

OUTFALL MONITORING SCIENCE ADVISORY PANEL (OMSAP) MEETING

Tuesday, October 16, 2001, 10:00 AM - 2:00 PM

Boston, MA

FINAL MINUTES

ATTENDANCE

Members Present: Andy Solow, WHOI (chair); Bob Kenney, 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 Borucke, MIT; Cathy Coniaris, MADEP; Kelly Coughlin, MWRA; Mike Delaney, MWRA; David Dow, NMFS; Marty Dowgert, USFDA; David Duest, MWRA; Marianne Farrington, New England Aquarium; Patricia Foley, Save the Harbor/Save the Bay; Carlton Hunt, Battelle; Russell Isaac, MADEP; Chris John, MWRA; Ken Key, MWRA; Christian Krahforst, MCZM; Wendy Leo, MWRA; Matt Liebman, EPA; Steve Lipman, MADEP; Rich Masters, Normandeau; Stormy Mayo, Center for Coastal Studies; Mike Mickelson, MWRA; Cornelia Potter, MWRA Advisory Board; Andrea Rex, MWRA; Larry Schafer, retired; Jack Schwartz, MADMF; Sal Testaverde, NMFS; Steve Tucker, Cape Cod Commission; Gordon Wallace, UMass Boston; David Wu, MWRA; and Meng Zhou, UMass Boston.

SUMMARY OF ACTION ITEMS & RECOMMENDATIONS

1. OMSAP approved the April 4, 2001 minutes with no amendments.
2. OMSAP recommends that EPA and MADEP approve of the addition of “unless background conditions are lower” to the DO percent saturation and concentration Contingency Plan thresholds and approved of the approach for calculating the background DO.
3. OMSAP recommends that EPA and MADEP approve of the change in the Alexandrium abundance Contingency Plan threshold to 100 cells/L.

MINUTES

WELCOME & REVIEW OF APRIL 2001 DRAFT MINUTES

ACTION: OMSAP approved the April 4, 2001 minutes with no amendments.

C. Coniaris summarized the morning Inter-Agency Advisory Committee (IAAC) meeting. The committee discussed their mission. S. Testaverde decided to continue with IAAC’s current role of advising OMSAP on regulatory issues. OMSAP may call upon them whenever they feel that it is necessary, and IAAC will convene to discuss issues of interest to the agencies, e.g. DO criteria.

BAYS EUTROPHICATION MODELING SCHEDULE

M. Mickelson outlined the status of the modeling effort and described the four reports that need OMSAP Model Evaluation Group (MEG) review. The first report is an addendum to the 1992-4 modeling. Several years ago, MEG requested that 1992-4 be run to test the models because of interesting features in 1993 (unusual high fall chlorophyll) and 1994 (very low dissolved oxygen). MEG reviewed the model results in March 2001 and recommended that that report have an addendum to document boundary conditions and model equation and that other additional questions be answered.

M. Mickelson then summarized the conclusions of the second report that addresses whether fine grid resolution should be used. The MEG had considered that perhaps the fit for dissolved oxygen (DO) would be improved if a fine grid were used for the water quality modeling. HydroQual reports that the fine grid did not considerably improve fit for DO but it was conceptually more accurate and should be used. Furthermore, it improves resolution in Boston Harbor.

M. Mickelson then described the third report, results of a boundary sensitivity analysis based on a question from MEG on the extent that the nearfield is influenced by the northern boundary. HydroQual found that DO in the nearfield is very sensitive to DO concentrations at the boundary. Nitrogen concentrations are less sensitive because there are other inputs. Cape Cod Bay is less influenced because of its distance from the boundary.

M. Mickelson then reviewed the fourth report that describes the results of HydroQual modeling using a third algal group as requested by MEG. The water quality model has always used two algal groups (winter and summer) that coexisted throughout the year. Algae abundances are varied through the year by changing the ratios of nutrients and boundary conditions. MEG will be asked to review the results. HydroQual will proceed with doing the 1998-1999 water quality modeling. UMass Boston is beginning practice runs of the hydrodynamic and water quality models to smooth the transition from HydroQual. After that, they will begin the post-discharge modeling, beginning with the year 2000. A. Solow asked if HydroQual recommends the incorporation of the third algal species. M. Mickelson replied that it appears to be a level of complexity that may not be needed. The inclusion of a third algal group would increase the modeling effort and cost, and it was not obvious that it was necessary but MEG will review this.

A. Solow asked about the fine grid and DO. M. Mickelson explained that the finer grid actually gave a slightly higher DO than the coarse grid. This was due to numerical dispersion. When HydroQual applied low DO boundary conditions, it moved faster with the coarse grid because the grid sizes are bigger from the boundary to the nearfield. The fine grid has smaller boxes and an input of low DO water at the boundary takes more time to reach the nearfield and thus it has less of an effect.

J. Urban-Rich asked how MWRA decided what years to model. M. Mickelson replied that MEG has in the past recommended what years to run. We actually learn a lot about biology and modeling from model shortcomings. R. Isaac is not sure if the third algal species will give a more complete or confusing picture. M. Mickelson will send R. Isaac a copy of the report on the third algal species. D. Dow asked if using a third algal species would allow a better prediction of the nuisance algal blooms like *Phaeocystis* and *Pseudonitzschia* that seem to occur in late winter/early spring. M. Mickelson replied that the third algal species was developed to try to model the fall of 1993 *Asterionellopsis* bloom. This not a nuisance algae, it just had a high ratio of chlorophyll to carbon. D. Dow wondered if the models could be improved to better predict nuisance algae blooms. M. Mickelson thinks this is an issue that could be brought up at the MEG meeting. J. Pederson asked to be reminded of the MEG membership. C. Coniaris listed the MEG members: Bob Beardsley (chair), Eric Adams, Jeff Cornwell, Don Harleman, Jack Kelly, Jay O'Reilly, and John Paul.

BAYS EUTROPHICATION MODEL'S NEW HOME: U MASS BOSTON

M. Zhou introduced himself and explained that he was recently joined U Mass Boston and is co-funded by MWRA to maintain, enhance, and apply the Massachusetts Bay hydrodynamic and water quality models. The principal investigators in this project are Meng Zhou, Gordon Wallace, and Bernie Gardner. Project goals are to: 1) maintain, enhance and apply the existing Massachusetts Bay hydrodynamic and water quality models; 2) provide model runs to MWRA; and 3) enhance the model by incorporating advances in modeling techniques and oceanographic understanding. M. Zhou then outlined their schedule for setting up their lab and running the models.

M. Shiaris said that M. Mickelson had said earlier that the dissolved oxygen is highly influenced by the boundary. M. Shiaris then asked how that would be taken into account as the model is improved. M. Zhou replied that the code in the model for horizontal mixing can be adjusted. It may be that the code does not currently accurately represent fine scale mixing but we will try to solve that problem. We will also try to improve the plankton components to the model. J. Pederson suggested that UMass have a phytoplankton or zooplankton biologist as part of their advisory group since one of the major purposes of the model was to try to look at the biology. M. Zhou agreed that it is important to combine physics and biology in the modeling and he does have a biology background.

D. Dow commented that he thought the problem with the poor DO fit is due to the fact that there are large scale advective processes in the Gulf of Maine that change the boundary conditions and not actual problems with the model. M. Zhou added that he thinks that the model needs to continue to be improved will be working on ways to better integrate the monitoring and modeling. We also need to see if we can deploy additional moorings at the boundary to collect more information.

M. Mickelson noted that M. Zhou has another project in which he is collecting, processing, mapping, and publishing optical plankton counter data from all over the world. M. Zhou added that this is a project that he has been working on with GLOBEC (GLOBal ocean ECosystems dynamics).

A. Rex welcomed M. Zhou and said that MWRA is excited that they are getting the academic resources of UMass for the modeling. In the past we have used the model as a planning tool. The discharge permit requires MWRA to continue to run the model every year to look for outfall effects and we anticipate the use of the model more as a diagnostic tool to help us understand anything unusual that occurs in Mass bays. She also pointed out that M. Zhou is a zooplankton expert so that should be helpful. G. Wallace added that UMass has made a substantial investment purchasing hardware such as an in situ nutrient analyzer that they plan to attach to GoMOOS' (Gulf of Maine Ocean Observing System) Cape Ann mooring to hopefully provide continuous boundary data.

PLUME TRACKING DYE STUDY UPDATE & REPORTING SCHEDULE

C. Hunt described the plume tracking studies that were conducted in April and July 2001. The discharge permit states that MWRA shall "implement plume tracking, including the use of acoustical technology, to understand the dilution available for the discharge" and "field test and certify whether the outfall's minimum dilution is equal to, or greater than, the predicted minimum dilution specified in the following document, 'Hydraulic Model Study of the Boston Wastewater Outfall II: Environmental Performance', 1993 by Roberts and Snyder". MWRA first conducted an April "shakedown" survey to test the protocols planned for the certification study and develop information on dilution during unstratified conditions (the plan was to have a March survey but there were weather delays, by April, there was some stratification). The purpose of the July survey was to certify the dilution under strongly stratified water column conditions. The primary tracer was Rhodamine WT dye added continuously at the Deer Island Treatment Plant for a measured amount of time.

C. Hunt then described how Battelle addressed various concerns including: temperature calibration of the dye, turbidity, background fluorescence in effluent and seawater, chlorine, bromine, photodegradation, and sensor calibration. Overall, we did not have to worry about all of these potential factors that could affect the data, and so from an interpretive perspective we have a clean set of data from the effluent and offshore, but we had to do a lot of work to get to that conclusion. He then explained how the dye minimum detection limit was calculated.

C. Hunt described the survey plan and results of the April 2001 dye study. Dye was added continuously to the effluent for six hours at ~50 ppb and discrete samples at Deer Island were collected as planned. Dye

addition had to be adjusted to take into account changes in volumes of flow at the treatment plant. They did have to change the location of the dye addition so that there was better mixing before chlorination. Offshore, the following was accomplished: background survey, one segment survey (set of fixed depths sampled in rapid succession over short segments of the diffuser), partial planned hydraulic mixing zone (HMZ) survey, two modified HMZ surveys, discrete sampling, Acoustic Doppler Current Profiler (ADCP) data collection, and farfield survey. Using the ADCP current data, they adjusted the survey plans to make sure they were sampling the plume. The plume was found to be to the NNW of the diffusers. Dilution calculated using the dye concentration data was 105-200 and dilution calculated using the ammonium data was 100-107. Overall, a six hour addition of ~50 ppb dye was enough to easily define the plume field and track it for at least three days.

C. Hunt then described the survey plan and results of the July 2001 certification dye study. Dye was added for six hours at ~84 ppb. Offshore, the following was accomplished: background survey along the diffusers, upstream background sampling, three HMZ surveys, ADCP data collection, 2 days of farfield sampling, and successful survey coordination with EPA and NOAA. The dye emerged from the risers at high slack tide within 10 minutes of the time predicted by the MWRA treatment plant operators. The currents ran ENE parallel to the diffuser for most of the day. The plume broadened and thinned towards the east and was trapped below the pycnocline at about 8-10 meters depth. M. Shiaris said that it almost looked like the effluent is reaching the surface near the diffuser. C. Hunt said that there is some “fluting” in the data; he would have expected the data to be much smoother and are still analyzing the data. M. Liebman said that they had noticed a rebound effect where the effluent flowed up and then rebounded back downwards. B. Berman noted that it also looks like there is some bottom fluting or rebounding. He asked if there is a change of depth along the diffuser line that might explain this. C. Hunt replied that all of the diffusers are at a fairly constant depth of ~35m. M. Mickelson said the fluting is an artifact of the lag in the instruments. A. Solow said that since the fluting tracks the data, it is interpolation error and that can be re-examined. C. Hunt added that it could also be a time lag of the dye cleaning out of the sensors in the tow-yo making the edges of the data not as smooth as they should be. The important point is that the effluent is off the bottom. Overall, it was a very successful survey and we are very confident that we sampled in the initial mixing area.

L. Schafer asked how far south could the plume be potentially measured before the signal was lost. C. Hunt replied that Battelle’s task was to focus on the hydraulic mixing zone so that the dilution of the outfall could be certified, but he will show some farfield results. M. Zhou asked what the tow speed and the circulation rate of the instrument sensor were. C. Hunt replied that the tow speed was ~1 knot vertically and 4 knots horizontally and the sensor was running at ~4 hertz.

C. Hunt then presented the farfield results. The path of the plume was consistent with model predictions, moving southeast overnight and SSW on the second farfield day. The plume stayed just below the pycnocline and there seem to be some fine scale topography influences. They decided not to do the third day of plume tracking because it had become difficult to find and map the plume. C. Hunt then presented a table of data. The dilution based on dye was 91-138 and the dilution based on ammonium was 89-127. As with the April survey, no water quality criteria were exceeded after initial mixing; also, the other potential plume tracers such as phosphate, total suspended solids, chloride (used in salinity-based dilution calculations), copper, silver, and *Enterococcus* were not used to calculate initial dilution because of the variability in the data.

B. Berman asked why the calculation of the dilution from the copper data was so much different than the calculated dilution from the dye and ammonium. C. Hunt replied that it could be due to the variability of copper concentrations in the water samples and the low sample number for copper. The main reason why we collected samples for copper analysis was to see if the water quality criterion was exceeded, and it was not. G. Wallace asked where the background samples were taken. C. Hunt replied that the samples were collected

to the north and east, towards Cape Ann. J. Schwartz asked about the chloride measurements. C. Hunt replied that the MWRA measures chloride because there is some seawater intrusion in the system. C. Hunt then presented Battelle's reporting schedule with their final report scheduled for completion by April 30, 2002.

REVIEW OF MODIFICATIONS TO CONTINGENCY PLAN THRESHOLDS FOR DISSOLVED OXYGEN (DO) & ALEXANDRIUM CELL COUNT

W. Leo described the DO percent saturation and concentration and *Alexandrium* interim threshold revisions that were put in place in summer 2001. The permit states that interim modifications can be introduced at any time during the year and are adopted within 30 days, unless EPA/MADEP object. They then get resubmitted in an annual list of Contingency Plan modifications by November 15th of each year. EPA and MADEP approved these changes on an interim basis but asked that OMSAP review them.

W. Leo presented an overview of the proposed revision to the dissolved oxygen thresholds. In 1997, the Outfall Monitoring Task Force recommended the deletion of the DO percent saturation threshold because this threshold was frequently violated before the new outfall went on-line. However, DO percent saturation is one of the state water quality standards, and so the regulatory agencies preferred to keep that standard. EPA and MADEP instead recommended that "unless background conditions are lower" be added to both the DO percent saturation and concentration thresholds to make them more consistent with the state standard. W. Leo then presented MWRA's approach for calculating the background. The lowest DO survey means in the nearfield and Stellwagen Basin for each of the baseline years are plotted and fit a normal distribution to the points. Then calculate the background as the 5th percentile of this fitted distribution. The background calculations are all below the thresholds (except for DO concentration in Stellwagen Basin).

S. Tucker noted that the DO fluctuates annually on a cyclical basis. He asked why they are trying to move away from the water quality standards (WQS) instead of trying to capture that cycle in the thresholds. W. Leo replied that this threshold revision would bring MWRA more in line with the WQS because the state standards recognize that there are areas where the DO will naturally fall below 6 mg/L and 75% saturation. Also there is only an ecological concern during the late summer/fall when the DO is at its lowest. EPA in the process is in the process of revising their guidance for water quality standards for dissolved oxygen and that will probably result in a lower level considered to be a concern. She also explained that there is another DO threshold, the rate of change over other summer, so if we were to see an accelerated drop in DO, that would trigger the threshold.

G. Wallace asked if there have been any trend analyses conducted and if there were any year-to-year trends seen that might provide any kind of indication of steady state. W. Leo replied that those types of analyses would be found in the water column synthesis reports whereas the thresholds are quicker calculations for early warning, rather than an in-depth look at the data. C. Hunt added that Rocky Geyer, Anne Giblin, and Scott Libby are looking at the low DO and examining how much is local versus external. M. Shiaris thinks it might make sense to do a temporally high resolution DO analysis to use for the trends analysis. W. Leo said that the MWRA late summer/fall data are collected about every two weeks. Temporally high resolution data would come from the USGS mooring. M. Mickelson pointed out that they do not have that data because the sensor fouled too quickly. MWRA does have respiration data and perhaps we could determine why the DO is declining, e.g. is it due to respiration, low DO flowing in from other areas, or vertical mixing. M. Shiaris thinks more resolution would be useful because MWRA might be missing low points between surveys. M. Mickelson said that the patterns of DO are smooth enough that they do not think that they are missing significant changes. J. Shine pointed out that in some years, there is just one point that connects the minimum to maximum. J. Urban-Rich thinks more sampling points would outline the minima better. J. Shine thought then the issue would be resources, would it really be better to give up stations so there could be more frequent sampling (i.e. space vs. time). A. Rex pointed out that the threshold is a survey average of all of the

nearfield stations. C. Hunt thinks the question is, does the DO go below a level within two weeks to cause the biology to shut down. He thinks this system cannot respond that fast because it is relatively cold, and respiration is slow.

J. Pederson thinks that the inclusion of the “unless background conditions are lower” is reasonable because it addresses some of the concerns that OMSAP had previously because the threshold was being triggered during the baseline period. This inclusion seems more realistic based on what we know about the environment and it addresses the ecology, which is what the concern is about. It also makes it more consistent with the state regulations. A. Solow thinks this is quite a small change and it is bringing the thresholds into conformity with existing standards. M. Liebman asked OMSAP what they thought about MWRA’s approach for calculating the background. A. Solow thinks it is certainly consistent with some of the other thresholds. **ACTION:** OMSAP recommends that EPA and MADEP approve of the addition of “unless background conditions are lower” to the DO percent saturation and concentration Contingency Plan thresholds and approved of the approach for calculating the background DO.

W. Leo then described the proposed change in the nearfield *Alexandrium* cell count threshold to 100 cells/liter in any sample rather than the 95th percentile of the seasonal mean. *Alexandrium* is patchy, both spatially and temporally, and there are other monitoring programs focusing on *Alexandrium* or paralytic shellfish poisoning (PSP) that better characterize this occurrence. For those reasons, and the fact that there was a new PSP threshold under development, OMSAP recommended in July 2000 that the cell count threshold be deleted. EPA and MADEP declined to allow this but suggested an alternative threshold of 100 cells/liter, and that was based on the maximum observed value of 163 cells/liter during the baseline. She showed MWRA’s *Alexandrium* data and explained the advantages of 100 cells/liter over the original 95th percentile of baseline means threshold. 100 cells/liter is well below 300 cells/liter which is the value at which toxicity seems to begin occurring. Yet it seems high enough to avoid false alarms, since it was exceeded only once in the baseline period. The original method of calculating the 95th percentile produced an unrealistically low number because of all of the zeros in the data. J. Urban-Rich asked if this threshold would apply to the average nearfield. W. Leo replied that it would apply to any individual sample, however if there were sample replicates, an average would be taken. *Alexandrium tamarense* and other *Alexandrium* species will be added together. She presented recent *Alexandrium* data and there are a total of seven samples that have measurable *Alexandrium*. W. Leo added that the PSP threshold mentioned earlier will hopefully be ready in the spring of 2002.

M. Shiaris asked if anyone monitors further north since these blooms begin as far north as the Bay of Fundy, perhaps there was a way to get an early warning. M. Mickelson replied that the MA Division of Marine Fisheries (MADMF) communicates with the state of Maine sampling program. In 2000 there was a lot of toxicity in coastal Maine, with less in 2001. Mass Bay has not seen any toxicity in the past two years except in Nauset Bay which has recurring toxicity problems.

S. Tucker thinks a 100 cells/liter revised threshold is reasonable, but the Cape is still concerned about downstream effects of the outfall. W. Leo replied that the PSP threshold is based on existing PSP monitoring and covers the entire coastline. If any of the samples were to exceed the 100 cells/liter, MWRA would notify MADMF, OMSAP, and others. S. Tucker asked if there was a plan to compare the PSP sampling on the shore with the MWRA sampling at sea. M. Mickelson replied that the correlation between cell counts and PSP toxicity is something that is in the purview of MADMF and that Mike Hickey from MADMF has said that the correlation is not great but it is something that they will continue to examine. S. Tucker wondered what would happen if there was a cell count exceedance, and whether there would be enough information available to determine the cause. J. Schwartz replied that developing a threshold is an imperfect science, but it does offer a flag. He is curious about the recent low values of *Alexandrium* because the last bloom was a while ago, in 1993. He is looking forward to seeing the new PSP threshold. A. Solow said that what is

interesting about the PSP data is the spatial pattern and the timing of the toxicity and he thinks the important information is not whether a station exceeded a threshold, but the pattern of the toxicity.

J. Pederson asked what the original question was, i.e. why was this cell count threshold developed. Are we just looking to see if there is an exceedance that is close to a level of concern or are we looking for something else. M. Liebman replied he thinks we are trying to do both. This is a caution “flag” that is designed to show us if something different is occurring and it means we need to gather more information to learn what is going on. It was designed to be at a level lower than when toxicity develops and it was chosen statistically because there was a sample over 100 cells/liter only once in the nine years of baseline. A. Solow is inclined to support this revision because it can be revised as more information becomes available. He thinks there should be such a threshold for political and scientific reasons. J. Pederson added that this is better than what we had before which was so low that it did not make any sense. J. Schwartz told the group that MADMF supports this revision, until an improved threshold is developed. P. Borrelli asked that this issue be revisited no later than a year from now. M. Shiaris would only want to revisit this if there was better scientific information. P. Borrelli thinks it is very important that OMSAP revisit this. A. Solow thinks there is a strong expectation that OMSAP will be revisiting this as soon as there is better scientific information available. **ACTION:** OMSAP recommends that EPA and MADEP approve of the change in the Alexandrium abundance Contingency Plan threshold to 100 cells/L.

RECENT CHLOROPHYLL MONITORING RESULTS, COMPARISONS TO THRESHOLDS, AND UPDATE ON DATA CORRECTIONS

M. Mickelson described the fall 2000 chlorophyll exceedance. An intense region-wide bloom was seen in satellite data in September to October 2000. The monitoring program measured high fluorescence during this time, however particulate carbon and phytoplankton cell counts were not unusually high and DO was not lower than usual. Lower zooplankton counts in some areas may have been due to large numbers of comb jellies that prey on zooplankton. These high chlorophyll values were not seen in 2001. After correcting their data for analytical errors, MWRA calculated the chlorophyll threshold based on OMSAP guidance. M. Mickelson then presented the chlorophyll data and the seasonal threshold values. He then showed recent data of the nuisance algae species, *Alexandrium*, *Phaeocystis*, and *Pseudonitzschia* as well as dissolved oxygen. D. Dow asked if MWRA monitors for domoic acid since not all species of *Pseudonitzschia* produce the toxin. M. Mickelson replied that they count all species of *Pseudonitzschia* but do not measure domoic acid. The threshold is the 95th percentile of seasonal abundance.

CAPE COD BAY MONITORING PROJECT

P. Borrelli described the Center for Coastal Studies (CCS) monitoring project. This is an independent program, separate from the MWRA, although MWRA has been very helpful in splitting samples from the nearfield. The project samples ~10 stations in Cape Cod Bay based on CCS station locations. The project is mostly privately funded though the Massachusetts Environmental Trust has been a major contributor. We anticipate maintaining the program at its current level for a total of three years and next year we plan to add a few more stations in the nearfield (MWRA station locations).

S. Mayo outlined the impetus for this project. First, he hopes that information collected will help manage an embayment that is of great interest to many people. Second, there are still concerns about MWRA and he hopes the project can help answer some of those questions. Lastly, we hope to add to the data collection in Cape Cod Bay related to the MWRA outfall. One of the main concerns is the southward flow of currents from the outfall, towards Cape Cod Bay. He still maintains that there is a lot to learn about Cape Cod Bay and he is particularly interested in the zooplankton dynamics in the spring in eastern Cape Cod Bay. The two main goals of the project are to set up a baseline against which to judge future results and to look at indicators such as stable isotopes of nitrogen.

S. Mayo then described the structure of the project and presented preliminary results [see report: <http://www.coastalstudies.org/research/monitoringupdates.htm>]. There are 10 stations sampled monthly at three depths in Cape Cod Bay. Parameters include stable nitrogen isotopes, phytoplankton, zooplankton, chlorophyll *a*, and CTD parameters. We plan on adding stations wherever we find interesting features. In the last year, they have seen no indication of an effect of the outfall on the density or species composition of the zooplankton. The data do not seem to have changed very much since Bigelow did his work in the 1920's and though the phytoplankton data are variable, there is nothing out of the ordinary to suggest a change due to outfall nitrogen. We did see some nuisance algae but not in numbers of concern. A. Solow asked what would have indicated an outfall influence. S. Mayo replied that since the project is only one year old, they would have had to see some type of extraordinary event. He supposed that a dramatic change in the zooplankton species or density could be an indicator. His point to those who are still concerned about the outfall is that right now they are not seeing a disaster scenario.

S. Mayo then described the stable nitrogen isotope sampling that may help track the outfall nitrogen. ^{15}N is a ratio of ^{15}N (less common in the marine environment) to ^{14}N . Sewage is richer in ^{15}N , giving it a higher ^{15}N . M. Mickelson noted that this occurs when there is incomplete sewage treatment [unlike MWRA] due to the loss of ammonia to the atmosphere – the lighter ^{14}N tends to escape first. When there is complete sewage treatment, there is no significant change in the isotopic ratio. S. Mayo added that biological processes tend to favor the accumulation of ^{15}N . M. Mickelson pointed out that because of more complete treatment, MWRA's effluent does not have a ^{15}N signal. D. Dow asked if it is possible to use this method to discern from septic sources on Cape Cod from the MWRA outfall. S. Mayo replied that all ^{15}N is the same and so it all depends on the ratio. It is not a marker but rather information that needs to be interpreted.

B. Berman asked about the lack of signal from the MWRA outfall. S. Mayo replied that his understanding was that Joe Montoya sees a ^{15}N that is ~8, well above the receiving waters. M. Mickelson countered that uptake by phytoplankton could be creating this same signal. A. Solow asked if we can use the ^{15}N to track effluent. M. Shiaris (and others) replied maybe, it is very complicated. J. Urban-Rich added that there are many ways of calculating the

^{15}N . C. Hunt said that if the source is not discharging a substantially different ^{15}N , then it could not be used as a tracer over a large distance. S. Mayo said that there do seem to be gradients of ^{15}N as you move away from the outfall. He then showed ^{15}N data from archived samples and there do not seem to be any changes or trends since the 1990's – although there is a concern that the samples may not have all been preserved properly. Next he showed the post discharge data. Soon after the outfall went on-line, there was a small increase in the ^{15}N but it may be related to a cyclical seasonal shift in the uptake and incorporation of nitrogenous compounds in organisms. He then showed the results from the MWRA samples. They are not preserved the same way that J. Montoya preserves his samples and we are reviewing the effects of these differences. Results show that ^{15}N is fairly low in Cape Cod Bay, but higher further north. Lastly, he compared ^{15}N by distance from the outfall that showed a similar pattern but there are still a lot of questions that need to be addressed. He emphasized that these results are preliminary.

P. Borrelli thinks it is useful to point out the 30-mile station. S. Mayo agreed. Looking at the ^{15}N data, there appears to be something occurring along the shore off of Plymouth but this needs to be examined further. There is no clear gradient in the ammonia assay of ^{15}N in the 1999 samples, but there seems to be a fairly clear ^{15}N signal from effluent spreading southwards towards the entrance to Cape Cod Bay in the October 2000 data assayed from nitrate. C. Hunt thinks the lower ^{15}N in Cape Cod Bay may not be outfall related. It could be that Cape Cod Bay, which is more of a closed embayment than Mass Bay, processes nitrogen differently. S. Mayo agreed.

OMSAP PUBLIC WORKSHOP TALKING POINTS

C. Coniaris gave an overview of the workshop format and public outreach to date. The OMSAP charter

states that OMSAP is to host a public workshop annually to present results of the monitoring. The workshop will be held at two locations, Boston and Hyannis. The workshop will begin with EPA and MADEP will describing permit and Contingency Plan, Andy Solow will describe OMSAP and its roles, and Patty Foley will discuss why public should be involved in this process. Then Andrea Rex and Mike Mickelson will present the monitoring and results for 2000 and the public will be given the chance to ask OMSAP questions at the end. A. Rex then discussed the topics that she and Mike Mickelson planned to present at the workshop.

M. Shiaris thinks the public would like to hear about the beaches and they are linked to MWRA because of CSOs. J. Pederson and A. Solow agreed. D. Dow thought discussing shellfish closures would be interesting. A. Solow is concerned that if you try to pack too much into a short presentation, people are going to be lost. A. Rex agreed and asked if OMSAP thought there was anything on the list of topics that could be removed. M. Shiaris thought that the public would not be very interested in the benthic community data since it is not something that they can relate to. J. Pederson thinks that the public should understand biodiversity and she thinks they should keep the benthic community discussion but perhaps drop flounder liver disease. A. Solow thinks it is important to make sure the audience is told what a benthic community is. A. Rex said that her presentation will be similar to her presentation at the Boston Harbor Symposium was understandable and interesting to the general public. P. Foley thought it was. She also agrees with J. Pederson in that the public needs to be educated.

W. Leo brought up beaches again. A. Rex would be happy to discuss beaches, but it is not a part of the Boston Harbor Project. B. Berman pointed out that whenever the press attempts to write about the outfall, they end up writing about the beaches. J. Pederson suggested leaving beaches to the end and be prepared to address it. A. Solow thought that maybe one way to save time is to mention a list of parameters monitored, but only discuss a few of them in any detail. J. Pederson thinks it is important to include in the introduction the four original questions to put things in perspective: Is it safe to swim? Is it safe to eat the shellfish? Are the aesthetics being maintained? And are we protecting the natural resources? A. Solow thinks that one of the important to show the benefits of the outfall. W. Leo thinks one of the things that can be shown is that now there are fewer blooms occurring in Boston Harbor that are exported to the bay. D. Dow asked if the audience will be allowed to ask questions during the presentations. A. Solow thought it would be more efficient for the audience to ask questions at the end. C. Coniaris offered to prepare a fact sheet and glossary to help the audience with terms they are not familiar with. W. Leo asked if A. Rex is planning on describing the improvements in treatment. A. Rex replied that it will be covered briefly. D. Duest asked if the treatment process will be described. A. Rex thinks that can be described in handouts. P. Foley thought that was fine. She likes the outline but thinks we also have to be flexible because the audience may want to discuss other issues.

A. Rex asked how the marine mammal observations should be reported. S. Testaverde thinks that the public needs to know that there are marine mammals, including right whales out there. A. Rex said that she could show the map of whale observations. B. Berman thinks it would be useful to include other independent research such as the Center for Coastal Studies work. The fact that there is not a broad divergence in MWRA and independent monitoring efforts is a powerful point to convey to the public. A. Solow thought that was a good idea. S. Mayo agreed that A. Solow could mention the project, as long as it is made clear that the results and conclusions are preliminary and the project has just begun. A. Rex then outlined the handouts and posters that they will prepare. She said that MWRA will also be prepared to answer questions on topics not covered in the presentations. She noted that the post-discharge benthic and fish and shellfish data are not ready to present. A. Solow thinks it is still important that the public know that they are being monitored.

PUBLIC INTEREST ADVISORY COMMITTEE UPDATE

P. Foley discussed the public outreach that PIAC has helped with. There has been a great effort to work to inform the public that these workshops are going to take place. We have reached out to newspapers to notify

them of the workshops and see if there is interest in covering these meetings. However, since we are not in crisis mode, there is not much interest from the press, but we will continue with that effort. She thanked OMSAP and all of the government agencies that have helped with outreach in this effort.

ADJOURN

MEETING HANDOUTS:

- Agenda
- October 2001 OMSAP/PIAC/IAAC membership lists
- April 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.