

Outfall Monitoring Science Advisory Panel (OMSAP) Meeting

Wednesday, February 23, 2000

10:00 AM - 2:00 PM

EPA Boston

FINAL MINUTES

ATTENDANCE

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

Observers: Margaret Callanan, Cape Cod Commission; Cathy Coniaris, OMSAP staff; Mike Delaney, MWRA; Brian Ellis, Technology Planning and Management Corp.; Gillian Grossman, Save the Harbor/Save the Bay; Carlton Hunt, Battelle; Mike Hill, EPA; Russell Isaac, MADEP; Ken Keay, MWRA; Christian Krahforst, MCZM; Wendy Leo, MWRA; Matt Liebman, EPA; Steve Lipman, MADEP; Mike Mickelson, MWRA; Bill Ravanesi, Healthcare Without Harm; Susan Redlich, WAC; Virginia Renick, MWRA; Andrea Rex, MWRA; Larry Schafer, retired; Jack Schwartz, MADMF; Dave Taylor, MWRA; Sal Testaverde, NMFS; Dave Tomey, EPA; Heather Trulli, Battelle; Steve Tucker, Cape Cod Commission/Bays Legal Fund; and Jeff Turner, UMass Dartmouth.

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.

SUMMARY OF ACTION ITEMS & RECOMMENDATIONS

1. A clarification of the definition of the zone of initial dilution was added to the March 1999 minutes by Dave Tomey (EPA) and the minutes were approved as amended.
2. The Model Evaluation Group is currently drafting their final report to OMSAP. Representatives from MADEP and EPA were asked to discuss whether the models would be useful management tools for other Massachusetts coastal discharges at an upcoming OMSAP meeting.
3. OMSAP does not support the current narrative zooplankton threshold as it is currently formulated and recommends its deletion from the Contingency Plan. However, OMSAP believes that continued zooplankton monitoring is extremely important and requests that MWRA present a plan to OMSAP on how to analyze the zooplankton data using a system-wide approach.
4. OMSAP believes that the current measurements of sludge and scum removed by the Deer Island Treatment Plant (DITP) as well as FOG (fats, oils, and grease) measurements in the treated effluent adequately address the NPDES permit requirements for aesthetics.

OMSAP recommends that MWRA delete the current floatables threshold in the Contingency Plan. OMSAP is requesting that MWRA report the sludge, scum, and FOG measurements in the annual Outfall Monitoring Overview; that MWRA address concerns regarding floatables if there is an adverse DITP event; and that MWRA “fingerprint” the floatables from the DITP in a special study

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MARCH 1999 MINUTES

D. Tomey pointed out that the definition of the zone of initial dilution (ZID) mentioned in the plume tracking section of the March 1999 minutes is from the 301h regulations and is defined in terms of water depth. EPA instead defined the ZID as the “edge of hydraulic mixing” in the Environmental Impact Statement for the new outfall since in this case, the classic 301h definition does not apply. The EPA definition is implicit in the ULINE model. M. Mickelson stated that MWRA would like to know if this is a significant issue since they are preparing the plume tracking work plan. For the ULINE modeling, Roberts et al. (1989) used a 52.5 meter scale for the physical model and MWRA is utilizing 60 meters for plume tracking. D. Tomey replied that though initial mixing was evaluated in the EIS, the size of the mixing zone was only approximated.

B. Robinson asked what the extent of the change in ZID would be seasonally using the EPA definition. D. Tomey responded that it could change based on flow but that using the 301h definition gives a static value. He mentioned this because he wanted to make sure the group was not thinking about the static 301h definition for the purpose of the plume tracking. Using the EPA definition, the extent of the ZID is somewhat dynamic and varies, possibly hourly, due to changes in flow.

W. Leo also pointed out that the EIS definition allows for a ZID calculation that takes changes in dilution into account. She asked whether changing the term “ZID” to “mixing zone” would clarify this issue in the minutes. D. Tomey stated that EPA used the term “initial mixing” instead of “mixing zone”. M. Liebman added that since the term “ZID” was used at the March 1999 meeting, it should remain in the minutes. OMSAP agreed that a clarification of the “ZID” definition be added as a post-meeting update to the March 22, 1999 OMSAP minutes. OMSAP approved the March 1999 minutes as amended.

ACTION: The following post-meeting comment will be added to the March 22, 1999 minutes: [UPDATE: Here is a clarification of the ZID definition provided by Dave Tomey. The permit had a different definition of the mixing zone than the outfall EIR/EIS. The size of the mixing zone for the permit was not determined based on a simple depth factor, as used in the Section 301 (h) definition and implied in the March 1999 OMSAP minutes. The Fact Sheet of the MWRA permit stated: *"The draft permit limitations are based on the most restrictive type of mixing zone, the area of hydraulic initial dilution, called the zone of initial dilution. For the draft permit, that area is expected to occur at approximately 60 meters (197 feet) away from the diffuser outfall, and that determination incorporated only the most conservative (i.e., 'worst case') conditions of the receiving water and discharge flow."* (Note: this applies to the final permit as well as the draft). The MWRA Facilities Plan/EIR and the EPA EIS, which predated

the permit, assumed a dynamic mixing zone. Both agencies agreed to define the mixing zone as a dynamic edge of hydraulic mixing, which, in reality, would vary with flow and ambient currents. The permit, on the other hand, needed a more definable area for developing a practical compliance strategy. Worst case conditions and the results of Roberts' modeling were used to delineate the permit mixing zone with the dimensions of 52.5 m in any horizontal direction from diffuser axis. (This was expanded to be about 60 m which happens to be about 2 times the depth.)

Although this distinction is not major issue, it is important to clarify the mixing zone in the context of the proposed plume tracking study. The primary objective of the study is to verify the dilution and immediate dispersion of the effluent plume as modeled in the EIR/EIS and subsequent analyses under the conditions present during the study. It will be important to find the dynamic hydraulic mixing zone first to verify the modeled dilution at that point. Then the study will follow the advection of the plume in the farfield to about 500:1 dilution after semi-steady state conditions are achieved. Whether the zone is completely encompassed within the 60 m box is only of secondary concern since the study is only a snapshot of mixing under the particular conditions of flow, ambient current and wind at the time of the study.]

MERCURY IN EFFLUENT

A. Solow informed the group that a question has arisen about MWRA's request to change the mercury standard. M. Delaney briefly described MWRA's industrial pretreatment program that regulates industrial discharges into the sewer system. Within this program, MWRA reviews the local limits every five years and this process, which involves public comment, is currently underway. MWRA is considering a revision to the mercury local limits. Currently MWRA prohibits mercury discharge and the action limit is 1 ppb or 1 ug/L. MWRA is planning to propose a limit of 4 ppb or 4 ug/L using calculations that consider the mercury levels in fertilizer pellets. The 4 ug/L industrial local limit standard would be adequate to safely meet the pellet limit. MWRA ENQUAD is still considering whether this is an OMSAP issue since there is no mercury limit in the final NPDES permit. Also, MWRA does not envision that this would cause a change to the Outfall Monitoring Program. At some point during the public comment period, or when MWRA submits their plan to EPA, the issue could be referred to OMSAP for review.

B. Ravanese asked how MWRA TRAC [Toxic Reduction and Control Department] developed the proposed 4 ppb limit. He thought that OMSAP would be one of the first groups to review any such proposals. There is a lot of controversy over this proposal and he thinks it is being driven by a few non-compliant hospitals. A. Rex argued that MWRA does not have a vested interest in the limit, however it must be defensible. MWRA used an EPA-approved model to calculate this new proposed limit. B. Ravanese feels that the calculation is not health-based. A. Rex pointed out that the main source of mercury in finfish and shellfish is atmospheric, not the MWRA effluent. B. Ravanese said that mercury levels are increasing in flounder [from: Mike Moore, OMSAP Workshop presentation September 1999, Mercury data in nearfield flounder tissue and liver 1997-1999].

S. Testaverde requested that MWRA give a presentation at a future OMSAP meeting on the TRAC Department. He also would like to know what other thresholds could be revised using this particular pathway. A. Solow said that this could be addressed at a future meeting. S. Redlich

suggested that MWRA provide information to the group by the end of the meeting about the public comment period and the steps involved with this proposed revision. She does not think that the Board of Directors have released these proposed limits yet and there are at least two public comment periods before the proposal is voted upon by the Board.

S. Testaverde suggested that OMSAP obtain the minutes or upcoming agendas of the MWRA Board of Directors meetings. A. Solow said that OMSAP will consider this. M. Delaney stated that this issue will be discussed at the March Board meeting, and the proposed revision to the mercury local limit has not been approved by them yet. If they decide on a local limit revision, the due review process (which includes public participation), takes about a year. A. Solow thanked B. Ravanesi for bringing this issue to OMSAP's attention and said that they will wait to see what the next step is. [UPDATE: the MWRA Board of Directors is no longer considering a revision to the local limit for mercury.]

MODEL EVALUATION GROUP (MEG) UPDATE

B. Beardsley updated the group on progress of the MEG in evaluating the Bays Eutrophication Model (BEM). In March 1999, OMSAP decided to form a Bays Eutrophication model evaluation group. Members are: B. Beardsley (chair), Eric Adams, Jeff Cornwell, Don Harleman, Jack Kelly, Jay O'Reilly, and John Paul. The group had an open meeting in December at Woods Hole to review the model results of the 1992-1994 runs, discuss baseline monitoring features, and discuss revisions and the future of the model. The group is currently in the process of writing a final report for submittal to OMSAP. It will be a brief report containing an introduction, summary, general recommendations on how to improve the model, and recommendations for additions to the BEM report. There will also be recommendations on how to improve the Outfall Monitoring Program boundary sampling. S. Redlich asked if the model is being used, and if so, how actively. B. Beardsley replied that the model has been run for the 1992-1994 period. M. Mickelson added that future use of the model is on hold pending the advice of the MEG.

J. Pederson believes that one important issue is finding a permanent home for the model so that it may be used for other coastal outfalls in Massachusetts. She asked whether MEG addressed this at their meeting. B. Beardsley said that the group did have some discussion on a future home but did not have a chance to discuss its possible use with other outfalls. He reminded everyone that there are two models involved, the Hydrodynamic (HD) Model (USGS) and the Water Quality (WQ) Model (HydroQual). The MEG has not reached a clear consensus on a home for the models but the group agrees that model results are useful in the planning process and that they should be continued. The MEG is not endorsing the current model until several revisions are undertaken, and thinks that this kind of effort is important to continue, thus the issue of a permanent home for the model needs to be discussed.

M. Liebman asked how the models are actually run. B. Beardsley briefly explained that the HD model uses real data (atmospheric forcing, winds, fluxes, etc.) to calculate the velocity field and stratification. The WQ model uses that information to help calculate phytoplankton abundance, dissolved oxygen, nutrient concentrations, and other water quality parameters. The two models are not necessarily coupled but it is clear that the physics need to be accurate in order to successfully reproduce the biology. Running the models requires a lot of data and effort. It can take several months to run a single year of just the HD model. Running the models involves

collecting all available data, running the physical model, and comparing results with actual data. Results then go into the biological model. Once the model is run, results are compared in detail with field observations and shortcomings are identified. For example, there was a significant fall bloom in 1993 that was not captured in the model and it was determined that the mix of phytoplankton in the model was not correct. The model can then be used to simulate various outfall scenarios. Interannual simulations and comparisons build confidence in the model and show where the are weaknesses. There is a similar model being run the Chesapeake Bay that is also being evaluated. C. Hunt added that the lag in the field data used in the HD model results from the lack of available Gulf of Maine (GOM) data, rather than Massachusetts Bay data, since the GOM is the major force driving the hydrodynamics in Massachusetts Bay.

J. Pederson feels that there are two issues: (1) what research would make the model better and (2) what should be the responsibility of MWRA and what might be another agency's responsibility. The state and EPA should look at whether these models can be used to address other issues. She suggested that at the next meeting, the state and EPA give OMSAP an idea as to whether they are valuable in terms of other regulatory issues. USGS is more interested in refining their model, not running "what if" scenarios. Even if the models move to a university or another agency, the state should definitely support some effort towards the runs and use them to try to answer some questions. B. Beardsley feels that it is not clear whether the HD model should move from USGS since it requires a lot of work to set up. J. Pederson agreed but thinks there should be some feedback directly from the state. A. Solow agreed with J. Pederson's suggestion and requested that it be added to the March agenda.

ACTION: The Model Evaluation Group is currently drafting their final report to OMSAP. Representatives from MADEP and EPA were asked to discuss whether the models would be useful management tools for other Massachusetts coastal discharges at an upcoming OMSAP meeting.

ZOOPLANKTON THRESHOLD REVIEW

M. Mickelson showed a map of plankton monitoring stations in the nearfield and farfield, which includes Boston Harbor, Massachusetts Bay, and Cape Cod Bay. He showed data on zooplankton from 1992 to September 1999 and pointed out that it is not easy to discern patterns in the data since there are so many species as well as stages of each species. MWRA is particularly interested in certain copepods since they are the prey of right whales. Several years ago, the Outfall Monitoring Task Force asked that MWRA attempt to develop a zooplankton threshold. MWRA has since made two attempts at developing such a threshold. The first, which is included in the Contingency Plan, is called the Acartia hypothesis and concerns a "shift towards an inshore community". *Acartia hudsonica* and *Acartia tonsa* are common in the inshore, but not offshore where several species of copepods and *Oithona* are numerically dominant. Species known to be prime whale food are *Calanus finmarchicus* and *Pseudocalanus newmani*, although they also feed on other zooplankton species. Stormy Mayo has found that though *Oithona* are smaller, and can pass through baleen, they can still be eaten with a 30% filtering efficiency.

The average harbor salinity is about 30-31 parts per thousand (ppt) and the nearfield is slightly more saline (31-32 ppt). When the outfall is relocated, the salinity of the harbor may increase by only about a half a part per thousand since there are also other sources of freshwater to the

harbor. When the *Acartia* hypothesis was originally developed, it was thought to be a good eutrophication threshold since it was believed that the limiting growth factor was a high concentration of nutrients. Further research into the literature has shown that *Acartia* is instead limited by salinity, thus MWRA began to question whether this is an appropriate threshold since the new outfall will not significantly change the salinity in Massachusetts Bay. Because of this problem, MWRA then looked for another hypothesis.

D. Tomey asked whether *Acartia* has ever been dominant in Cape Cod Bay. J. Turner replied that *Acartia* has been found there, but never in high abundances. D. Tomey asked whether *Acartia* reproduction is limited to areas with lower salinities, such as inshore areas. J. Turner replied yes, and that his experiments during the mid-1980's in Beaufort NC examined this hypothesis. Key to the inshore distribution of *Acartia tonsa* is the fact that the nauplii do not develop past stage N3 if the salinity is greater than 20-25 ppt. Optimum temperature is approximately 20 degrees Celsius, which is why they are mainly found in this area during the summer months. A few adults and late stage copepodites can be found in Cape Cod Bay and are probably being washed out of nearshore embayments. J. Pederson pointed out that M. Mickelson showed that the salinities in Boston Harbor do not reach as low as 25 ppt very often so *Acartia* must be developing near the rivers. J. Turner thinks that the nauplii are developing further upstream in the harbor and the adults or late stage copepodites are washed out to the nearfield, but not in any large numbers.

M. Mickelson then described the second potential zooplankton threshold MWRA has been developing – an oceanic hypothesis in which the winter/spring mean abundance of the five species (*Calanus finmarchicus*, and others) would not decrease substantially below the 5th percentile of the existing baseline abundances. MWRA had its statistician examine the data to determine the 5th percentile. M. Mickelson pointed out that if there was an exceedance of the threshold, MWRA would notify OMSAP and the regulators, post the information on the Internet, as part of the Contingency Plan procedures.

M. Mickelson showed results from C. Hunt's presentation at the September 1999 OMSAP workshop that showed regional analyses of zooplankton abundances in several zones: offshore, boundary, coastal, harbor, and Cape Cod Bay. Results show dramatic differences among the regions, thus the 95th percentile does not appear to be a very sensitive indicator. There is also a conceptual problem with this new threshold in describing how eutrophication would lead to a decrease in zooplankton species of interest. MWRA is seeking OMSAP guidance as to the next step. Should there be some other threshold, or no threshold with continued data analyses.

D. Tomey asked why the hypothesis should be limited to the nearfield. M. Mickelson replied that MWRA focuses on the nearfield for most of the thresholds because this is where impacts would most likely be observed. D. Tomey thinks that since the zooplankton community is driven by what is entering the system from the boundary and is transported into Cape Cod Bay (where the residence time is higher, increasing the chance of a response), the threshold should focus there. M. Mickelson agreed that the system is much like a conveyor belt, or "river" and the species in the nearfield are very much reflected by what is entering from the Gulf of Maine. The question is whether MWRA is somehow "dosing" this "river" and will there be an effect on the zooplankton appearing further "downstream". MWRA will continue to monitor the Cape Cod Bay stations for changes. C. Hunt added that all modeling results indicate that the amount of nutrients from the

outfall are perhaps 10%, a relatively small fraction, of what is entering from the Gulf of Maine. So the assumption is that this 10% increase will somehow have a major impact to the zooplankton downstream in Cape Cod Bay. He questions whether this will in fact occur. W. Leo also pointed out that it is important to note that MWRA will not be discharging greater amounts of nutrients with the new outfall.

A. Solow replied that MWRA will continue to monitor in Cape Cod Bay. The question is whether there is some sensible and specific threshold. What would be interesting would be to compare what is entering the nearfield from the Gulf of Maine with what is seen in Cape Cod Bay in order to put this in a regional context. If changes in Cape Cod Bay were in fact due merely to variations with the input along the “conveyor belt”, it would not cause alarm about the outfall. J. Pederson asked A. Solow how the variability in the system should be approached. She is concerned that a specific threshold would not be meaningful due to natural variations in species compositions over time. A. Solow said that he does not necessarily think that there should be a zooplankton threshold but that zooplankton monitoring should definitely be continued.

J. Shine asked how the 5th percentile value was chosen. M. Mickelson replied that it is fit it to a distribution. D. Tomey asked whether the data are normally distributed. M. Mickelson replied yes, the annual means are normally distributed. K. Keay added that January to May (spring annual mean) was plotted. Five of seven individual annual means were normally distributed for nearfield samples. The 5th percentile assumes a normal distribution. B. Beardsley asked whether there are differences between Cape Cod Bay and the nearfield. K. Keay replied that a set of summaries has not been completed for the Cape Cod Bay stations. C. Hunt added that the entire farfield was compared to the nearfield set of zooplankton data and there is no significant difference between the two data sets. The entire farfield was examined, without separating Cape Cod Bay. M. Mickelson asked OMSAP what they would like MWRA to do next.

J. Shine asked that given the variability in the data, and the fit to the distribution for the 5th percentile, the threshold must be an incredibly low number. M. Mickelson replied that it is about 2400 animals per cubic meter. J. Shine asked whether there should be some kind of benchmark above that number which could be considered a low value. M. Mickelson said about 4000 animals per cubic meter is a number determined by Stormy Mayo. S. Testaverde added that the value at which a whale stops feeding is about 3800 animals per cubic meter or less. A. Solow added that whales do not eat the “mean”, they eat the patches which is the 95th percentile.

A. Rex stated that MWRA is proposing to remove the zooplankton threshold from the Contingency Plan but will continue to collect zooplankton data and conduct detailed data analyses and presentations for OMSAP since MWRA has not been able to develop a threshold that makes biological sense. MWRA requests OMSAP comment on this proposal.

S. Testaverde asked how many whales were seen by the surveys during that period in the nearfield. C. Hunt replied less than 10 have been seen during the Battelle surveys; it is relatively rare to see them and there is no data on how long the whales remain in the nearfield. S. Testaverde asked why MWRA is suggesting lowering thresholds before going on-line and why MWRA cannot wait until there is at least one year of post-discharge data. J. Pederson replied that

the concern is having a threshold for zooplankton which is so variable. Everyone knew from the beginning that it would not be possible to collect enough samples to give a full picture and she thinks to try to set a threshold in the absence of sufficient data is irresponsible. However, monitoring should continue and perhaps there are other ways of determining whether or not there is a problem. She, at this point, feels strongly that from a biological perspective, this data is so variable that she is hesitant to set a threshold. J. Shine thinks that there is a lot of data, just that it is stochastic and so variable, that in order to see a significant change, meaningful change is missed due to the underlying variability. This variability cannot be reduced, no matter how many samples are collected.

A. Solow asked S. Testaverde why there should be thresholds that do not make sense scientifically. S. Testaverde replied that the thresholds went to public comment and were approved. He is concerned that thresholds will be changed without full public review and post-discharge information. G. Renick clarified that OMSAP does not have the authority to make threshold changes. OMSAP recommendations will go through the permit procedures that include public comment before they become final changes in the Contingency Plan. Even though the permit and outfall start-up have been delayed, OMSAP needs to be looking at data to figure out what makes sense and whether there are changes that could be recommended. However, any changes do not become final until there is the opportunity under the permit to make formal recommendations to EPA and MADEP and have public comment. This is not intended in any way to be a runaround to full public participation at the appropriate time and with the appropriate procedures. A. Solow believes that this is a good point. He thinks that the argument against the *Acartia* hypothesis threshold, even though the outfall is not operational, makes good scientific sense. He agreed with the other OMSAP members. OMSAP has not heard a logical scientific argument about what kind change they should be looking for, so it seems appropriate to keep the options open. There should be the flexibility to look at these data and be able to say that something else seems to be going on here that is worth a careful look. We do not want to fixate on a threshold that we do not have confidence in. He would support dropping the zooplankton threshold as long as OMSAP continues to have the opportunity to review these data.

B. Robinson asked whether it would be possible to develop a threshold on distribution/predominance of zooplankton instead of change in community structure. J. Turner thinks that since MWRA will continue to monitor, changes in distribution/predominance would be seen. The problem is interpreting what might have caused any changes. B. Robinson thinks that having a zooplankton threshold is a way of giving OMSAP the option of becoming involved if there is a problem in the future. However, there should only be a threshold if it makes scientific sense.

B. Beardsley feels that since there is such interannual variability in the biology, he would be uncomfortable setting a threshold. Even if there was a community structure threshold, data would have to be examined for at least several years in order to see if the change was significant. He thinks that it would be more worthwhile to instead try to refine the analysis on the monitoring data and perhaps instead of looking at the nearshore, examine variability at F26 and F27 (outer boundary) and track changes throughout the year. This would be something like the “state of the ecology” for the year. This information would give OMSAP a better sense of what the variability is.

R. Isaac thinks that from a regulatory standpoint, it would be important to acknowledge that any changes in the threshold are based on scientific information that was not available when the original threshold was developed in order to make a reversal scientifically sound. A. Solow asked when the threshold was submitted. M. Mickelson replied that it is in the Contingency Plan dated November 1997. J. Pederson did not think that the Outfall Monitoring Task Force approved the *Acartia* hypothesis threshold during the permitting process, and because it was in the Contingency Plan, it was accepted as part of the permit without full scientific review. R. Isaac thinks that it is important to present that in the final OMSAP recommendations to EPA/MADEP. D. Tomey added that the 5th percentile threshold (decrease in whale prey) is only a proposal. The *Acartia* hypothesis (a qualitative shift from an offshore to inshore community) threshold is the one included in the Contingency Plan as part of the permit.

A. Solow pointed out that even if OMSAP recommends deleting this threshold, that there will always be the option to develop a threshold in the future as new information and knowledge arises. He made the motion to remove the current zooplankton threshold, acknowledging that continued zooplankton monitoring is extremely important. B. Beardsley added that OMSAP should also recommend that since the Massachusetts Bays system is “flow-through” system, MWRA develop a method of analyzing the current data spatially and temporally in order to better contrast differences between the nearfield and Cape Cod Bay. A. Rex suggested that OMSAP request that MWRA develop a plan for OMSAP approval for analyzing the data. A. Solow agreed with B. Beardsley and A. Rex and emphasized that if one examined the difference between the boundary stations and the other stations, some of this variability will disappear, i.e. “differencing” stations will reduce variability and changes over time may be better discerned. Or, MWRA could also come forward to OMSAP and propose a better way of looking at the data.

A. Solow asked all in favor of the motion. OMSAP members unanimously approved of the motion. J. Schwartz asked what the motion reads as. A. Solow repeated the motion. **ACTION:** OMSAP does not support the current narrative zooplankton threshold as it is currently formulated and recommends its deletion from the Contingency Plan. However, OMSAP believes that continued zooplankton monitoring is extremely important and requests that MWRA present a plan to OMSAP on how to analyze the zooplankton data using a system-wide approach.

FLOATABLES THRESHOLD REVIEW

M. Mickelson described the floatables collection devices at the Deer Island Treatment Plant (DITP). The Contingency Plan threshold for floatables is: “floatables shall not exceed 5 gallons/day in the final collections device.” However, the way the DITP was designed, it has turned out to be impracticable to sample the final collections device. He described the other measures which can give confidence that the floatables problem is being efficiently addressed, as well as a description of floatables nearfield sampling [see information briefing dated February 23, 2000]. MWRA has other measurements of plant performance such as sludge and scum removal, and fats, oil, and grease (FOG) effluent concentrations. The treatment plant is very efficient at removing these as long as it is functioning properly. They are measured daily in primary treatment and reported in the monthly discharge report, and will eventually be posted on the Internet. The DITP is very effective at removing FOG and concentrations approach the 7 mg/L detection limit. Continued in-plant efficiency of the removal of sludge, scum and FOG is a surrogate measure of plant performance and implies that floatables are not discharged. The field

program has also begun testing another type of sampling that involves dragging a net (two meters wide, 1 mm mesh size) over a measured distance between stations to capture floatables. This has been done by H. Trulli (Battelle) for EPA and there is an established sampling approach for this method.

L. Schafer thinks the whole issue of floatables is largely an aesthetic one and is not directly concerned with plant operations. He thinks that one of the biggest areas of vulnerability of the Outfall Monitoring Program is public perception – if there was something accumulating aesthetically in the bay, the danger is that the Authority will be blamed for it, regardless of the source. He suggests starting a sampling program that looks at the condition of the surface waters before the outfall goes on-line. He understands that floatables in the nearfield are not necessarily from the DITP but he thinks it is important that MWRA at least measure it. Upsets do occur at treatment plants and the Authority should have a well defined position so that they are able to state that floatables in the environment are not due to any upset at the DITP. He thinks that as long as there is surface water sampling, that monitoring at the plant is unnecessary. S. Lipman pointed out that most of the floatables are from stormwater drains, not from the DITP. He is not sure how sampling in the bay or harbor will give any relation to plant operations. Results from the MWRA, EPA, and MADEP floatables/CSO program show that clearly. L. Schafer agreed that there probably is no relationship but he thinks there should be some baseline data before the outfall goes on-line in order to compare pre- and post-discharge conditions. S. Lipman doubts there is a relationship.

B. Robinson asked whether the scum and sludge measurements include a high percentage of water. M. Mickelson replied that there is a fair amount of water in the samples but the results are more a measure of plant performance. C. Hunt pointed out that MWRA also quantifies how much debris is captured in the screenings. M. Mickelson added that this information is reported in the monthly discharge report and the units are “tons per day”.

H. Trulli described the field sampling methods. Battelle is using a similar sampling protocol to the EPA headquarters program that studied 20 harbors. A net is deployed on the surface in transit between two stations and an estimate of sampled surface area is made. S. Testaverde asked how many gallons of water that represents. H. Trulli replied that they are not estimating volumes since the net skims the surface, not completely submersed. S. Testaverde thinks that calculating a gallon estimate is possible and would be useful for comparison to the volume being discharged by the outfall. M. Mickelson said that they could do that but would also have to factor in dilution.

B. Beardsley asked how many tows are made. C. Hunt replied that there are at least two floatables net tows in the nearfield region. A. Solow asked what types of numbers arise from the sampling. H. Trulli replied that the survey team has not found anything yet but would record the number of plastic and paper items. A. Solow asked how long this sampling has been done. C. Hunt replied that Battelle began sampling at the new outfall site last year. Historically, only observations of floating debris have been recorded.

R. Isaac asked how ambient monitoring of floatables would discern trash from ships or other sources as opposed to the DITP. He suggested attaching a video camera to one of the skimmers.

M. Mickelson thinks that this sort of sampling should be event-driven, if there is an obvious problem. H. Trulli added that as part of a separate program, Battelle sampled material at the Nut Island and Deer Island Treatment Plants about 10 years ago so there is some information on the types of floatables present. R. Isaac agreed that we know what to expect but it is still difficult to extrapolate the sources. S. Lipman still thinks it would be difficult to tell whether floatables in Massachusetts Bay were from the DITP, CSOs, stormwater discharges, ships, or other sources. M. Mickelson agreed and therefore does not think there should be an ambient threshold for floatables, but MWRA is willing to summarize the field net information in the annual Outfall Monitoring Overview report.

B. Beardsley thinks that it is not clear that the sludge and scum measurements are a good measure of changes in influent composition. M. Mickelson pointed out that these measurements are not a direct indication of plant efficiency. The graph of sludge and scum removal merely shows that the treatment plant is functioning well. S. Testaverde suggested using the field sampling net within the plant at a specific location for a specified amount of time. M. Mickelson thinks this could be done as a special study.

S. Redlich thinks that there should be records available of any bypasses of secondary treatment as measurements of plant overload. M. Mickelson believes that would not address the aesthetic issue. B. Beardsley asked whether it would be feasible to conduct a simple study when secondary treatment is bypassed (e.g. during a storm). M. Mickelson thinks that it may be possible even though the skimmers are not bypassed. B. Beardsley suggested this because in order to be able to make any causal relationship, there needs to be a measurement of any floatables discharged during times of upset. M. Mickelson agreed and thinks that this would be especially useful in the winter when the plume surfaces.

C. Hunt asked OMSAP whether the type of information presented here, as well as field data, are sufficient to address the question of aesthetics. A. Solow thinks that these data do not give an absolute measure of the plant performance, only relative performance, but how that corresponds to what is being discharged is not clear. He asked if there is there anything else MWRA can do short of the original proposed sampling that is dangerous and difficult. M. Mickelson thinks MWRA could sample within the plant to “fingerprint” the types of material and relate it to the materials sampled in the field. MWRA can also examine plant records to pinpoint any problems.

B. Beardsley asked whether floatables can be seen flowing in the effluent. M. Mickelson replied no, only the materials captured by the skimmers are visible. B. Beardsley suggested placing a camera in the plant at a place where the flow is slow. M. Mickelson thinks that since the material is sparse, the best place for a camera would be in one of the tip-tubes that accumulates material. As material accumulates upstream, the tip tube rotates slowly, fill up, rotates back up, and is flushed with effluent. The problem is that tube flushing time is not well quantified.

J. Pederson thinks the question is if a lot of material is observed in Massachusetts and Cape Cod Bays, is it because of a plant upset. MWRA could use a video camera in the event of a plant shutdown and material has an opportunity to escape, but she wondered if it would be worth the effort. She thinks that MWRA is conducting more than enough monitoring to address aesthetics. B. Beardsley agreed. He thinks obtaining some baseline nearfield data is a good idea to contrast

with post-discharge and if the efficiency of the plant ever drops. He suggested that there be sampling if there is a problem as opposed to random sampling and is in favor of dropping the current threshold. A. Solow agreed and asked how the rest of the group felt.

J. Shine asked whether the transect is chosen randomly each time. C. Hunt replied that it is semi-random – location depends when the crew has time. Sampling transects are straight lines between stations. J. Shine asked if there is bias between transect stations. C. Hunt does not think so, his sense is that it will become a fairly standardized set of stations over time.

A. Solow liked B. Beardsley's suggestion of some kind of adaptive sampling and asked whether that would require some kind of special surveys. C. Hunt stated MWRA has an adverse condition survey that is set up to sample if there is ever an adverse activity at the plant. Protocol for this type of survey is still being developed. K. Keay added that the adverse condition is part of the Memorandum of Understanding with the MADMF, primarily gauged at collecting coliform samples. Net tows could potentially be incorporated to this sampling.

J. Schwartz suggested that any OMSAP motion include whether or not they think that MWRA's treatment plant measurements and field observations will adequately address the permit requirements for aesthetics. M. Liebman read the permit requirements [see page 7 of the NPDES permit]. J. Shine approved of the field sampling, as long as there is no bias in station/transects selection. A. Solow agreed and thinks MWRA should be willing to continue to make and report those measurements. B. Robinson thinks that the surrogate measures of plant performance (FOG, slum and scum) are the best measurements possible and should be sufficient to address aesthetics. M. Mickelson summarized that OMSAP is requesting that MWRA report the sludge, scum, and FOG measurements in the annual Outfall Monitoring Overview; that MWRA address spatial concerns if there is an adverse plant event; and "fingerprint" the floatables from the DITP as a special study. A. Solow put forth this statement as a motion which OMSAP members approved unanimously.

ACTION: OMSAP believes that the current measurements of sludge and scum removed by the Deer Island Treatment Plant (DITP) as well as FOG (fats, oils, and grease) measurements in the treated effluent adequately address the NPDES permit requirements for aesthetics. OMSAP recommends that MWRA delete the current floatables threshold in the Contingency Plan. OMSAP is requesting that MWRA report the sludge, scum, and FOG measurements in the annual Outfall Monitoring Overview; that MWRA address concerns regarding floatables if there is an adverse DITP event; and that MWRA "fingerprint" the floatables from the DITP in a special study.

DISSOLVED OXYGEN (DO) CONCENTRATION THRESHOLD DISCUSSION

M. Mickelson reviewed the current DO thresholds and suggested revisions [see information briefing dated February 23, 2000]. He then showed baseline data in order to give an idea of the types and frequencies of exceedances that are occurring during the baseline period. DO varies with time and is affected by a number of physical factors. For example in the fall of 1994, there was low bottom DO in Massachusetts Bay, and also low DO in bottoms waters entering from the Gulf of Maine implying a boundary influence. M. Mickelson then showed all farfield and nearfield baseline DO data, as well as survey averages. As seen in the data, MWRA focuses on

the nearfield since this is where low DO measurements generally occur. He then showed 1994 data in detail which showed that at a given station, DO can vary by 1 mg/L within just one day (actual data, not averages). Overall, there is a lot of temporal variability that is not well characterized.

EPA has developed a new draft guidance document for DO in salt water for the Virginian Province (Cape Cod to Cape Hatteras). The MWRA 6 mg/L warning threshold is based on a freshwater criterion and the new EPA guidance document states that a salt water criterion is different. This is based on acute and chronic survival studies of representative species to the various exposures. From these studies, EPA determined that a 4.8 mg/L DO threshold concentration would be protective. The document goes on to discuss how to take temporal variability into account. EPA implies that this may be used as a national criterion. D. Tomey clarified that EPA will look at how the new threshold could apply to other areas. M. Liebman added that the guidance is on the web, and is open for public comment. There is an appendix that describes how to apply the criteria to other regions. [Available at: <http://www.epa.gov/ost/standards/dissolved/> EPA HQ] D. Tomey added that he and M. Liebman have yet to see a draft of the appendix. J. Pederson pointed out that Scott Nixon (OMSAP member) believes that the salt water standard for DO should not be based on the freshwater standard.

B. Robinson asked whether the chronic values considered mortality. M. Mickelson replied that they are based on growth. He then showed Bays Eutrophication Model results that show some improvement in DO in Boston Harbor and minor DO variations at the new outfall location due to relocation and secondary treatment. MWRA has exceeded the existing DO threshold a few times during the baseline period.

D. Tomey stated that the draft EPA guidance will go to public notice, federal register, and national review, but this report only pertains to the Virginian Province. A. Solow said that OMSAP should treat this as a “heads up” that there will be EPA action on the DO national criteria soon and that OMSAP will have to then re-examine the MWRA DO thresholds.

REPORT FROM THE PUBLIC INTEREST ADVISORY COMMITTEE

G. Grossman updated the group on recent PIAC membership changes. She introduced herself as the new chair, replacing Cate Doherty, also from Save the Harbor/Save the Bay. Other changes: Steve Tucker and Margaret Callanan [correction, John Lipman] replaced Jim O’Connell and Patty Daley from the Cape Cod Commission and Maggie Geist replaced Scott Mitchell from the Association for the Preservation of Cape Cod. At the last meeting (October 1999), PIAC reviewed the conclusions from the OMSAP workshop. There was also a lengthy discussion on the group’s public communication strategy and effective ways for us to reach out to constituents and disseminate information about the outfall pipe activation. PIAC considered hosting a public meeting or briefing and discussed the timing in relation to when the outfall pipe goes on-line, meeting locations, appropriate format and content, level of technical information, and who should be invited. PIAC did not agree as a group to host a public meeting, and so the committee discussed other mechanisms of public outreach such as hosting a website and radio talk shows. PIAC also discussed the Barnstable Science Advisory Panel’s (BCSAP) alternatives to MWRA’s

food web model scope of work. The BCSAP is continuing their work on this and will hopefully be presenting in the near future.

REPORT FROM THE INTER-AGENCY ADVISORY COMMITTEE

S. Testaverde gave a brief IAAC update. The committee convened in May 1999 and October 1999. IAAC is still grappling with its mission, and is moving for a modification of its mission in the OMSAP charter that states that the committee will advise the OMSAP on environmental regulations. IAAC approved a new mission statement that states that “the committee will advise the OMSAP, EPA and MADEP on scientific, technical and/or regulatory matters related to discharges from and operations of the MWRA system outfalls that may directly or indirectly affect Boston Harbor, Massachusetts Bay, and Cape Cod Bay. The IAAC may review or evaluate other environmental matters as necessary.” IAAC will forward this request to EPA/MADEP. IAAC has also had discussions on the food web model scope of work, nitrogen removal technologies, and polymer usage in secondary treatment.

C. Hunt asked whether the charter mission statement revision will go through public review. S. Testaverde was not sure but will find out when he meets with EPA/MADEP. M. Liebman added that Ron Manfredonia (EPA) had said that EPA/MADEP will approve charter changes, not OMSAP, since these agencies appointed OMSAP and its subcommittees.

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MEETING HANDOUTS:

- Agenda
- OMSAP/IAAC/PIAC Membership Lists
- March 1999 OMSAP Minutes
- MWRA Information Briefings
- Copies of MWRA Presentation Transparencies
- Draft March OMSAP Agenda