, and Mass Bay water quality models. He knows they all have a common history with HydroQual. He wondered how different the natural world is in these three systems according to the models. B. Beardsley replied that part of the objective of one of the reports was to try to have what has been used in the Mass Bay modeling documented thoroughly so that one could compare it to other models. His qualitative impression is that the Mass Bay model is not different from Long Island Sound. He does not think there is a significant difference in the coefficients in the models. M. Mickelson thought that was correct but will look into it further [the principal difference is in the algal kinetics and stoichiometry]. S. Nixon thinks certainly it would be interesting to compare the models. He noted that the Chesapeake Bay model was not well documented. S. Nixon then asked if MEG will continue its work. A. Solow assumed that the MEG would continue. MWRA agreed. J. Pederson thinks that it is important that the MEG continue to review the model output.

S. Nixon said that this model was put together to make some predictions about the impact of a specific management action, the construction and operation of the outfall. It was used for that purpose and decisions were made. It was shown to agree to some degree with the monitoring data and that database grew over time. The question is, does MWRA still need the model since it has a very extensive monitoring program.

N. Jaworski thinks that having the model is like an insurance policy. For example, if there is a debate for adding nitrogen treatment, the model can show the large contribution of nitrogen from the northern boundary. A. Solow also thinks there is a value if there is an effect seen in the monitoring data and there is a question about the cause. The model could be used to test competing hypotheses, for example, was what happened at the boundary sufficient to explain what happened at the outfall. J. Shine agreed.

S. Nixon thinks this is an extremely valuable scientific resource for the state of Massachusetts and he would argue that MWRA having built it, funded it, developed it, and verified it, the model should now be taken care of by the state and used as a community based model so all the institutions could use it. A. Rex replied that making the model available to the research community this was one of MWRA's goals when the model was transferred to U Mass Boston, but MWRA has to maintain and run the model according to the permit. K. Keay said that U Mass Boston understands that there will be a scientific oversight body, either the current MEG, or something similar to it, that they would be discussing model runs and preparing reports for them.

S. Nixon thinks that the critical part of a successful modeling program (and monitoring) is to have this independent review. It is important that the MEG maintain independence and someone has to always bring skepticism to the process. He suggested rotating the MEG membership. Also, the hydrodynamic model is very good but perhaps less attention has been focused on the water quality model. A specific review of the water quality model may be needed. B. Beardsley agreed about the issue of

independence. However, MEG members such as Don Harleman have added a healthy dose of skepticism.

ACTION: OMSAP accepted the March 2002 MEG report. OMSAP recommends the following: MEG continue its review of the model output; MEG membership should rotate; and that MWRA develop a plan to maximize the use of this model (e.g. publishing in peer-reviewed journals and graduate thesis work).

DEVELOPMENT OF PROCESS FOR REVIEW OF AND MODIFICATIONS TO MWRA'S EFFLUENT OUTFALL AMBIENT MONITORING PLAN

A. Rex stated that MWRA has conducted almost 10 years of monitoring in Mass Bay and in a few months, we will complete our second year of post-discharge monitoring. We think it is appropriate right now because the nature of the questions is changing. Initially the question was, what are the acute impacts of the outfall, if any. Now we need to think of a Monitoring Plan that looks at more long-term impacts and can be supported over the longer term. She pointed out that there is a process outlined in the permit for reviewing and changing the Monitoring Plan.

The Monitoring Plan was developed over a two-year period in a very structured way. It included input from regulators, scientists, and the public. We would suggest that modifications be considered in a similar structure and fashion. We are very interested in improving the focus and efficiency of this program and we are also interested in the cost. The cost of this program has increased dramatically over the past few years and right now we are spending about $4 million a year on monitoring and modeling. She suggested that OMSAP convene a subcommittee that will look at reviewing the Monitoring Plan and making modifications to the Monitoring Plan over course of the next couple of years or so. She would like to hear discussion on that.

S. Nixon agreed with A. Rex, with the amount of data that have been collected, it is time to see if MWRA can monitor smarter and more efficiently than in the past. This is not a trivial task. The data need to be analyzed and that is beyond what OMSAP is able to do. Perhaps some resources can be given to Battelle present recommendations, suggestions, and their analyses for modifications to the Monitoring Plan. A. Rex was not implying that OMSAP was to analyze raw data; she would like OMSAP to agree on a structure for making the review happen. She also would like to see more active participation on the part of the agencies and the public throughout this process.

J. Shine thinks that MWRA could reduce the number of nearfield stations without losing statistical power because the stations are so close together. N. Jaworski agreed that the review of 10 years of monitoring is not a trivial task. He asked how the bulk of the work would be done. A. Rex replied that MWRA and its contractors would do most of the work but they would like to do it in the context of interaction with the OMSAP. She thinks it would be useful to have an OMSAP meeting this summer to get started.

J. Pederson noted that page 3 of MWRA's information briefing outlines the process that was followed from the NRC book "Managing Troubled Waters". When the Monitoring Plan was first developed, the questions were primarily those raised by the public. The scientists worked on deciding how to answer those questions. This is a good time to go back and look at those questions.

A. Solow noted that though there have been 10 years of monitoring, there have only been 2 years of post-relocation monitoring. Also, it has always been his feeling that one would want to compromise

on the spatial coverage and capitalize on the temporal coverage and so it is better to look, especially for subtler effects over time. So that is a compromise to make here between tight spatial coverage and longer time sampling. C. Hunt said that he would like to see everyone working as a group, where MWRA and their consultants present analyses, then OMSAP, the regulators, and the public work together. He thinks it is important to lay out this process before the review begins. B. Beardsley thinks it is important to document changes to the monitoring program. A. Rex thinks the best place to start for this review is an overview of exactly what MWRA does. She asked OMSAP what they thought about convening a subcommittee for the review. S. Nixon thought OMSAP was small enough that a subcommittee was not needed. He thinks it would be useful to meet this summer. B. Berman pointed out that it will be important to have the public involved throughout this process.

ACTION: OMSAP will convene during summer 2002 to begin discussions on developing a process for the review of the Monitoring Plan.

ADJOURNED

MEETING HANDOUTS:

- Agenda
- April 2002 OMSAP/PIAC/IAAC membership lists
- October 2001 draft OMSAP minutes
- MWRA information briefings and copies of presentations

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.