

Outfall Monitoring Science Advisory Panel (OMSAP) Meeting

Thursday, November 16, 2000, 10:00 AM - 2:00 PM

Boston, MA

FINAL MINUTES

ATTENDANCE

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

Observers: Bruce Berman, Save the Harbor/Save the Bay; Peter Borrelli, Center for Coastal Studies; Mike Bothner, USGS; Brad Butman, USGS; Margaret Callanan, Cape Cod Commission; Cathy Coniaris, OMSAP staff; Mike Delaney, MWRA; David Dow, NMFS; Marianne Farrington, New England Aquarium; Patricia Foley, Save the Harbor/Save the Bay; Maggie Geist, Association for the Preservation of Cape Cod; Barbara Hecker, Hecker Environmental; Carlton Hunt, Battelle; Russell Isaac, MADEP; Chris John, MWRA; Ken Keay, MWRA; Christian Krahorst, MCZM; Matt Liebman, EPA; John Lipman, Cape Cod Commission; Steve Lipman, MADEP; Mike Mickelson, MWRA; Dale Miller, Normandeau Associates; Katherine O'Meara, Wastewater Advisory Committee; Andrea Rex, MWRA; Larry Schafer, retired; Rich Signell, USGS; Dave Taylor, MWRA; Sal Testaverde, NMFS; and Jeff Turner, UMass Dartmouth.

SUMMARY OF ACTION ITEMS & RECOMMENDATIONS

1. June 21, 2000 minutes were approved with no amendments.
2. OMSAP voted unanimously to recommend that EPA/MADEP modify the MWRA pH permit language to: "pH shall not be less than 6.0 or more than 9.0 unless these values are exceeded due to natural causes or as a result of approved treatment processes and provided the effluent does not cause a violation of water quality standards for the receiving water."
3. OMSAP approved of MWRA's approach to analyzing the zooplankton data. They requested that adult and copepodite stages of zooplankton be kept separate in the analyses and not grouped together. They also urged MWRA to keep in mind the hypotheses and goals of the study to help guide the analyses.

MINUTES

A. Solow welcomed Dr. Juanita Urban-Rich from U Mass Boston to OMSAP. She is a plankton ecologist with an interest in zooplankton. J. Urban-Rich thanked A. Solow and added that she studies zooplankton, primarily looking at carbon and nutrient cycling, grazing, and fecal pellets.

JUNE 2000 MINUTES

There were no comments on the minutes. OMSAP unanimously approved the June 2000 minutes with no amendments.

GENERAL UPDATE ON PLANT PERFORMANCE TO DATE RECENT AND UPCOMING PERMIT DELIVERABLES

M. Mickelson reminded everyone that Bob Beardsley (OMSAP) had urged him to pay attention to work in the Gulf of Maine, particularly the Gulf of Maine Ocean Observing System (GoMOOS). M. Mickelson found out that there will be a buoy placed at the entrance to Mass Bay, offshore of Cape Ann. Measurements will include conductivity, temperature, fluorescence, and dissolved oxygen. They measure additional parameters at other locations in the Gulf of Maine. This will provide useful information in the future. He then stated that the treatment plant has been working well, with cBOD (carbonaceous biochemical oxygen demand) and TSS (total suspended solids) values of less than 10 towards the end of October. M. Mickelson then described the types of reports at the MWRA website

[http://www.mwra.state.ma.us/harbor/html/ditp_performance.htm].

DISSOLVED OXYGEN (DO) SATURATION CAUTION EXCEEDANCE

A. Rex described and showed data of the early October exceedance of the caution level for the existing DO percent saturation level in the outfall nearfield and in Stellwagen Basin. DO percent saturation is slightly below the 80% caution level in those areas, however, the actual concentration is well above the class SA standard of 6 mg/L. She then showed how this appears to be a seasonal pattern that has occurred annually in the late summer/fall during the monitoring period 1992-2000. For this reason, the Outfall Monitoring Task Force (OMTF) recommended that this threshold be deleted back in 1997 and OMSAP concurred. MWRA has formally requested the deletion of this threshold in a letter to EPA/MADEP dated October 13, 2000.

J. Shine pointed out that in the absence of any permit modifications, this will occur again next year. S. Nixon asked why after three years after OMTF recommended deletion of the threshold, it still has not been deleted. A. Rex replied that the Contingency Plan was “frozen in time” in 1997 when it was attached to the permit, and there was no process for making revisions. There are a lot of changes that need to be reviewed now that the permit is in effect.

J. Shine asked if the DO concentration threshold will remain the same. A. Rex replied, yes, for the time being. EPA is working on finalizing its draft regulations on dissolved oxygen and this may affect MWRA’s threshold. A. Rex thinks that DO percent saturation is a very useful parameter to help understand how the system is working, but not so useful for measuring treatment plant performance or environmental impacts. C. Hunt thinks the low DO percent saturation may be due to slightly warmer bottom temperatures.

D. Dow thought that the change in the DO percent saturation was more likely due to biological activity, not so much due to temperature and salinity changes in the bottom water. A. Solow pointed out that that would also decrease the DO concentration, and that was not measured. D. Dow does not think it is a good idea to delete this threshold because it is useful when trying to learn about what is going on in the system. A. Rex said that even if the DO percent saturation was deleted, it would still be calculated and used for interpreting future data. She pointed out that

the state standards include a phrase with the DO percent saturation “unless due to naturally occurring background conditions”. This phrase did not make it into the Contingency Plan.

A. Solow asked what would happen if the threshold is dropped and then for some reason it kept dropping, or something else unusual occurred. A. Rex replied that MWRA would ask for advice from OMSAP. J. Shine thinks it would be fine to delete the DO percent saturation threshold since the concentration will remain as a threshold. If that decreases, biological activity, temperature, salinity, DO saturation, and other factors will be examined. A. Solow agreed that DO saturation is a supportive parameter. S. Nixon thinks having DO saturation as a threshold is superfluous since DO concentration, the critical parameter, is a threshold. N. Jaworski added that calculating DO saturation is good information, but it does not have to be a threshold.

M. Shiaris asked what would happen if the data went below the DO concentration went below the thresholds. A. Rex said that it would trigger Contingency Plan notification and OMSAP would provide advice. She also pointed out that DO concentration has gone below the thresholds during the baseline period. S. Nixon added that the standard (6.0 mg/L) is based on a freshwater standard. M. Shiaris noted that the DO saturation is based on a calculation using the DO concentration which is also a threshold.

REQUEST FOR CHANGE IN EFFLUENT pH PERMIT LANGUAGE

M. Mickelson showed influent and effluent pH data over time (1998 to present). The influent pH exhibits a pattern over time, and MWRA is currently investigating this pattern which may be partially due to drinking water corrosion control. He noted that the effluent is almost 0.2 of a pH lower than the influent. This can easily be explained by high effluent carbon dioxide (CO₂). Pure oxygen is injected into the secondary reactors increasing the production of CO₂ by bacteria. Equilibrating this increase in CO₂ with the atmosphere and taking alkalinity into account to calculate pH corresponds to what is measured in the effluent. This evidence suggests the decrease in pH is due to the CO₂ rather than the production of acetic acid or addition of other acids. However, MWRA plans to further confirm this.

N. Jaworski asked if the effluent is being nitrified. M. Mickelson replied no. He then described the pH permit language being proposed by MWRA.

Current language:

“The pH of the discharge shall not be less than 6.0 nor greater than 9.0 at any time to meet the criteria of 6.5 to 8.5 in the receiving water, and shall not change the pH of the receiving water more than 0.2 standard units outside of the normally occurring pH range. There shall be no change from background conditions that would impair any use assigned to class SA waters, unless the cause of the excursion from criteria is due solely to naturally occurring background conditions.” (from MWRA NPDES permit, section I.1.c)

Proposed language:

“The effluent values for pH shall be maintained within the limits of 6.0 to 9.0 unless the publicly owned treatment works demonstrates that: (1) inorganic chemicals are not added to the waste stream as part of the treatment process; and (2) contributions from industrial sources do not cause the pH of the effluent to be less than 6.0 or greater than 9.0.” (from federal regulations)

40CFR133)

or

“pH shall not be less than 6.0 or more than 9.0 unless these values are exceeded due to natural causes or as a result of approved treatment processes and provided the effluent does not cause a violation of water quality standards for the receiving water.” (from other discharge permits)

M. Mickelson asked OMSAP if they would recommend adding some flexibility to the current permit language to allow MWRA to reduce the pH relative to the influent due to the approved secondary treatment process.

N. Jaworski asked about acid rain deposition and corrosion control of the drinking water. M. Mickelson replied that sodium carbonate and CO₂ are added to drinking water to reduce corrosion. N. Jaworski and M. Mickelson then had a brief discussion about Quabbin Reservoir water quality and atmospheric nitrate and sulfate deposition. J. Shine pointed out that regardless of the pH at the reservoir, MWRA increases the pH to 8-9 to reduce pipe corrosion. He thought that the group should discuss whether decreasing the pH by 0.2 below 6.0 is due to an “approved treatment process”.

A. Solow asked if MWRA is confident that they can determine whether an exceedance would be due to an approved treatment process. M. Mickelson replied that MWRA can confirm the cause of a decrease in pH using air equilibration. He asked if EPA and MADEP need more guidance on this issue. R. Isaac thought that the state water quality standard focuses on the receiving waters, i.e. that the pH does not change significantly in Mass Bay. If travel through the outfall tunnel and mixing are considered, the pH is probably not going to change very much.

J. Pederson asked where MWRA is measuring the effluent pH. M. Mickelson replied that the pH measurements are taken at the end of the sample loop that simulates the 850’ distance in the tunnel where the sodium bisulfate (dechlorination) is added. This sample loop is a sealed tube that does not allow the CO₂ to escape. M. Delaney added that the sample loop collects effluent at the beginning of the disinfection basin. However, the effluent actually flows for about an hour through the open-air disinfection basin where the CO₂ can dissipate. Samples taken by MWRA indicate that the effluent is 0.4 pH higher at the end of the disinfection basin compared to the start of the disinfection basin. The purpose of the closed sample loop is to simulate the distance in the tunnel where the sodium bisulfate is added so that it can be dosed accurately.

S. Nixon asked if the monitoring program has measured any evidence that there is a significant impact on the pH in the receiving water. M. Delaney replied no. MWRA has also made calculations using effluent pH and alkalinity, Mass Bay pH and alkalinity, and dilution at the outfall. The pH at the diffusers would decrease by 0.2 with a 50:1 dilution (initial turbulent dilution under the worst-case scenario flow) and an alkalinity of 2.5 milliequivalents per liter.

ACTION: OMSAP voted unanimously to recommend that EPA/MADEP modify the MWRA pH permit language to: “pH shall not be less than 6.0 or more than 9.0 unless these values are exceeded due to natural causes or as a result of approved treatment processes and provided the effluent does not cause a violation of water quality standards for the receiving water.”

UPDATE ON MWRA'S NPDES PERMIT AND PROPOSED CONTINGENCY PLAN MODIFICATIONS

M. Liebman gave a brief update on the proposed modifications to the Contingency Plan (CP) dated November 1997. It was not until the permit was issued in August 2000 that a process was set up to allow revisions to the CP, which is understood to be an evolving document, as better scientific information becomes available. MWRA has developed several proposed revisions over the last three years and OMSAP has deliberated and provided recommendations on them. EPA/MADEP received the official letter from MWRA dated October 13, 2000. The issues that specifically relate to the CP are dissolved oxygen percent saturation, floatables, benthic diversity, zooplankton, and nuisance algae species. MWRA's food web model scope of work is also being evaluated. OMSAP deliberations and recommendations are playing an important role in this evaluation.

UPDATE ON PRELIMINARY RESULTS OF DISCHARGE SURVEYS IN BOTH HARBOR AND BAY

M. Mickelson described the September 29, 2000 bypass incident at the Deer Island Treatment Plant and the corrective actions that are being implemented [for information, see Nov. 2000 PIAC minutes]. He then described measures underway to improve floatables removal and a possible surveillance system to monitor floatables in the disinfection basin.

S. Testaverde asked if sampling on September 29th actually captured the event. M. Delaney replied that there was sampling of the final effluent before, during, and after the bypass event. S. Testaverde asked if the effluent will be very turbid (as seen on the 29th) each time there is a large storm. M. Delaney replied no, because on that day, mixed liquor, the waste activated sludge from the secondary reactors, had draining into the effluent channel, increasing turbidity. A. Rex added that effluent is more diluted during storms.

S. Testaverde asked if the data presented were individual samples or 24-composites. M. Delaney replied that the notification letter included data from the 11:09 AM and 1:37 PM bacteria grab samples. At 11:00 AM, total suspended solids were measured at 28 mg/L, 2.5 times higher than the usual 24-hour composite. However, this still meets the weekly maximum permit limit of 45 mg/L (there is no daily maximum).

THE HARBOR

D. Taylor described recent observations in Boston Harbor since the transfer of the Deer Island discharges to Mass Bay on September 6, 2000. In the northern part of the harbor, there has been a decrease in dissolved inorganic nitrogen (DIN) concentrations. The decrease is more apparent in the ammonia data. There seems to be a decrease in the southern harbor, although it may be too early to discern patterns. He compared the Fall 2000 decrease in the southern harbor to 1998 when the Nut Island flows were transferred to Deer Island.

R. Signell thought that 1999 DIN in the southern harbor would be noticeably lower than the previous years, but this is not apparent in the data. D. Taylor replied that there other factors affecting the DIN concentrations. S. Nixon added that it could be due to a relaxation of the biological demand. D. Taylor agreed that there is a very strong metabolic signal in the harbor. Jack Kelly had noticed this several years ago, that despite the rapid flushing of the harbor, there

was a very strong metabolic signal. During summer, nitrogen occurred mainly in the particulate form and in the winter, as dissolved inorganic nitrogen. MWRA has been observing a long-term increase in the DIN in the harbor during winter that appears to be positively correlated with an increase in minimum winter water temperatures. As the harbor is warming, there is presumably, increased mineralization and increased buildup of ammonium in the harbor.

S. Nixon asked what D. Taylor thought the “memory” of the harbor is. D. Taylor predicted that DIN will decrease. S. Nixon noted that some feel that there is a build-up in the sediments and it will take years before a significant decrease is seen. D. Taylor agreed and pointed out that the “memory” is seen when storms resuspend bottom sediment causing periods of poor water quality. J. Shine added that the amphipods are already working a lot of the sediment. S. Nixon predicted that chlorophyll and nitrogen will be lower next summer, with significant improvements in water quality. He thinks the sediments will recover quickly.

D. Taylor then showed somewhat increasing secchi depth data from the northern and southern parts of the harbor. S. Nixon asked why MWRA still uses a secchi disk when better technology has been available for decades. D. Taylor replied that it is a measure that the public understands. MWRA also uses a transpiezometer to measure vertical light PAR attenuation. A. Rex said that they find the secchi data useful for comparison to old data. S. Nixon agreed that was a good reason.

D. Taylor showed total suspended solids data and stated that changes are not being observed. He then showed chlorophyll data. There does not appear to be a significant decrease in chlorophyll in either region within the harbor. There has been a historic debate about whether phytoplankton are light or nutrient limited and it appears that with the transfer offshore, water clarity is increasing, stimulating the phytoplankton growth in the harbor and compensating any decrease as a result of the reduction of nitrogen loading to the region. Another explanation is the large, ctenophores bloom within the harbor that may be grazing the zooplankton, allowing the phytoplankton populations to build up.

S. Nixon thinks that the present conditions in the harbor should not compare to the 1998 transfer of Nut Island flows to Deer Island because that only moved the discharge within the harbor. Now the flows have been moved offshore. D. Taylor agreed.

R. Signell asked why there was not a significant change in total suspended solids even though there was an increase in secchi disk depth. S. Nixon replied that in coastal areas, the total suspended solids measurement does not correlate well with light penetration. D. Taylor agreed and added that dissolved organic material affects water quality more than total suspended solids.

J. Shine asked about changes in salinity in the harbor since the outfall was relocated to Mass Bay. D. Taylor replied that he has not looked at the data yet. Past sampling at the harbor outfall discharge where the wastewater plume reached the surface indicated very rapid mixing of the wastewater, even within the harbor, and the salinity signal was only one part per thousand. Thus a large change in salinity is not expected. S. Nixon noted that MWRA has a great data set to work with.

THE BAY

C. Hunt described preliminary results of monitoring in Mass Bay and Cape Cod Bay from September 1 to October 24, 2000. He showed salinity and density data that indicated stratification and no significant change in the salinity of the system. The plume can be discerned from the salinity data, but only close to the diffusers because of the rapid mixing.

L. Schafer thought the preliminary prevailing current was southward. C. Hunt replied that instantaneous or daily currents can go in any direction but the long-term net drift is to the south. On Sept. 28th, there was a much more extensive survey tow-going back and forth on either side of the diffuser line, coming in very close to the diffusers. He showed data results including a salinity deviation as expected from the plume as they passed over a diffuser, through the hydraulic mixing zone. There was also a slight increase in beam attenuation, temperature and, sigma-t. He then showed temperature-salinity plots and how they determine background salinity the system is mixing into, as well as evidence of the plume. Using flow at the treatment plant, tides, winds, and the salinity of the effluent and Mass Bay, they estimated that the diffuser is working as predicted, with rapid dilution of at least 100:1.

C. Hunt then noted that bottom water dissolved oxygen concentrations also seemed unaffected by the outfall. Values were typical for the fall at 7.0-7.5 mg/L. He then showed satellite imagery of a regional chlorophyll bloom from September-October. Initial phytoplankton counts show a healthy, robust and diversified diatom bloom in September. The satellite indicated that there was a reduction in the system-wide bloom in October. He also pointed out that *Phaeocystis* occurred in spring of 2000 and so it seems to be repeating on a 3-4 year cycle. He then presented DIN and ammonia data. The ammonia data indicate that the plume is flowing somewhat to the north on this survey day and concentrations are within range of predicted values. C. Hunt noted that there is the ctenophore bloom seen in Boston Harbor and Buzzards Bay is not as prominent offshore, but it may be influencing some of the measured chlorophyll response. J. Pederson asked about temperatures during the ctenophore bloom. C. Hunt replied they appeared normal. J. Pederson thought that the ctenophore was common in the harbor. J. Turner responded that this is the first time they have seen appreciable numbers in MWRA monitoring, whereas it is typically very abundant in enclosed embayments such as Narragansett Bay and New Bedford Harbor in the summertime.

M. Liebman asked about the chlorophyll bloom in western Mass Bay that began in the fall of 1999. C. Hunt said that the chlorophyll has been quite variable but elevated since late 1998 and may have something to do with the transfer of flows from Nut Island to Deer Island. M. Liebman asked if there is an explanation for this. C. Hunt thinks it may be due to an increase in the discharge of ammonium from Deer Island and increased light penetration due to better treatment of effluent.

S. Nixon asked how sure they are that the apparent increase is due to the relocation of the Nut Island discharge. C. Hunt said that this will be examined further. S. Nixon urged caution in attributing an increase in chlorophyll to the diversion of the Nut Island flows, unless there is compelling evidence.

R. Signell asked if the dilutions in Cape Cod were calculated. C. Hunt replied that they have not been calculated yet, but guessed that they were probably around 500:1. R. Signell thought that was consistent with the model predictions.

N. Jaworski asked if they have looked at the total phosphorus data yet since it could prove to be a good tracer. C. Hunt replied that they have not but will soon examine all of the measured parameters. He is glad that the results so far indicate no surprises in Mass Bay.

PROGRESS ON OMSAP-REQUESTED ZOOPLANKTON ANALYSES, PROPOSED ANALYTICAL DIRECTION AND SPECIES LIST, MWRA STAFF

K. Key stated that at the February 2000 meeting, OMSAP recommended that MWRA present a plan to OMSAP in Fall 2000 for analyzing the zooplankton (ZP) using a system-wide approach. This plan should pay particular attention to the understanding that the Mass Bay (MB)/Cape Cod Bay (CCB) system flows like a conveyor belt from north to south. Data should be examined temporally and spatially to contrast differences in the system. For example, there may be no concern if changes were seen in CCB concurrent with changes in the northern boundary system. However, an alarm may be raised if changes in CCB did not seem to be related to anything entering the system. The first step in the plan is to review the baseline ZP data (1992-2000). He then briefly reviewed previous MWRA zooplankton reports. MWRA will also review and summarize the understanding of the biology of ZP species in the bays. Then MWRA will analyze the entire baseline dataset using a multivariate approach to test differences in community composition and abundance. This will be an exploratory data analysis starting with clustering and principle components analysis to hopefully identify characteristic station and species groups, and compare them to past interpretations. The last set of analyses that planned are multivariable correlation and regression analyses to see if changes in the abundance of *Acartia* in the nearfield (NF) can be correlated to run-off, rainfall, and other climate forcing functions. He then showed the station locations and described the analyses in detail. He noted that Dr. Jeff Turner has finished counting the final baseline ZP samples and results will be available shortly. They have reviewed the baseline and developed a good species list in terms of what has been consistently identified through time. He asked OMSAP if this sounds like a reasonable approach given the questions that they asked MWRA to address.

J. Shine asked about the clustering analyses. K. Key replied that it will help to identify more quantitatively the similarities in the offshore community and provide information to verify that the CCB communities resemble the source of the ZP in the Gulf of Maine. If the analyses do not show that, then perhaps the conveyor belt hypothesis needs a closer look, or the data needs to be looked at differently. This is an exploratory analysis of the baseline data and the results will help in thinking about what sort of change could be outfall related, were they to be seen. J. Shine wondered how a caution and warning threshold could ever be developed from this information. K. Key could not give an answer to that until the analysis is carried out and evaluated.

D. Dow asked if the clusters will be based on annual averages for the different years. K. Key replied no, they will be based on the ZP samples collected every year. Each sample will be treated independently. D. Dow asked if there will be a separate cluster for each time period. K. Key replied no, not necessarily. MWRA will be examining whether there is seasonal signature in the clustering. D. Dow would be interested in learning whether the clusters change seasonally

in relation to where the right whales feed or are distributed. K. Keay agreed and said that with this type of analysis, they can focus on the winter/spring and see whether the CCB stations are clustering very strongly with the boundary stations.

A. Solow thinks that the first step is to confirm the conveyor belt theory. If it is confirmed, then predictions could be made on what to expect in the ZP community in CCB based on what is measured at the northern boundary of Mass Bay and then those predictions could be tested. He wondered if cluster analysis was the right approach. K. Keay described several analyses MWRA will undertake to examine the ZP community.

A. Solow approved of the plan. He wondered what would happen if the conveyor belt hypothesis was confirmed, and a northern community was identified, but the population naturally changed by the time it was transported to Cape Cod Bay. In this case, there would be a good prediction of what to expect but the results might look quite different. K. Keay thinks one way to address that concern is to pay special attention to the monitoring results. C. Hunt added that Rocky Geyer will be examining the physics in the system and this will hopefully provide important information. J. Pederson agreed with A. Solow's concern and wondered how comprehensive the MWRA ZP species list is. J. Turner said that the list is comprehensive, only excluding the meroplankton.

J. Urban-Rich asked if the ZP are fairly evenly distributed through the entire area. J. Turner replied yes, with the exception of the two *Acartia* species that are found in higher numbers in Boston Harbor. J. Urban-Rich asked if all of the stages of ZP will be used in the analyses, i.e. copepodite I to adults. J. Turner replied that adult male and female copepods are identified to species and copepodites are identified to genus. He then described the sample analysis. Copepodites have not been staged to each one of the 6 stages, but they have been distinguished from the adult males and females. He also noted that samples are archived for future re-analysis, if needed.

P. Borrelli asked how the proposed analysis addresses ZP patch formation in the south. K. Keay replied that it does not because the MWRA monitoring does not sample the ZP patches. P. Borrelli noted that this means that MWRA will have abundance, but not distribution.

A. Solow asked why the copepodites will not be kept separate from adults for all of the analyses since a difference in the age distribution, from the north to the south is predicted if there is a conveyor. K. Keay thinks they could keep the copepodites of *Calanus* and some of the other taxa separate in the analyses.

J. Urban-Rich pointed out that even if the adults and total copepodites numbers are lumped, this may affect the cluster analysis because there may be more copepodites than adults. She suggested that it would be useful to know exactly how the stages are separated, instead of lumping all of the data. K. Keay thought it would be possible to include in the analyses the copepodite and the adult forms as separate taxa and said that they will take another look at the species list with this in mind.

M. Farrington said that the approach depends on what question is being asked, i.e. is there a conveyor belt; does the outfall have a change on the ZP community. K. Keay agreed and said this approach will help shed some light on the conveyor belt question by perhaps providing another tool to look at changes between the north and the south parts of the system. A. Solow feels that there is important information in the age structure of the ZP. OMSAP continued to discuss ZP dynamics and the proposed study plan.

A. Solow then asked OMSAP if they approved of the plan. J. Shine approved with the plan as long as the life stages are considered separately and not grouped together.

M. Shiaris thought it might be useful to be more hypothesis-driven in terms of focusing on how many stations, and exactly what is going to test the hypothesis.

J. Urban-Rich suggested a literature review on ZP genetics in this area before the cluster analysis is done. J. Turner summarized genetic work that would relate to this. It appears that the *Calanus* and *Pseudocalanus* populations are the same for the Gulf of Maine. There are no genetically distinct populations. J. Urban-Rich said to go ahead with the clustering then.

ACTION: OMSAP approved of MWRA's approach to analyzing the zooplankton data. They requested that adult and copepodite stages of zooplankton be kept separate in the analyses and not grouped together. They also urged MWRA to keep in mind the hypotheses and goals of the study to help guide the analyses.

EPA LISTSERVER

M. Liebman asked if people received the DO percent saturation exceedance listserver message. If anyone would like to subscribe, send their email address to liebman.matt@epa.gov.

ANIMATION OF CURRENTS

R. Signell showed a computer animation of currents from the USGS long term monitoring station about 2 km SSE of the outfall. This animation gives a feeling of the kind of temporal variability of the currents and also the structure of the currents from top to bottom. Sometimes it looks like the whole water column is going in the same direction, even when it is strongly stratified, which seems unusual, and at other times even when it is not stratified, there is a lot of shear. So it is very complicated. This data from July shows that there is no prevailing current and very complicated top to bottom structure. This is not something a model can predict on a day to day basis.

S. Nixon asked if it is common that the highest lateral flow at the pycnocline rather than above or below. R. Signell replied that it is not. The winds and the currents are not very well correlated at this location. R. Isaac asked what this information provides. R. Signell replied that this information will aid the interpretation and evaluation of the plume tracking data.

C. Hunt asked when the USGS data will be available. R. Signell replied that this data (through the end of September 2000) is available now. B. Butman added that the fall data will be available in February 2001. C. Hunt asked when the data will be interpreted. R. Signell replied that USGS can set up a mechanism for Battelle to receive information as quickly as possible so that it can be used for the plume tracking. He has looked at data three weeks pre-discharge and three weeks

post-discharge, examined the average currents and found that there was too much natural variability in the system to notice any changes in the currents due to the outfall. There will have to be a much longer averaging period to discern any outfall effects.

UPDATE ON CONTINGENCY PLAN EMERGENCY SIMULATION PLAN AND DRY RUN FOR RED TIDE/CHLORINATION EXCEEDANCES, MWRA STAFF

M. Mickelson stated that the permit requires MWRA to conduct a dry run of two threshold exceedances, a chlorination upset and a red tide exceedance. The dry run was conducted in August 2000. MWRA will be drafting a final report shortly. The most important result was the development of a notification list. He asked OMSAP for some guidance on how they would deal with the notification of an exceedance. J. Shine thinks that the decision to meet should be based on the severity of the exceedance. A. Solow agreed that it would be a judgement call on a case-by-case basis. N. Jaworski added that conference calling was always an option. C. Coniaris pointed out that a conference call would be open to the public, they could come to a meeting room and listen in.

B. Berman anticipates that there will be exceedances in the future and the press will contact members of PIAC before they ever call OMSAP. That said, it might be sensible to have a system of rapid PIAC notification set-up, at least involving the chair.

REVISED PLUME TRACKING STUDY DESIGN, CARLTON HUNT

C. Hunt described the plume tracking study design that has been revised based on input from peer review. The purpose of the plume tracking is to field test and certify whether the minimum dilution of the outfall is equal to or greater than the minimum dilution specified in the permit. [Plume tracking study design is located at: <http://www.mwra.state.ma.us/harbor/enquad/pdf/ms-058.pdf>]. Two surveys are planned. In March 2001 (unstratified conditions) there will be “shakedown” survey to test the dye addition protocols. The July 2001 survey (stratified conditions) will be the actual dilution certification survey.

Before the March survey, there will be laboratory studies to evaluate dye interference and degradation. Right before the surveys, turbidity and background fluorescence will be measured. For both surveys, non-toxic Rhodamine dye will be added at the Deer Island Treatment Plant, measured in the effluent, and tracked in the field. In addition, other parameters will also be measured including temperature, salinity, coliform, ammonia, phosphate, chlorine, chloride, suspended solids, silver, and copper.

J. Shine asked how instantaneous the flow measurements from the plant are to adjust the dye addition. C. Hunt replied that flow measurements will be in real time and explained the procedure. He then showed where the dye will be added. J. Pederson asked how much dye will be used. K. Key replied three 55-gallon drums for a 28-hour experiment. J. Shine asked what the toxicity of Rhodamine dye is. M. Mickelson replied that the levels that will be discharged would be drinkable. N. Jaworski noted that in his experience, the amount of dye to add is often underestimated. C. Hunt said that they are well aware of this issue and will have plenty available. They have already conducted dye studies with harbor discharge #5 so they have a good idea of how much will be needed. C. Hunt then detailed the survey sampling plans.

N. Jaworki asked if the duration of the study will be long enough to reach steady-state or if this will be a batch addition. C. Hunt replied that steady-state will be reached in about two hours. M. Mickelson noted that steady-state will be reached in the nearfield but it will be considered batch addition in the farfield. C. Hunt added that during those two hours, the ship will be towing instruments to find and define where the plume is and an ADCP [Acoustic Doppler Current Profiler] will be measuring currents in real time. Instruments will also be taking water column profiles and discrete water samples and will be brought as close to the seafloor as possible. He then described details in the sampling procedure.

R. Signell asked what kind of weather conditions can be tolerated for the surveys. C. Hunt replied that it has to be relatively calm to stay within the track lines, however, once the experiment begins, it cannot be halted. He then described the sampling plans and reporting schedule.

A. Solow asked why they are sampling the entire diffuser field, instead of focusing in on a smaller area in greater detail. C. Hunt replied that they are trying to get a more robust set of initial dilution conditions and the best way to do that is to sample the entire loop around the diffusers. The July survey plan may be adjusted based on what is learned during the March survey. C. Hunt then reviewed data shown earlier in the day to outline recent patterns of salinity and temperature that indicate evidence of the effluent plume. Mixing appears rapid, with the hydraulic mixing zone close to the diffusers, within the expected range of 5-20 m.

N. Jaworski asked if the design or operation of the diffusers can ever be changed. He thought one adjustment would be to alter the amount of salt water entrained with the buoyant plume. C. Hunt replied that the physical design cannot be changed, but the operations may be adjusted. M. Mickelson noted that only five of the eight caps were opened on each diffuser but tests (using a small-scale diffuser discharging dye in a tank) showed that opening more caps would not enhance dilution. J. Shine asked if a 500:1 dilution is the best they can measure. C. Hunt thinks they can measure to an even greater dilution, but it depends on the background correction factors.

ADJOURN

MEETING HANDOUTS:

- Agenda
- November 2000 OMSAP/PIAC/IAAC membership lists
- June 2000 draft OMSAP minutes
- MWRA information briefings and copies of overheads: recent monitoring results, dissolved oxygen, effluent pH, zooplankton, plume tracking, and Contingency Plan notification
- MWRA proposed Contingency Plan modifications dated Oct. 13, 2000

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.