FINAL REPORT

# Peer Review (Independent Technical Review) of The Massachusetts Estuaries Project Report on the Pleasant Bay System

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June 2009

# Peer Review (Independent Technical Review) of the Massachusetts Estuaries Project Report on the Pleasant Bay System

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**June 2009**

**Prepared for:**
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**Town of Orleans**
**19 School Road**
**Orleans, MA  02653**

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# EXECUTIVE SUMMARY

This report presents the Final results of an independent technical review (peer review) conducted on the Massachusetts Estuaries Project Report for the Pleasant Bay System in the Town of Orleans. This work was conducted under a contract to the Town of Orleans, awarded to the Woods Hole Group after a competitive Request for Proposal (RFP) process. The contracted scope of work included three (3) tasks, each with a separate Task Manager. The Task Managers were selected to offer the Town the highest level of expertise and objectivity.

- **Task 1. Benthic Flux Measurements and Analysis of Mechanisms**; Task Manager - Dr. Jeffrey C. Cornwell, Chesapeake Biogeochemical Associates, Inc. and the University of Maryland CES
- **Task 2. Hydrodynamic and Water Quality Models**; Task Manager – Kirk F. Bosma, P.E., M.C.E., Woods Hole Group, Inc.
- **Task 3. Health of Eelgrass and Benthic Community** – Dr. John M. Teal, Teal Partners, Ltd.

Additional technical support for the independent technical review was provided by Dr. John Trowbridge (Senior Physical Oceanographer/Ocean Engineer, Woods Hole Group and Woods Hole Oceanographic Institution) and Dr. Heidi Clark (Environmental Scientist, Woods Hole Group). Bob Hamilton (V.P. and Coastal Engineer, Woods Hole Group) was the overall Project Manager for the contract. Brief biographical sketches on each Task Manager and the technical team are presented in Appendix A. Written comments on the Draft Report received from the Wastewater Management Validation and Design Committee are provided in Appendix B. Some comments are addressed directly in Appendix B, and others resulted in modifications to the main body of this Final Report.

In addition to this Executive Summary, the report contains an individual section on each Task area. Each Task Manager prepared an overall review in accordance with the scope of work. The scope of work is stated in the report for each Task, and the ensuing comments and findings were prepared by the Task Manager to independently address the key issues outlined in the scope. In addition to the independent technical review, each Task Manager prepared a response to certain comments/questions (i.e., Chapter Reviews) prepared by the Wastewater Management Validation and Design Committee based on its review of the MEP report before the peer review contract was issued. Although these questions did not provide the basis for the independent review, the peer review team felt it was important to address relevant questions to ensure the stakeholder interests were addressed. Only comments within the scope of the review and the expertise of the reviewers were addressed.

This peer review generally assumes the reader is familiar with the MEP document and process for the Orleans portion for Pleasant Bay. Although a summary of the document and general scientific processes is not within the scope of services for the review team under this contract, this review includes some discussions to clarify some of the key issues. For instance, Section 1.3 summarizes biogeochemical processes, Section E.4 summarizes some of the components of the MEP linked modeling approach, and Section 3.1 summarizes the influence of bottom oxygen on benthic populations, including eelgrass.

### E.1 PURPOSE/EXPECTATIONS

As stated clearly in the RFP, the Town is seeking to understand the degree of accuracy and certainty that may be derived from the MEP report, the importance of individual components, and the efficacy of utilizing the report for watershed-based planning. Before committing to a large wastewater infrastructure project, the Town is seeking:

- A peer review of the School for Marine Science and Technology at University of Massachusetts Dartmouth (SMAST) work to validate that the calculated reductions in nitrogen are required to significantly improve water quality and habitat.

- To determine whether the methodology is sufficiently flexible to accommodate refinements and adjustments for unpredictable, naturally-occurring changes in nitrogen concentrations (e.g., effects of the breach that occurred after the MEP report was published).

This Final Report summarizes the objective findings and opinions of the experts who reviewed the MEP reports. The information presented herein is intended for review and consideration by the Town, including the Town Staff, the Wastewater Management Validation and Design Committee, other stakeholders, and the public. The Woods Hole Group review team had limited access to data, and no access to the models and analysis methods employed by SMAST when preparing the peer review. Not having access to this information, particularly the data and models, introduced challenges to the peer review process, which more traditionally would involve open sharing of information and data. Therefore, the findings presented herein are based solely on a review of the MEP document, supporting published documents, the scientific literature, and the wealth of collective experience shared by the review team. The review team has extensive relevant knowledge and has formed objective ideas and recommendations, but it is not possible for the team to be expert with all facets of Pleasant Bay, nor can the team address all questions and concerns shared by the stakeholders. Instead, we present opinions to the best of our ability based on experience and the information available for review.

The findings presented in this Final Report were developed through a process of first preparing a Draft Report independently and objectively without influence or input from other stakeholders. Initial comments on the Draft were then received from the Wastewater Management Validation and Design Committee as the basis for discussion at a public meeting on May 7, 2009. Slide presentations were made at the public meeting, which are available along with a video of the meeting on the Town website (http://www.town.orleans.ma.us/Pages/OrleansMA_BComm/WMVD). Formal written comments were then received from the Committee after the public meeting, which are included as Appendix B. Since the Woods Hole Group team worked for several months on the Draft, while other stakeholders, including the MEP team had worked on Pleasant Bay for years, it was anticipated that meaningful discussions would result through sharing and discussing the Draft Report. Substantive input was obtained from stakeholders, and information evolved between the Draft and Final Reports.

The level of conclusions and recommendations also evolved from the Draft to the Final Report. Our goal for the Final Peer Review to be as closed-ended as possible was made challenging by the lack of available data and model information. Certain recommendations presented herein encourage more in-depth analysis and data collection; however, the peer review is not intended to recommend more studies that would impede progress. Since action is required on behalf of the Town to respond to TMDL requirements, the final recommendations are geared toward supplemental activities that can be completed in parallel and to optimize the ongoing wastewater facilities planning process.

Based on the work conducted by the independent technical review team, there are a number of key issues the peer review is intended to help the Town address:

- **Federal requirement for action**: The US EPA has mandated establishment of Total Maximum Daily Loadings (TMDL) for stressed Massachusetts estuaries. The Orleans portion of Pleasant Bay is one of many estuaries (89 have been identified) in the Commonwealth that have suffered serious environmental degradation as a result of excessive nutrient loading. Water quality is degraded and sensitive habitats have been lost, resulting in adverse environmental, recreational, and economic impacts. The US EPA has charged the Massachusetts Department of Environmental Protection (MA DEP) with the responsibility to develop and enforce the TMDLs. There is no choice but to take action to reduce the nutrient loadings. The Federal government requires action, and failure to comply will result in consequences. <u>It is our intent that the results from the peer review can improve the approach the Town will take to comply with its mandatory charge.</u>

- **So much at stake**: The solutions to meeting the TMDLs and reducing nutrient loadings are substantial. Various measures are contemplated for the overall solution, and the primary method is the reduction of direct inputs of nutrients from wastewater, which will likely require some level of sewering. Town or regional wastewater treatment facilities and infrastructure are extremely costly. In the case of Orleans, budgets reportedly exceed $150M, which represents a public works project an order of magnitude higher than previous Town endeavors. Financial impacts to the Town and its citizens are significant. <u>It is our intent that this independent technical review will help the Town optimize its approach to complying with the EPA requirement, potentially saving substantial resources in the future.</u>

- **Multiple Key Assumptions are Required**: There is not a simple and direct solution for achieving the results intended from implementing the TMDLs required by EPA. The desired effect is improvement of water quality and re-establishment of productive benthic communities and habitats (e.g., eelgrass). However, there is not a direct one-to-one correlation between reducing nutrient loadings and growth of eelgrass. There are a number of key assumptions, or leaps of faith, involved, which require intermediate and linked factors to occur, including:

- *Reducing nutrient loading from septic systems will result in a reduction in the nutrient levels in the estuary* – There are a variety of nutrient sources to Pleasant Bay, including groundwater from septic systems, atmospheric, fertilizer, sediments at the bottom of the Bay, and others. There needs to be certainty that septic systems are a primary source of nutrients, and that reducing septic sources will produce a meaningful reduction in the source of nutrients to the Bay as compared to other sources. There also needs to be confidence in the understanding of groundwater flow patterns, such that reduction of septic loads is achieved in areas that truly affect the quality of the groundwater flow to the Pleasant Bay estuary.

- *Reducing nutrient levels in the estuary will improve water quality, including increasing dissolved oxygen levels in the water* – High nutrient levels affect water quality in multiple ways. Algal production is increased, which shades the bottom, thereby decreasing photosynthetic activity among benthic plants including eelgrass, and decreasing dissolved oxygen in the water. The excessive algal blooms also produce increased biomass loading to the sediments, which can produce hypoxic or anoxic conditions when the dead algal matter decomposes. There must be confidence that the decrease in nutrient loading is sufficient to minimize algal blooms and the associated adverse eutrophic conditions.

- *Reducing nutrients and raising dissolved oxygen levels in the water will result in healthier benthic communities, including re-establishment of eelgrass beds that existed more than fifty (50) years ago* – Benthic communities, including invertebrate organism diversity and abundance and eelgrass, are affected by numerous factors, including excess nutrient loading, boat/mooring impacts, sediment transport, disease, and other factors. There must be confidence that the increased water quality resulting from the nutrient TMDLs will be sufficient to allow the benthic community to recover in spite of the other stresses. There also must be confidence that the substrate, system hydraulics, use patterns, and other factors are suitable for recolonization.

- *The anticipated benefits will be realized within a reasonable timeframe* – The results will not be immediate. Reducing septic nutrient sources today would start to improve the groundwater quality, but, depending upon the location of the septic source and precipitation trends, it could be many years before the existing nutrient load from septic systems reaching the Bay is actually reduced. Groundwater flowing into the Bay today is derived from many years of loading under current conditions. It will also take many years to actually implement the wastewater infrastructure. Furthermore, decades of septic nutrient load also resides within the bottom sediments, which results in a benthic flux of nutrients to the water column. The sediments can be cleaned up over time as well, but this will be an indirect result that occurs after the groundwater source is reduced. Consequently, there must be clarity and patience regarding the timeframe of expected improvements, and the public and regulatory compliance expectations must be realistic.

For the desired effect of the TMDL project to be realized, all of these key assumptions must come true. <u>We intend that the results from the peer review will help address these key assumptions, and add confidence regarding the long-term anticipated benefits. The review also helps clarify the uncertainty regarding the extent and rate of recovery of the Pleasant Bay ecosystem following nutrient load reductions.</u>

- **Complex technical matters/lack of transparency**: The MEP work was conducted by qualified, experienced scientists and engineers, who exercised careful professional discretion when performing the work. The work also was completed according to an established protocol and quality assurance plan. For these reasons, there is a certain level of confidence in the MEP work. However, the work is complex, many assumptions are required, and there is potential for significant interpretation and uncertainty. Pleasant Bay also is complex, and the standard MEP approach is not sufficient to describe certain processes in this particular estuary. There also remains an unfortunately contentious matter regarding the public availability of the SMAST data and models, which results in a lack of transparency. Furthermore, the MEP reports, while extensive, do not detail all the assumptions, analysis techniques, and implications/sensitivity of the analysis to the input parameters. Even for the trained expert, some elements of the analysis remain unclear. <u>The independent review presented herein is intended to help clarify some of the complex technical matters that are confounding the Town when interpreting and acting on the MEP report.</u>

- **Public opinion**: As a result of the high stakes, investment required to comply with the TMDLs, complexity of the technical issues, and lack of transparency there is some inherent level of skepticism in the process. There also may be the natural tendency to avoid major public expenditures. Given the complexity of the matters, there also is opportunity for various stakeholders to develop conclusions that may be inconsistent or at odds. The information presented within this peer review in some ways helps confirm and boost confidence in the findings of the MEP report, but also identifies, in some cases, significant potential sources of uncertainty and bias in the results. We understand that some stakeholders will place more emphasis on different parts of our review. While this peer review might ideally help resolve all stakeholder concerns/differences that exist, this is not a reasonable expectation. This peer review merely reflects the educated opinions and findings on the three scope areas identified in the RFP. This peer review is focused on objective science, and it not intended to respond to, or resolve, public opinions. The peer review is also not intended to critique the MEP as a whole. Information presented herein should be considered only in the context of Pleasant Bay.

## E.2 OVERALL SUMMARY OF THE INDEPENDENT TECHNICAL REVIEW

Overall, the team's findings can be summarized as follows:

- MEP is a high quality and necessary endeavor, and is conducted by qualified and experienced scientists, engineers, and planners. There are serious issues facing the Orleans portion of Pleasant Bay that threaten the environment, property values, fishing livelihoods, and general enjoyment of this valuable natural resources. There is a need for action, both to address local concerns and to comply with Federal and State TMDL mandates.

- The MEP report for the Orleans portions Pleasant Bay represent a strong foundation for developing a course of action to develop and comply with site-specific TMDL requirements, but may not provide all necessary information for Pleasant Bay depending upon the Town's tolerance for risk and uncertainty.

- We understand that sewering is one contemplated future solution for meeting TMDLs within Pleasant Bay, which is a costly course of action. Defensible science and efficient engineering solutions are required to optimize the level of action required. Even small uncertainties and variability in the MEP findings can result in large financial implications for the solution. Recommendations are provided in this peer review to identify and minimize uncertainty.

- Some of the standard MEP methods are not considered state-of-science for a system like Pleasant Bay. Given the value of the Pleasant Bay estuary and potential cost of contemplated solutions, the peer review team would perform certain aspects of the analysis using more rigorous methods, which would better characterize the complexities of Pleasant Bay.

- In spite of the voluminous nature of the reports, the contents fail to characterize the overall estuary processes (e.g., circulation processes, meaningful model/data comparisons, and layman's overview of the data and model results). The report also overly complicates certain matters, but does not document basic analysis techniques, assumptions, and associated implications and uncertainty.

- The peer review team identified a number of technical limitations associated with the analysis that introduce uncertainty, and in some cases may bias the results. Although there are potential biases in both directions, the MEP results are likely to be conservative in that the nitrogen inputs to the model may be biased high. It is also possible that a positive environmental response could exceed expectations if the TMDL is met (subject to more sophisticated analysis necessary to resolve remaining uncertainties).

As an overall comment, one must recognize there are inherent limitations to the MEP process for Pleasant Bay. At present, there is a relatively small qualified team headed by SMAST responsible for evaluating Pleasant Bay along with every other stressed estuary in MA. There are 89 estuaries, which results in a significant effort given the level of detail and number of technical steps required to complete a study for each estuary. From a scientific perspective, it is unreasonable to expect one small group to have intimate knowledge for all estuaries. From a management perspective, it also is unreasonable to expect one small team to place the priority level on each estuary that the local stakeholders demand for making planning decisions involving heavy public investment.

To achieve overall scheduled statewide requirements for the MEP, certain protocols and a standardized process needed to be established to satisfy schedule and budgetary constraints. The protocols may not be ideal for all estuaries, and Pleasant Bay is a particularly complex estuary, with large public infrastructure implications, that warrants more detailed consideration. The MEP work represents a substantial basis, but does not represent the optimal solution for Pleasant Bay in the opinion of the peer review team (depending upon stakeholder tolerance for risk and uncertainty). With the high stakes involved, local stakeholders may demand more attention, more scenarios, and in some cases more rigor than the SMAST team can possibly offer. The Town may demand a "no stone left unturned approach" and have expectations that are unreasonable for SMAST to achieve within its contracted scope and available resources. In this regard, although there are certain areas where the work could have been approached or presented differently for Pleasant Bay, many of the findings of this peer review should not be interpreted as flaws or errors with the MEP or SMAST work. Rather, the findings represent site-specific recommendations to better understand and minimize uncertainty for Pleasant Bay to supplement the scope of work provided by MEP.

As a possible solution, we recommend the Town proceed with more detailed supplemental analysis where warranted. This peer review offers specific suggestions for consideration. However, the lack of public access to the data and models prohibits this level of diligence on behalf of the Towns. Decisions on complying with EPA mandates, then, either will be postponed, proceed in the absence of sufficient information, or fail to be made due to lack of public support and consensus. For this reason, we feel strongly that the data and models should be made available to the Towns and public. The information was compiled with public funds by a public university using commercially available products and models that can be applied by other qualified groups in addition to the SMAST team. If the Town is unable to work with SMAST to perform supplemental work, or obtain necessary information to conduct the supplemental work itself, then a more comprehensive program would be required on behalf of the Town to obtain data independently.

Summaries of each of the three (3) Task areas follow.

## E.3 TASK 1 BENTHIC FLUX REVIEW

The Task 1 independent technical review of benthic flux measurements and analysis of mechanisms revealed both strengths and limitations with the MEP report. Notable strengths include the spatial resolution of the measurements, explanation of the cause/effect of nitrogen loading on the retention and remineralization/recycling pathways, and the quality of the experiments and measurements.

The fundamental over-arching concern is the general absence of data presented in the report, and the lack of public access to this information. Having access to the data would allow for better assessment of the overall utility of the work. In spite of the wealth of information collected, the report reveals remarkably little about the benthic biogeochemistry of Pleasant Bay. Perhaps it was not within the SMAST scope to explain the processes, or it was not anticipated that the stakeholders would have the interest and technical background to benefit from this information.

From a technical perspective, the primary limitations are:

- All of the cores were incubated in the dark, resulting in the absence of consideration of benthic microalgal effects on sediment nitrogen fluxes. It is well documented that photosynthesis on the sediment surface alters nitrogen fluxes. Daytime fluxes are likely to be much lower than those in the dark in areas where light reaches the bottom (i.e., much of the shallow Pleasant Bay system). On a whole system basis, it is possible that the overall rates could decrease 1/4 to 1/3, but it must be emphasized that we have insufficient access to data to make a defensible prediction.

- The information presented in the MEP report for benthic flux is not directly comparable to similar studies at other sites. The information includes a modeled nitrogen depositional term, does not break the sediment-water exchange into distinct species, and presents no accompanying data on fluxes of soluble reactive phosphorous and nitrogen.

- An overall nitrogen mass balance is lacking for the system.

- The estimation of non-summer flux rate was not made; this does not mean that the predicted rates in other seasons are unreasonable, but rather that they are likely poorly constrained. The model relationship with a mid-summer nitrogen flux peak is similar to observations in other systems, but adds uncertainty to the flux numbers. It is not certain whether the hypothesized seasonal change in net nitrogen retention in these shallow water estuarine sediments is correct, and supporting data do not appear to be available regionally except in wetlands.

- The water quality model includes a relatively simplistic model of benthic flux that may underestimate the improvement in sediment nitrogen efflux with decreasing external loading. The model assumes a linear relationship between reduced benthic flux and reduced septic load. However, the response is likely to be non-linear and multiplicative. For instance, the No Septic Loading Scenario (no anthropogenic loading) results in a decrease of watershed loading of 50% and a benthic flux decrease of 35%. Due to a variety of factors discussed in the Task 1 peer review, the system may respond more effectively to reductions in the nutrient load than described by the MEP model (i.e., benthic flux rates may decrease more rapidly than assumed in the modeled scenarios).

- The model does not simulate dissolved oxygen directly, which is the primary parameter that will affect benthic restoration in nature. Supplemental measurements and modeling would afford better predictions.

Another key finding is related to the "memory" of the sediments. From a biogeochemical perspective, most of the nitrogen remineralization in sediments comes from recent inputs of organic nitrogen (e.g., algae, sea grasses). Under summer conditions, this nitrogen is rapidly depleted, and, although there is some long-term residual, it should not fuel substantial fluxes in future years. Changes in loading will rapidly be reflected in changed fluxes from the sediments, suggesting the recovery time for sediment flux from the sediments will be shorter once the nitrogen load that reaches the estuary is reduced.

## E.4 TASK 2 HYDRODYNAMIC AND WATER QUALITY MODELING REVIEW

The Task 2 peer review focused on the hydrodynamic and water quality models as applied to the Pleasant Bay Estuary. The hydrodynamic model (RMA-2) simulates the flow of water within the estuary as caused primarily by the rise and fall of the tide. The water quality model (RMA-4) is coupled to the hydrodynamic model, and simulates the transport of waterborne constituents (i.e., nitrogen and salinity) within the estuary. Through a process called model calibration, the hydrodynamic model was compared against tide measurements collected within the system to ensure the model simulated natural processes, and subsequently verified to current observations at a couple of locations in the estuary. The water quality model was compared against measurements of nitrogen for calibration, and then was verified against measurements of salinity. It is notable that the water quality model was applied to simulate bioactive nitrogen for Pleasant Bay instead of total nitrogen, which differs from all other MEP studies conducted by SMAST. The models applied for the Pleasant Bay system are the same models applied for other Massachusetts Estuaries Project sites. The computer programs required to run the models are commercially available, and can be installed and run on a personal computer (PC).

The first fundamental purpose of the modeling is to simulate the nitrogen distribution within the estuary for present-day, existing conditions. Models allow for development of a more detailed picture of existing conditions than can reasonably be afforded to measure. The second fundamental purpose of the modeling is to simulate future scenarios, including future build-out scenarios, and alternatives for improving upon existing conditions, such as nutrient reduction alternatives (e.g., sewering scenarios) and changes in flushing dynamics (e.g., inlets or breaches). It is important to recognize that the RMA-2 and RMA-4 models do not simulate regional groundwater or surface water hydrology. Instead, assumptions are input to the model to account for these processes and others.

Therefore, if a certain sewering alternative is modeled, the effect on the models is to change an input parameter. The model does not directly simulate the process of removing a septic system. Various other spreadsheet models, calculations, and assumptions are required as an interim steps in the MEP linked-modeling approach. These other steps in the MEP process were not considered as part of this peer review.

The overall modeling approach is well conducted and the hydrodynamic model appears to be reasonably calibrated and verified. As with the benthic flux sections of the report, an overall comment is that the information and data presented in the MEP report are relatively sparse. Although the overall modeling approach is well-described, there is little transparency regarding the interim analyses and assumptions that are required to develop model inputs/boundary conditions and to interpret model results. Lacking the majority of the data, analysis, and any access to the models, it was not possible to conduct a true scientific peer review. Opinions were formed based on the material in the report, related documents, some supplemental analysis, and experience with similar models and estuarine systems.

Another overall comment on the modeling is based on a review of supplemental MEP documents. There was apparently an in-depth review and sensitivity analysis of the linked modeling approach as a whole (of which RMA-2 and RMA-4 represent steps in the process). There was no consideration, however, of any other models that might be supplemented into the linked modeling approach instead of RMA-2 and RMA-4. In the experience of the review team, it has been shown that one model is not suitable for all sites. It is possible for Pleasant Bay that an alternative hydrodynamic and/or water quality model might be more appropriate to address either more complex physics or to address more detailed local questions. If the MEP process requires a standardized model, than perhaps a more rigorous supplemental approach could be pursued by the local stakeholders. This would be much expedited and more cost-effective with access to the SMAST data.

The independent technical review of the modeling focused on three (3) major areas: 1) the hydrodynamic model; 2) the water quality model; and 3) the breach. Overall, it is believed that the hydrodynamic model does a good job of simulating depth-averaged, typical tidal flows within the estuary. RMA-2 is a useful model for this purpose, and has proven to calibrate well to tidal hydrodynamics in many estuaries. There are certain limitations of the hydrodynamic modeling, however:

- There is a noticeable lack of explanation of overall system circulation dynamics that could be prepared based on RMA-2 output, such as screen shots or animations of currents. An overall description of the physical oceanography of the system should be possible based upon the available measurements and model results, and would help the public understand the key issues, limitations, and possible solutions.

- The modeling also is two-dimensional and averages all processes over depth. This is a standard practice for MEP, which may not be applicable to Pleasant Bay in certain locations. There were no data observations taken to either verify or discount the importance of potential 3-D processes within the system. Analysis is presented herein exhibiting meaningful stratification in Areys Pond, for instance. Failure to consider 3-D processes may bias the results, and significantly impact the nutrient concentration levels.

- There is an absence of tidal measurements available for calibration in some of the upper embayments (e.g., Areys Pond, Paw Wah Pond, and Kescayo Gansett Pond). Relying on the model to simulate tides in restricted areas adds uncertainty, and supplemental measurements would help resolve this uncertainty.

- There are culverts within the estuary that are simulated in the model, but it is not clear how these were represented in the model. RMA-2 is not ideally suited for simulating flow through constricted structures; better models and calculation methods exist.

- The tidal residual is relatively large. At the fish pier for instance, the tidal residual is more than 1 ft. This accounts for a significant fraction of energy that can influence circulation. It is unclear if this residual was completely generated from short-period, non-deterministic processes. Since the model is run for tidal constituents, not including this level of energy may underestimate flushing potential in certain locations.

The peer review of the water quality model resulted in the most critical aspect of the MEP report for Pleasant Bay. Although the RMA-4 model was applied by qualified personnel according to an accepted quality assurance program, the RMA-4 model does not represent the state-of-science for nutrient modeling, particularly for complex systems such as Pleasant Bay. Basically, the currents from RMA-2 are derived separately and input to RMA-4; there is no dynamic interaction between the hydrodynamic and water quality models. The input terms for septic, watershed, and atmospheric loadings of nitrogen are constants, not accounting for any of the inherent dynamics in the nutrient cycle. Specifying the constant inputs to RMA-4, particularly for benthic flux, is simplistic and requires many assumptions. The assumptions generally result in added uncertainty and may introduce conservatisms inherent to the analysis. Specifically, for simulation of future sewering scenarios, the benthic flux term is scaled linearly with the reduction in nutrient loading. Nutrient dynamics would suggest the reduction in benthic flux may be more substantial. Combined with the 2-D nature of the model, the basic theory behind RMA-4 is overly simplified for a complex system like Pleasant Bay. More sophisticated models exist that are more routinely applied for TMDL studies.

Other key findings regarding the water quality model are:

- There is a unique approach applied for Pleasant Bay that focuses on bioactive nitrogen instead of total nitrogen, which is the focus for other MEP reports. To the review team's knowledge, no other estuaries have been addressed this way (other than for Chatham's Bassing Harbor), making the Pleasant Bay work unique. There is no explanation of how boundary conditions (e.g., sources from septic, watershed, atmospheric, benthic flux) may have been altered or background nitrogen levels may have been modified to simulate bioactive nitrogen instead of total nitrogen, which is the subject of other MEP sites. Analysis of more recent nitrogen data in the system from 2006 and 2007 also suggests the average background nitrogen levels may be lower (~20%) than the values used in the modeling. The RMA-4 water quality model for Pleasant Bay also was calibrated to nitrogen and validated against salinity, which is the opposite approach from many other MEP sites. That this analysis is so unique for Pleasant Bay warrants a more comprehensive consideration of implications to the overall TMDL process in the opinion of the review team. There also is some research suggesting that organic nitrogen (dissolved and/or particulate) may contribute to eutrophication in estuaries.

- Based on the information presented in the Pleasant Bay report, as well as information presented in other MEP documents, the RMA-4 model is strongly sensitive to the dispersion coefficients. These coefficients represent the primary calibration parameter that can be adjusted to ensure the model results match measured data. There is certainly more than one unique set of coefficients that would constitute a calibrated model. Other MEP reports suggest that a 50% reduction in dispersion coefficients can result in more than a 90% change to model nitrogen concentrations. Some of the coefficients in the Pleasant Bay model are nearly an order of magnitude (factor of 10) different than values typically recommended for use in RMA-4 users manual.

- The calibration of the RMA-4 model is relatively simplistic, and involves extensive time and other averaging. Although the averages compare reasonably well, it would be helpful to understand how well a time series of the model output compares with measured data (even for the spot measurements on the outgoing tide). This would indicate how well the model described the dynamic physics of the system. Careful examinations of the graphs that compare model results against data also reveal some inconsistencies with how the model and data results were compared. A more comprehensive sensitivity analysis for the water quality model would improve the overall confidence in the nitrogen concentration results, and reveal if there are parameters or input conditions that significantly influence the outcome of the modeling.

- RMA-4 is a finite element model, which by definition can have limitations with conserving the mass of water quality constituents depending upon how the model grid is developed. The report should acknowledge this limitation of RMA-4, and present results to demonstrate that the mass of nitrogen is conserved within reasonable margins.

The peer review of the MEP analysis of the breach in the barrier island suggests the RMA hydrodynamic model is sufficiently flexible to evaluate the impact of natural changes, such as the breach. There are substantial changes in circulation patterns as a result of the breach that result in improved flushing and commensurate improvements to water quality. Based upon the review team's familiarity with the coastal geomorphology in the region, and given the relatively long design life for the contemplated wastewater infrastructure, it would be overly optimistic to plan the wastewater infrastructure with a dependence upon the additional flushing introduced by the new breach. Unless an active inlet management plan is implemented to maintain the inlet, it will continue to evolve and migrate.

### E.5 TASK 3 EELGRASS AND BENTHIC COMMUNITY REVIEW

The Task 3 peer review of eelgrass and the benthic community included a detailed review of the MEP documents, the published literature, and data/information from other locations. The review concluded there are uncertainties and limited data regarding the extent of eelgrass decline and the replacement of diverse benthic infauna by opportunistic species in Pleasant Bay. There also is an absence of data on the extent to which wasting disease, herbivory, and boating activities contribute(d) to the decline in eelgrass in Pleasant Bay. These factors have also been generally unmeasured in other systems around the world where eutrophication has resulted in eelgrass declines, so there is little information available from other systems for comparison purposes. Although widespread losses of eel grass are not documented throughout the estuary and the relative influence of environmental stressors is not fully understood, there is little doubt that degradation of environmental conditions has occurred due to nitrogen-induced eutrophication in the innermost parts of Pleasant Bay. There also is the potential for future continued degradation. The extent to which the existing and future potential degradation warrants aggressive wastewater management controls is the subject of federal/state regulation, and the Town's risk tolerance, both of which are not within the scope of this peer review.

There also are limited existing data to document recovery of similar estuary systems in response to nitrogen reduction on a scale similar to Pleasant Bay. In spite of the uncertainty, the Task 3 peer review generally supports the findings of MEP with regard to eelgrass and benthic communities, and has confidence that benthic communities can recolonize and eelgrass beds can recover when nutrient loadings are reduced to the levels suggested in the Pleasant Bay MEP report. The review confirmed that eelgrass and the benthic community respond negatively to high nutrients, including nitrogen and bioactive nitrogen, in particular. High nitrogen reduces dissolved oxygen in the water column via algal production, shading, die-off, decomposition, and increased benthic flux of nitrogen into the water column. Low dissolved oxygen levels are well-documented to adversely impact the benthic community, including loss of submerged aquatic vegetation. Although there are other factors affecting the eelgrass habitat, including wasting disease, sand transport, and boating activity, decreased oxygen levels caused by excessive nutrient loading, and particularly anoxic conditions caused by eutrophication, have the primary adverse impacts on eelgrass in Pleasant Bay, particularly within the innermost portions. Benthic communities, including submerged aquatic vegetation, respond well to higher dissolved oxygen, and reducing nitrogen will increase dissolved oxygen, leading to improved conditions for benthic community diversity and eelgrass. In the few instances where there are data from salt and freshwater systems, once nitrogen is reduced, eelgrass in marine systems and SAV in freshwater systems recover. There is also evidence from larger ecosystems that levels of nitrogen recommended by MEP of about 0.14-0.16 mg N/l will result in ecosystem recovery.

## E.6 RECOMMENDED NEXT STEPS

As discussed above, action is ongoing and required by the Town of Orleans to comply with federal mandates for TMDLs. The peer review results and recommendations are not intended to slow the wastewater facilities planning process, or suggest that more study is required before more progress can be made. The peer review team strongly encourages the Town to make incremental progress, and recommends certain activities that can be completed in parallel to help understand and reduce uncertainty. Although there are technical limitations of the standard MEP approach that introduce potential biases for a complex system like Pleasant Bay, the MEP work provides a valid basis for planning purposes. There should be confidence in the overall solution path, particularly if site-specific enhancements can be implemented to reduce the uncertainty that exists for Pleasant Bay in the MEP report. With an optimized design, the response of the system to reduced nutrient loading might be better than anticipated. We believe the ongoing planning and design efforts should have sufficient flexibility to refine the MEP work without compromising the overall schedule. Since it will take time to complete the planning and design, and more time to realize the benefits, delays will only postpone the desired improvements.

On a parallel path, there is opportunity to quantify and reduce uncertainty, thereby building confidence in the results, optimizing the engineering design, and potentially saving public funds. This can be achieved through implementing the recommendations offered below.

### Benthic Flux Recommendations:

Although the current benthic flux data program has excellent spatial component for “dark” fluxes, particularly in the main bay areas, there remain important questions about temporal sampling scales, the role of light in regulating sediment-water exchanges, and the absence of assessment of nitrogen sinks such as denitrification and possibly nitrogen burial. The MEP report shows a high level of spatial variability, with the highest nitrogen efflux rates occurring in ponds and other areas with restricted circulation. It is likely the daily benthic flux terms are overestimated in the MEP report, although insufficient data are currently available to quantify this. Supplemental benthic flux data would help resolve this uncertainty, and possibly result in changes to the boundary conditions in the water quality model. The recommendations presented below would improve the understanding of the effects of management actions in the Pleasant Bay complex.

- Obtain and evaluate the original flux data generated by SMAST, rather than depend on the broad averages and data that include a modeled nitrogen deposition term.

- Obtain a hypsometric curve for the system, showing the cumulative percent of the estuary as a function of water depth. Based on this information, and light penetration data, a better estimate of benthic microalgae production could be developed. This is not a substitute for illuminated benthic flux testing, though.

- A limited benthic flux measurement program should be carried out to confirm or refute the assertion that benthic microalgal nutrient uptake could have a major impact on sediment nutrient fluxes. This would help resolve the potential bias associated with all cores having been incubated in the dark. A limited scope of work could be implemented at first to determine the extent to which photosynthesis on the bottom sediment is an important process in Pleasant Bay. If so, then a more substantial data collection program, including possible modification of the model boundary conditions, may be recommended.

- There are basic assumptions about the seasonality of benthic flux inherent to the MEP report, which should be tested with field data. The supplemental flux measurement program should also examine whether the MEP proposed seasonal pattern of sediment fluxes is valid. A subset of stations should be selected for seasonal analysis. Although the highest benthic flux is expected in summer, a limited seasonal analysis would add confidence to the analysis and help reduce/understand uncertainty.

- Supplemental *bona fide* benthic flux analysis by traditional methods would help determine if internal nitrogen sinks, such as denitrification and possibly sediment burial, are numerically important relative to both nutrient inputs and export.

- Some assessment of small-scale spatial variability in sediment fluxes in one or more of the ponds would also be useful as part of the supplemental sampling. These areas currently have the worst water quality, are likely to receive the biggest changes in sediment fluxes after management actions, and are one of the key reasons the Pleasant Bay system does not comply with water quality standards.

### Hydrodynamic and Water Quality Modeling Recommendations:

Based on the primary concerns identified herein, the following recommendations may be considered to assist in improving confidence in the overall hydrodynamic and water quality modeling.

- Analysis conducted as part of this peer review identified possible stratification of the drowned kettle ponds within the Orleans portion of Pleasant Bay based on limited data taken within Areys Pond. To verify potential stratification of the drowned kettle ponds, and their relative importance on the overall nitrogen concentrations, temperature and salinity data should be collected as a function of depth in two or more of the kettle ponds. This data collection would consist of simple Conductivity-Temperature-Depth (CTD) casts throughout the summer season(s). If the collected data revealed consistent stratification, as in the Horne and Horne (2001) data set, then the existing hydrodynamic and water quality model could be extended to three dimensions in these specific subembayments. There are a number of options that could be considered to evaluate the 3-D processes in the ponds, including:

1. The existing MEP model could be expanded to three dimensions for the entire Pleasant Bay system using RMA-10 (3-D hydrodynamics) and RMA-11 (3-D water quality)

2. The existing MEP model could be expanded to three dimensions in the terminal ponds only. The RMA series of models has the ability to combine both a 3-D grid portion and a 2-D grid portion

3. An independent 3-D model of a terminal pond could be developed and calibrated to the new data collected in the terminal pond. This model would assess the processes and dispersion of the nitrogen within a terminal pond. Such a model would also permit a more physics-based analysis of how the pond would respond to future decreases in nitrogen loading. There are limitations of RMA-4 since it does not incorporate feedback from changing environmental conditions, such as changes in bottom water oxygen concentrations in the more heavily impacted parts of the system.

- The background nitrogen concentration used in the Pleasant Bay modeling was based on a single year of data (2005). Additional data collected in the two years (2006 and 2007) following the MEP modeling effort indicated that the background nitrogen concentration observed in 2005 may have been larger than average. The water quality model (RMA-4) could be used to re-simulate the Pleasant Bay system using an average value of all the observed nitrogen values at the Atlantic Ocean sampling station or test for a reasonable range of expected background bioactive nitrogen concentrations of the Atlantic Ocean. This would require the model to be recalibrated.

- Conduct a RMA-4 water quality model sensitivity analysis. The dependence of modeled nitrogen concentrations on model input parameters (e.g. dispersion coefficients) and boundary conditions (e.g., benthic flux rates, atmospheric inputs, background nitrogen) should be quantified. Other MEP reports (e.g., for Bassing Harbor) show the model is highly sensitive to certain inputs. The relative uncertainty of the dispersion coefficients relates directly to the sensitivity of this dispersion parameter in the water quality model (Howes et al., (2001). Since the calibrated, existing conditions model must represent the observed nitrogen concentration data, or at least the range of averaged data (as shown in Figure VI-3 of the MEP Pleasant Bay report), it seems reasonable to assess the potential range of dispersion coefficients needed to match the range of the observed data. There is not likely one unique selection of model coefficients that could be used to calibrate the model. In this manner, upper and lower bounds on the nitrogen concentration results predicted by the water quality model could be provided and a reasonable assessment of the potential error associated with the nitrogen concentration results could be offered. This would help understand the inherent uncertainty, and guide the Town of Orleans in future design efforts. A site-specific sensitivity analysis performed on the dispersion coefficient and on an appropriate range of nitrogen input data would help improve model confidence and provide a possible range of nitrogen concentration results.

- Collect supplemental tidal measurements in upper embayments, such as Areys Pond, Paw Wah Pond, and Kescayo Gansett Pond. This information would help ensure the hydrodynamic model accurately represents flushing of these critical Ponds that affect the level of sewering required to meet the TMDLs.

- Although the current configuration of the Pleasant Bay inlets to the Atlantic Ocean improves tidal flushing and water quality, it would be overly optimistic to plan long-term wastewater facilities under the assumption that the enhanced flushing will remain (in the absence of coastal engineering initiatives to maintain the inlet (e.g., structures, dredging, sand bypassing, etc.). We understand the design process and design life for the wastewater infrastructure will exceed 50-years, which is a time scale within which the inlet has proven to evolve substantially in the past. The worst-case scenario in the MEP report (pre-1987 breach) may also be overly pessimistic. Instead the post-1987 breach conditions may represent a more reasonable design condition. The Town should select a suitable design condition that is consistent with its risk tolerance.

**Eel Grass and Benthic Community Recommendations:**
There remains substantial uncertainty regarding the extent of eel grass and benthic community decline over time; however, it is difficult to develop more meaningful conclusions in the absence of more rigorous, long-term data. Detailed surveys of eel grass could be conducted to document the extent and condition of eel grass beds. Individual plants could be surveyed for evidence of wasting disease and herbivory, for instance. Beds could be surveyed for areal coverage, relative density, and evidence of boat or mooring impacts. However, many years of detailed data would be required to form meaningful conclusions. Since there is evidence of decline, but Pleasant Bay has not yet deteriorated to the extent observed in some other more severely stressed systems, the Town should be aware of the potential for future degradation. In this regard, as part of the overall wastewater facilities planning process, the Town may wish to embark upon a rigorous eel grass monitoring program. Over years, the monitoring data could be used to further test the impacts of excess nutrient loading on the benthic community, particularly in vicinity of some of the ponds with the highest observed nitrogen concentrations (in the absence of or before restoration efforts are effective). Periodic benthic invertebrate community surveys could be conducted as well. In addition to documenting ongoing degradation, the eel grass and benthic community data also would expand the baseline of data available to document recovery in response to measures implemented in Town to reduce the nutrient load.

## General Recommendations:

- The Town should strongly pursue an avenue through which the SMAST data and models become part of the public record.

- There is a need to resolve the methodology, implications, and concerns related to the water quality model's inability to represent total nitrogen, and the subsequent MEP justification of using the model solely for bioactive nitrogen modeling. This approach may be reasonable, but is difficult to assess without access to the data. An attempt was made as part of this peer review to obtain comparable data for other systems to evaluate the unique nature of Pleasant Bay, but the data were not available. Interaction with SMAST on this matter would be most helpful.

Even with additional technical work to supplement the MEP report, inherent uncertainty will remain. The design criteria for wastewater management strategies to meet the TMDLs and estuary restoration goals are not as well-constrained as those for typical civil works projects. Time, budgetary, and technical constraints do not permit collection of sufficient data to resolve all uncertainties, numerical models have inherent simplifying assumptions, and there are relatively few case studies similar to Pleasant Bay to document the extent to which and how quickly the benthic community will recover. The Pleasant Bay case also is complex, because the extent of documented decline does not include severe system-wide losses of resources. Although eel grass remains, the potential for future declines is of concern.

Given the uncertainty with the findings in the MEP report and with how the Pleasant Bay system will recover, the peer review team strongly recommends certain steps to supplement the SMAST work to reduce uncertainty for Pleasant Bay. We also suggest it is not reasonable to expect to have a single upfront design basis for a full long-term solution that will produce the desired effects in Pleasant Bay. Instead, we encourage the Town to prepare for a phased wastewater facilities planning approach with incremental steps for complying with TMDL requirements, perhaps starting with the most stressed areas. A long-term commitment to ongoing water quality, benthic community, and eelgrass monitoring also should be made as the basis for compliance and demonstrating effectiveness. Depending upon how the estuarine system responds, future phases or plans for wastewater control could be modified. This is the essence of an adaptive management approach, which we recommend the Town pursue as part of its overall long-term compliance strategy.

For an adaptive management approach to be successful, however, the Town will require sufficient flexibility and patience, mainly with regard to timing. With the time required to advance the planning process, design and implement initial measures, allow for the nitrogen reductions in the groundwater to reach Pleasant Bay, and afford targeted areas an opportunity to recover, more than a decade may elapse before the benefits from initial aspects of a wastewater management plan can start to be quantified. Understanding these time scales should be an essential part of the planning process, and we encourage the Town to be cautious not to make final or large-scale infrastructure and financing decisions prematurely with inadequate technical information. An effective adaptive management plan may allow future wastewater management phases to be minimized depending upon how the system responds. These decisions must be made on time scales that are consistent with the estuary's expected and observed response.

# 1.0 BENTHIC FLUX MEASUREMENTS AND ANALYSIS OF MECHANISMS

## 1.1 INTRODUCTION

The charge for this analysis is described in the italicized paragraph:

*The consultant will a) evaluate the information in the Pleasant Bay Report that describes the methodology and protocol for measuring the benthic nitrogen flux and critically review the data table; b) The consultant will also evaluate the nitrogen criteria used in the Pleasant Bay Report to determine the health of the benthic community and compare it with other published information. In its analysis, SMAST conducted its measurements of oxygen concentrations at the deepest parts of the salt water ponds; c) In addition, the consultant should consider whether, in the absence of septic system nitrogen loading, oxygen would be depleted by other benthic activity.*

The approach to evaluating the information can be outlined:

<u>Evaluation of benthic flux information (task a)</u>

- <u>Sampling design.</u> Are cores taken from the middle of these embayments representative of processes throughout the whole ecosystem? Are there coarser sediments with lower rates of metabolism, or are shallow water sediments covered with sea grasses?
- <u>Experimental approach.</u> SMAST uses standard methodologies for the assessment of sediment-water nutrient exchange; chambers are stirred and temperatures matched to the local environment. Core replication appears to be excellent. One key issue is with regard to illumination; if even a small amount of light reaches the sediment-water interface, benthic microalgae can grow. In general, such benthic microalgae can: 1) inhibit denitrification by taking up ammonium otherwise used for nitrification; and 2) inhibit rates of ammonium and/or nitrate efflux. Dark core incubations are insufficient to estimate daily fluxes of nitrogen when benthic microalgal production is moderate or high. Data on light penetration will be examined to determine whether such microalgal production might be important.

<u>Evaluation of the nitrogen criteria used in the Pleasant Bay Report to determine the health of the benthic community and compare it with other published information (task b)</u>

- The approach to estimate changing sediment nitrogen recycling as a function of loading will be examined.
- The modeling approach using this data, particularly the extrapolation used to induce seasonality in the flux data, will be evaluated.

Oxygen depletion – scenarios under lower loading task (c)

- The multiplicative effects of lower loading on both nutrient recycling and upon the net sediment oxygen balance will be discussed.

## 1.2 OVERALL COMMENT

The key (and only) data set for this analysis is Table IV-11 in the Pleasant Bay MEP final report. That table presents net nitrogen return to the overlying water and does not break the sediment-water exchange into distinct species (i.e., ammonium, nitrate plus nitrite, dinitrogen gas). Moreover there is no accompanying data on fluxes of soluble reactive phosphorus and oxygen. The lack of oxygen flux data makes characterization of these sites difficult, and eliminates the use of elemental stoichiometry (i.e., $O_2$:N flux ratios) as a means of evaluating the nitrogen fluxes. The net flux number also includes a modeled nitrogen deposition term, making this table one more step removed from the experimental sediment-water exchange data. This modeled term is a source of uncertainty in these benthic flux numbers, and the true net thus becomes a non-empirical flux rate. The benthic flux data are a snapshot of summer conditions, with no measured seasonality. The basic benthic flux data set generation followed standard procedures used in all non-illuminated incubations, and given the qualifications of the investigators, is likely to be first rate.

The strengths of this data set are:

- A large number of sediment-water exchange sites. The spatial coverage was excellent and appeared to reasonably cover small embayments and both shallow and deep environments in the main bay. For this kind of evaluation, this is a strong spatial data set.
- The report very clearly explains cause/effect of nitrogen loading on the retention and remineralization/recycling pathways.
- Based on our reading of their published literature and other reports by these investigators, we have absolutely no concern with the quality of the experimentation and measurements.

The chief limitations of the data set and report are:

- Limited seasonality. The key period for the benthic flux of nitrogen to create water quality problems is summer, mainly because of warmer temperatures, remineralization of organic N deposited during cooler temperatures, higher sediment-water flux rates, and reduced efficiency of denitrification as a nitrogen pathway. However, we don’t really know if the hypothesized seasonal change in net N retention in these shallow water estuarine sediments is correct and such coverage does not appear to be available regionally except in wetlands (Hammersly and Howes 2003).

- Lack of consideration of the effects of benthic microalgae on benthic processes. It is well documented that photosynthesis on the surface of the sediments alters nitrogen fluxes (e.g., Sundback et al. 1991; 2000; Risgaard-Petersen 2003), it is mainly a matter of the degree of attenuation of such fluxes. Benthic microalgal photosynthesis both intercepts nitrogen produced within the sediment and under high rates of photosynthesis, can remove nitrogen from the water column.

- Minimal data presentation. From this report, we know remarkably little about the benthic biogeochemistry of Pleasant Bay; the data useful for comparison to other systems and for examination of internal consistency is not available. In particular, a complete data set on nitrate, ammonium, phosphate and oxygen fluxes would be helpful; denitrification would also be very useful, but harder to obtain since it is much harder to measure.

- The most basic analysis required for the understanding of the nitrogen cycle and its effects in estuaries is the development of a defensible ecosystem nitrogen mass balance (e.g., Boynton et al. 2008). To fully understand nitrogen cycling, one must have a basic quantification of nitrogen inputs (surface water, groundwater, atmosphere) and outputs (advection out of the system, denitrification, nitrogen burial). In addition, for the purpose of prediction of ecosystem effects, it is generally useful to understand internal nutrient cycling, such as fluxes of dissolved inorganic nitrogen from sediments and rate of water column nitrogen uptake. A bona fide mass balance is not possible using the Pleasant Bay data, and exists in a synthetic form via the use of 1) both modeled and measured nutrient inputs, 2) measured nutrient fluxes from sediments and 3) combined water column nutrient analysis and hydrodynamic modeling. Key loss terms such as denitrification and nitrogen burial are unassessed.

Key to the overall analysis and to the importance of nitrogen in the Pleasant Bay system is that while it is important to meet nitrogen concentration standards set by management agencies, the main concern regarding increased nitrogen is the effect on living resources. Concerns include: 1) the loss of habitat suitable for benthic and pelagic animals through substantial decreases in water column dissolved oxygen; and 2) diminishment of the health of seagrasses. While excess nutrient inputs are often a major part of both hypoxia and eelgrass losses, they are often exacerbated by physical factors such as vertical density stratification and regional scale seagrass diebacks.

### 1.3 SEDIMENT NITROGEN CYCLING: BIOGEOCHEMICAL BASICS

Nitrogen is essential for plant growth, and a key component of amino acids, DNA/RNA and proteins. In aquatic photosynthetic systems, nitrogen and phosphorus are generally the two key micronutrients of interest. In general, the ratio of nitrogen to phosphorus for algal growth has a central tendency around 16:1, the "Redfield" ratio. In freshwater, there is usually an excess of nitrogen relative to phosphorus and the addition of phosphorus produces more algal biomass. In estuarine and marine waters, there is generally an excess of phosphorus relative to nitrogen, and addition of more nitrogen stimulates the production of more algal biomass. In some estuaries there can be seasonal differences in which element limits primary production.

The dominant nitrogen species of interest are ammonium ($NH_4^+$), nitrate ($NO_3^-$), dissolved organic nitrogen (DON), particulate organic nitrogen (PON), and dinitrogen gas ($N_2$, the dominant gas in the atmosphere). The dissolved inorganic species are generally the most useful to algae, with DON important for some cyanobacteria. Inputs to estuaries from groundwater are often in the form of inorganic fixed nitrogen (DIN, generally $NO_3^-$ + $NH_4^+$). Watershed inputs can be in many different forms, but groundwater inputs from aerobic groundwater is generally nitrate. Nitrate is especially mobile because it adsorbs very little to soils, especially when compared to ammonium. Organic nitrogen (DON, PON) from the watershed can have a range of reactivities within the aquatic system, with a substantial part of such nitrogen having only a moderate liability. In the water column, uptake of nitrogen leads to algal growth; the senescence of such phytoplankton by the collapse of algal blooms or by zooplankton grazing results in the conversion of algal nitrogen back into inorganic nitrogen (remineralization). This can occur in both the water column and in sediment.

The sediment nitrogen cycle starts with the deposition of organic matter to the sediment-water interface (Figure 1). The organic matter can be very labile, such as from phytoplankton and benthic microalgae, of intermediate reactivity such as sea grass organic matter, or be relatively recalcitrant, such as some terrestrial organic matter. Fermentative processes release nitrogen as dissolved organic nitrogen, which is rapidly remineralized to ammonium in conjunction with oxidants such as dissolved oxygen, iron oxides, or sulfate. Under reducing conditions, the ammonium can diffuse out of the sediment or adsorb to particles. Under oxidizing conditions, ammonium can be nitrified, with a conversion to nitrate or nitrite. That nitrate can be released to the water column or utilized by denitrifying bacteria to convert fixed nitrogen to $N_2$ gas; denitrification results in nitrogen losses to the ecosystem.

In environments with minimal oxygen at the sediment-water interface, nitrification can be limited by the anaerobic conditions. In systems with high nitrate in the overlying water, diffusion of nitrate into the sediment can be an important supply of nitrogen for denitrification. Finally, in extremely reducing, often sulfidic sediments, nitrate can be reduced back to ammonium, a process called dissimilatory nitrate reduction to ammonium (DNRA).

Not shown in this diagram is the considerable effect that algae growing on the sediment surface can have on the net fluxes of nitrogen and phosphorus. Benthic microalgae take up remineralized ammonium and water column nitrate, effectively resulting in the minimization of dissolved inorganic nitrogen releases from the sediment. In addition, denitrification is often minimized because benthic microalgae effectively compete with nitrifiers for remineralized nitrogen (Risgaard-Petersen 2003). Benthic microalgae affect the overall nitrogen balance by being an important sink for nitrogen in the sediments. Rather than allowing a simple reflux of nitrogen back to the water column, they effectively intercept fixed nitrogen. The loss of benthic microalgae results in the loss of this sink, and possibly a non-linear enhancement of nitrogen inputs to the water column. Sea grasses also sequester considerable amounts of nitrogen, with storage in above and below-ground biomass. The turnover of sea grass organic matter generally proceeds slower than for algal organic matter, resulting from a lower nitrogen content that results in slower microbial utilization. Regardless of whether the sediments have algae or macrophytes, their exchange of nitrogen with the water column is very different than in non-vegetated sediments. One semantic issue is whether the nitrogen released from sediment is a **new** nitrogen load to the ecosystem. Such nitrogen is more effectively thought of as nitrogen from internal cycling; its source is organic nitrogen from autocthonous sources (algae/sea grasses) or allocthonous sources (e.g., terrestrial organic matter). Experience in other systems indicates that the remineralization of organic matter, and thus organic nitrogen, occurs rapidly after deposition. In temperate estuaries, most nitrogen remineralization comes from organic matter deposited within the last year. Within seagrass beds, where carbon:nitrogen ratios are higher, organic matter is longer lasting.

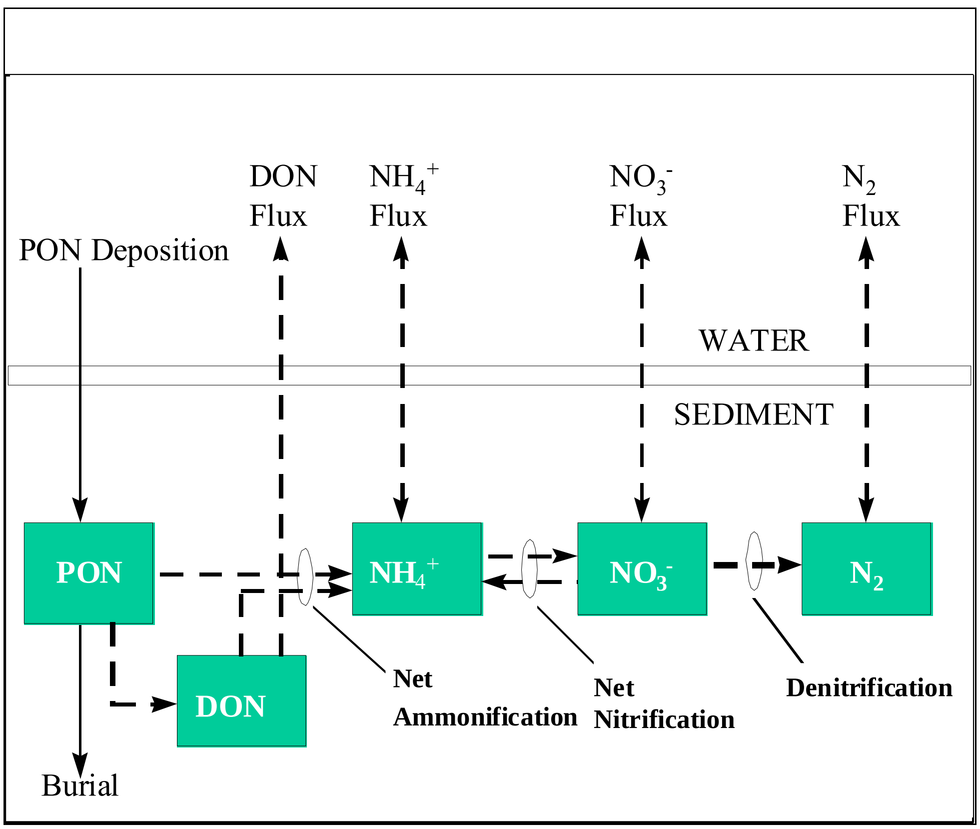


**Figure 1. Simplified sediment nitrogen cycle (Cornwell et al. 1999). The conversion of PON to DON and finally to NH4+ is variously called remineralization or ammification. Fluxes across the sediment-water interface are driven by concentration differences across that boundary; sediment $NH_4^+$ concentrations can be orders of magnitude higher than water column concentrations. The backwards arrow from nitrate to ammonium is referred to dissimilatory nitrate reduction to ammonium (DNRA) and often occurs in sulfidic sediments.**

Oxygen has a central role in the sediment nitrogen cycle. With minimal oxygen in the water column, nitrification does not occur and when water column nitrate is not excessively high, denitrification is limited. The result is a high flux of ammonium out of the sediment (e.g., Kemp et al. 2005). Under aerobic water column conditions, nitrification rates are high and denitrification is enhanced. Denitrification is a positive attribute in aquatic systems represent a loss of fixed nitrogen that could otherwise grow more algae.

## 1.4 EVALUATION OF TECHNICAL APPROACH

The SMAST sediment-water flux approach is similar to that used by many groups. Cores (15 cm inside diameter) were collected without disturbing the sediment-water interface and transported to a shore laboratory for incubation. Field temperatures were maintained and the cores were incubated in the dark. A time series of chemical measurements were then taken, with samples taken for 24 hours. The report presents no information on the depletion of oxygen during the incubations, either from the perspective of perturbation of existing redox gradients by changing oxygen concentrations or by presentation of sediment oxygen demand rates. This limits this evaluation of processes affecting the balance of nitrogen in these sediments. Based on other well described recent SMAST studies (e.g., Howes et al. 2008 a,b), all technical phases of the measurement program are likely more than adequate.

### *1.4.1 Sampling Design*

Sediment biogeochemical studies are relatively expensive and investigators are challenged to capture both temporal and spatial variability within a reasonable budget. The study design necessarily requires compromise; each investigator and modeling team is likely to have a different mix of time and space.

The SMAST study design had an emphasis on the spatial scale, with a large number of sites examined for sediment-water exchange of nitrogen. An impressive total of 62 sites were sampled for the benthic study. These sites were divided into 28 sub-embayments in Table IV-11. In all but three of the sub-embayments, at least two cores were incubated. Replicate cores (i.e., from the same coring location) were taken at 3 sites. Each basin's N fluxes appeared to be relatively similar at the different coring locations; the standard deviations for the sediment regeneration numbers was relatively low for this kind of study. The relatively tight grouping of rates suggests there has been a good characterization of each basin for the time sampled.

The lack of seasonal sampling of benthic fluxes is often compensated for by using relationships with temperature. Overall sediment respiration and nitrogen recycling are often a function of temperature, with more remineralization under warmer conditions. In cooler conditions, organic nitrogen sedimented to the sediment-water interface may degrade more slowly and persist until warmer conditions. SMAST present a hypothetical pattern of net nitrogen flux (Figure IV-20); similar patterns have been observed in other estuaries (e.g., Chesapeake Bay - Cowan and Boynton 1996; tidal marsh creeks – Hammersly and Howes 2003). With little regional guidance for the seasonal pattern of dark fluxes in shallow water estuaries, it is quite possible that this predicted seasonal pattern may not be a good representation of fluxes in fall, winter and spring.

In recent decades, benthic microalgal production has been shown to be important to benthic biogeochemistry, influencing both the balance of nutrients and decreasing the resuspension of inorganic particulates. In Figure V-9, the bathymetric contours suggest that the dominant water depths in the extended ecosystem are usually < 2 m. Site-average secchi depths from Pleasant Bay in 2007 ranged from 0.63±0.25 m at Pochet-Upper (WMO-3) to 2.53±0.81 at Namequoit Pt.-N (PBA-13). Average of all 17 monitoring sites was 1.62±0.6 m. Typically, secchi depths represent ~10-15% of incident light (Wetzel and Likens 1991), well within the range in which benthic microalgae can grow. Given these water depths and secchi depths, it would be surprising if benthic microalgae were not an important part of this ecosystem. The sampling design did not include illumination of the sediment water interface during incubation, so this component was not assessed.

The 1) modeled nitrogen inputs to sediment, 2) lack of seasonal data for fluxes, and 3) dark incubation of sediments that are likely photosynthetic are all potential limitations to this data set. It is difficult to evaluate the inputs of nitrogen to the sediment because, as in many systems, there is no straightforward way to determine these rates empirically. Simple concentration changes in solid phase nitrogen are too insensitive to develop loading estimates, and sediment traps in shallow systems mainly measure resuspended sediment. The lack of seasonal data is a concern, but the hypothetical seasonality almost certainly has the obvious summer maxima and winter minima predicted by SMAST. Certainly the off-season data are likely to be within a factor of two; if summer conditions are the main issue in the TMDL, then this may not be a key issue. From our perspective, the major issue is with regard to dark illuminations. In many cases, there is a major decrease in sediment ammonium or nitrate efflux from such sediments when they are illuminated during incubation (e.g., Risgaard-Petersen 2003; Kemp and Cornwell 2001; Howes et al. 2008a,b). In areas with benthic microalgae, one might expect a 50-100% decrease of fluxes in the light; this would translate into a 25-50% decrease in the daily efflux.

### *1.4.2 Evaluation of Flux Data*

The data from SMAST Table IV-11 are shown in Table 1. We have converted the mass units to μmol units in adjacent columns, using hourly rates instead of daily (mainly for our own convenience and to make stoichiometric conversions easier to understand). Our interpretation of the sediment N regeneration numbers is that this is not a benthic flux rate. Rather than being the benthic flux experimental data, it also includes a PON input number modeled from water column data. Thus, each flux consists of actual measurements, plus model output for nitrogen deposition. Net deposition is difficult to measure in shallow water sediments and modeling may be the most appropriate approach. There is no data to test the deposition model. The net long-term balance of N in sediments is:

*PON deposition to sediments = N return to the water column (sum of nitrate, ammonium and $N_2$ fluxes) + N burial*

**Table 1. Rates of sediment N regeneration in Pleasant Bay.**

| Location | Sub Unit | Sediment N Regeneration mg N $m^{-2}$ $d^{-1}$ Mean | S.D. | Sediment N Regeneration µmol $m^{-2}$ $h^{-1}$ Mean | S.D. |
|---|---|---|---|---|---|
| Meetinghouse Pond | Pond Basin | 79.5 | 12.7 | 221 | 35 |
| Lonnies Pond | Pond Basin | 22.7 | 3.9 | 63 | 11 |
| Areys Pond | Pond Basin | 107.3 | 13.9 | 298 | 39 |
| | Namequoit River | 107.3 | 2.1 | 298 | 6 |
| The River | Mtghouse Channel | 113.0 | 13.5 | 314 | 38 |
| | Upper River | 14.3 | 5.6 | 40 | 16 |
| | Mid River | 12.0 | 1.8 | 33 | 5 |
| | Lower River | 34.2 | 6.4 | 95 | 18 |
| | Mouth River | **-10.9** | **11.3** | **-30** | **31** |
| PawWah Pond | Pond Basin | 120.7 | 13.9 | 335 | 39 |
| Quanset Pond | Pond Basin | 98.0 | 5.9 | 272 | 16 |
| Round Cove | Cove Basin | 138.9 | 10.4 | 386 | 29 |
| Muddy Creek | Upper | 81.8 | 1.7 | 227 | 5 |
| | Lower | **-16.0** | **5.0** | **-44** | **14** |
| Bassing Harbor Sub System | Ryders Cove | 19.7 | 1.6 | 55 | 4 |
| | Frost Fish Creek Upper | **-5.1** | **0.0** | **-14** | **0** |
| | Crows Pond | 12.3 | 1.3 | 34 | 4 |
| | Bassing Harbor Basin | **-8.9** | **1.8** | **-25** | **5** |
| Pochet | Upper-Mid | **-1.2** | **1.5** | **-3** | **4** |
| | Lower Basin | **-1.7** | **2.5** | **-5** | **7** |
| Little Pleasant Bay | Upper | 16.0 | 1.1 | 44 | 3 |
| | Mid | 0.2 | 1.3 | 1 | 4 |
| | Broad Creek | 4.1 | 2.3 | 11 | 6 |
| | Lower | **-1.1** | **1.9** | **-3** | **5** |
| Pleasant Bay | Main Basin | 24.1 | 2.2 | 67 | 6 |
| | Little PB-ChatHbr | **-7.0** | **1.4** | **-19** | **4** |
| | Strong Isl-Bassing Hbr | **-18.1** | **1.1** | **-50** | **3** |
| Chatham Harbor | Basin | **-8.8** | **0.7** | **-24** | **2** |

There is no requirement that this equation balance on an hourly or daily basis. Indeed, net labile PON storage during cool months results in a buildup of N in the sediments that is remineralized by bacteria during warmer summer months. This storage effect would be especially evident in sea grass environments. This is represented by the net cool season deposition shown in SMAST Figure IV-20. However, this equation should balance on an annual or multi-year basis; when there is any burial of nitrogen, the result should always be a net influx of nitrogen. If unmeasured denitrification N fluxes are moderate to high, this will also hinder a full N balance calculation in the sediments. Characterization of high N effluxes (> 100 µmol m-2 h-1) as a new loading to the water column is inappropriate. The organic N responsible for the fluxes has already been counted as: 1) a flux of external organic N into the ecosystem from groundwater, the atmosphere or streams; or 2) as a net flux of organic N from the algae or macrophytes derived its nitrogen nutrition from groundwater or atmospheric nitrate or ammonium.

Comparisons to other flux data sets are not feasible since there is a component of modeled net deposition of N to the sediment surface embedded in the available data. It is interesting to consider how/why there are such dramatic differences between the small embayments and the main bay areas. The report effectively explains these differences as a result of hydrography/residence time, proximity to loading, differences in water column productivity, and redox-related shifts in N-cycling pathways. Additional differences may occur because of the presence/absence of benthic microalgae and sea grasses.

### *1.4.3 Benthic Flux Modeling*

The changes in benthic fluxes based on different loading scenarios (Table 2) were (page 144) calculated as:

(Projected N flux) = (Present N flux) * $[PON_{projected}]/[PON_{present}]$

The No Septic Loading Scenario (no anthropogenic loading) results in a decrease of watershed loading of 50% and a benthic flux decrease of 35%. The approach to determining changes in sediment-water N balance does not appear to reflect any changes in proportions of N burial/efflux/denitrification and driven mainly by PON sedimentation changes. Changes in water column oxygen associated with decreased N loading may result in the increased importance of coupled nitrification-denitrification. Any increases in light penetration associated with decreased N inputs may result in an increased biomass and productivity of benthic microalgae and sea grasses. On average, benthic microalgae modulate ammonium and nitrate fluxes from sediments and decrease the importance of denitrification. Sea grasses also often stimulate denitrification. The modeling in the MEP report may underestimate the improvement in sediment N efflux with decreasing external loading.

### *1.4.4 Effects on Bottom Water Oxygen*

It is reasonable to project increases in bottom water oxygen as overall rates of sediment metabolism decrease and rates of benthic photosynthesis increase. The current data set does not include rates of benthic respiration (as sediment oxygen demand) and/or benthic photosynthesis. Since oxygen concentrations are a function of: 1) biochemical demand in the water and sediment, 2) rate of exchange with the atmosphere, 3) water residence time; and 4) photosynthesis, making quantitative estimates of changes in bottom water oxygen is not possible without a modeling exercise. Other changes in the system as nutrient loading changes could include changes in the abundance of macroalgae and/or phytoplankton, also affecting net oxygen balance of the sediments.

**Table 2. Loading and benthic flux rates: whole system from report tables VI-2, VI-6, and VI.2.6.2.**

| | Present | Build Out | No Anthropogenic Loading |
|---|---|---|---|
| | kg $d^{-1}$ | | |
| Watershed Load | 127 | 165 | 20 |
| Direct Atmospheric Load | 86 | 86 | 86 |
| Total Loading | 213 | 251 | 106 |
| Benthic Flux | 185 | 213 | 120 |
| Benthic Flux:Loading Ratio | 0.87 | 0.85 | 1.14 |

### 1.5 COMPARATIVE DATA

Initially, we had hoped to directly compare benthic flux data sets from other Cape Cod or regional studies to the Pleasant Bay data. Given the absence of *bona fide* benthic flux data in the report, we are unable to make a meaningful comparison. Several studies would be of moderate relevance if the flux data was made available:

- Hopkinson et al. 1999. Benthic fluxes along the Parker River estuarine gradient
- Nowicki et al. 1999. Denitrification in the Nauset Marsh Estuary
- LaMontagne et al. 2002. Nutrient fluxes and denitrification in the Childs River.
- Hammersly and Howes 2003. Nutrient fluxes and denitrification in the Mashapaquit Marsh tidal creeks.

### 1.6 IMPLICATIONS

To summarize the analysis of the benthic flux data, the implications of this report from our perspective may be summarized:

- What are the chief limitations of the benthic flux work? The foremost and most difficult part of this analysis is the lack of access to information already generated by SMAST and paid for by the State of Massachusetts. Embedded in that data set are better answers to the questions being asked by the Town of Orleans. The second limitation is the absence of consideration of benthic microalgal effects on sediment nitrogen fluxes. Daytime fluxes are likely much lower than those in the dark in areas where light reaches the bottom (i.e., much of the bay). Finally, the estimation of non-summer flux rate was not made; this does not mean that the predicted rates in other seasons are unreasonable, but rather that they are likely poorly constrained. The model relationship with a mid-summer nitrogen flux peak is similar to observations in other systems, but adds uncertainty to the flux numbers.

- Are sediment N fluxes likely to be under or overestimated? In general, because of the attenuation of nitrogen fluxes by benthic microalgae, these rates are likely on the high side. In sub-embayments and tidal rivers with either higher turbidity or greater water depths, these data should be an excellent representation of summer conditions. On a whole system basis, it is possible that the overall rates could decrease 1/4 to 1/3, but it must be emphasized that we have insufficient access to data to make this a quantitatively defensible prediction.

- Is the N sedimentation model appropriate? The estimation of N inputs to the sediment is difficult in any aquatic environment, especially difficult in shallow water systems. These estimates were made on particulate nitrogen concentrations and residence time, and are likely the only way to make this estimate. If a seasonal nitrogen balance had been done (annual fluxes out of sediment plus sediment N burial = N inputs), an annual N sedimentation budget could be made. For the shorter-term estimates, a model is likely the only approach.

- Would one expect a simple relationship between decreased loading and benthic fluxes? In this model, the benthic flux to total N loading ratio increases about 20% with lower anthropogenic inputs. This suggests that relative to loading estimate, increased sediment nitrogen fluxes of ammonium and nitrate would occur, an unexpected result. One might expect that nitrogen fluxes would decrease because of increased bottom water oxygen in upstream parts of the system, leading to an increased proportion of remineralized nitrogen going to denitrification, a nitrogen loss. Furthermore, if decreased loading results in more habitat for benthic microalgae and sea grasses, the flux to loading ratio would decrease even more.

- What are the implications for oxygen concentrations from the benthic fluxes? That calculation requires running the flux information through the whole model. Clearly, sediment respiration can deplete overlying water oxygen; without oxygen flux data, the direct effect cannot be estimated. The whole ecosystem effect of producing less algae via decreased nitrogen loads will necessarily lead to improved bottom water oxygen concentrations.

- Is sediment "memory" an issue? From a biogeochemical perspective, most of the nitrogen remineralization in sediments comes from recent inputs of organic nitrogen (algae, sea grasses). Under summer conditions, this labile organic nitrogen is rapidly depleted. Although there is some residual over the long-term, it should not fuel substantial fluxes in future years. Changes in loading will thus rapidly be reflected in changed fluxes from the sediments; this is reflected in the Table 2 information from SMAST.

- Are there simple ways to resolve some of these benthic issues? Benthic flux studies are expensive and will always be limited in either or both temporal or spatial coverage. SMAST has done an exemplary job on the spatial aspect of sampling, this is an unusually large number of sites for this kind of work. Perhaps the key missing element is whether our hypothesized benthic microalgal influence is truly important. A few simple measurements could be used to see if this is the case. More temporal coverage would be very useful, but costly. A typical approach might be to select a small subset of the stations for seasonal or multi-year analysis.

## 1.7 RESPONSE TO SELECT COMMITTEE COMMENTS

The following two sub-sections offer responses from Dr. Jeff Cornwell to a selection of the validation committee's independent comments on the MEP reports (the chapter summaries that were prepared prior to initiation of the peer review project). Not all comments are addressed, since many are not within the contracted scope of work, there is insufficient information in the MEP report to address the matter, or the question was not considered germane to Task 1.

### *1.7.1 Chapter IV Comments*

***IV.1 WATERSHED LAND USE BASED NITROGEN LOADING ANALYSIS p 25 ¶ 1***

*Seems important to understand what mechanisms are involved, how this impacts the nitrogen mass balance and the 'seasonal' aspect. Mass of Nitrogen from decay of plant matter? kg/acre/year? Where does it go? When? How? What is the specific (step by step detail) analytical procedure used? Is there a standard analysis procedure in Standard Methods for Water and Wastewater Analysis (includes sediment analysis?) Is there a copy of Standard Methods available in town? (none in library although it is available in the reference section of CC Community College). Should we have a copy?*

***IV.3.3Rates of Summer Nitrogen Regeneration from Sediments p 79 ¶ 2***

*It seems that the relative magnitude of N loading to the sub-embayments is important. lf septic nitrogen loading is eliminated or reduced, does it make the sub-embayment healthy again? Or, is the nitrogen accumulation in plants and animals and the impact of their life cycles on the nitrogen mass balance the problem? This is not a prejudgment of the facts; it is simply a question at this time.*

- Response: Removal on nitrogen inputs will have a relatively rapid impact on the system. The organic matter in plants, animals and sediments does not persist for long periods of time. In the case of sediment, most of the nitrogen fluxing back to the water column is of recent origin (<< 1 year). If less nitrogen is put into the embayment, there will indeed be an attenuation of algal growth, sediment nitrogen deposition, and return flux of nitrogen to the water column.

***IV.1 WATERSHED LAND USE BASED NITROGEN LOADING ANALYSIS p 29 ¶ 1***
*Wording seems vague as to how much detailed data on land use was available or employed vs. broad categorization of land use and estimates. How detailed (e.g. lot by lot) was the land use information and what is meant by pre-determined nitrogen loading rates? Do we want to do a detailed analysis and nitrogen mass balance for a specific sub-embayment and compare to the model results?*

- Response: Not in task, I have the same questions. Clearly having access to the spatial/temporal input data would be valuable.

***IV.1 WATERSHED LAND USE BASED NITROGEN LOADING ANALYSIS p 29 ¶ 3***
*Why was data collected only during summer months? Does this properly account for nitrogen transport and mass balance on year round basis?*

- Response: Not in task. In non-summer months, it's a watershed model.

***IV.1.2 Nitrogen Loading Input Factors Wastewater/Water Use p 32 ¶ 3***
*Discusses "..MEP has derived a combined term the effective N Loading Coefficient (consumptive use times the nitrogen concentration) of 23.63 to convert water (per cubic meter) to nitrogen load (N grams)." Do we understand how this compensates for changes in occupancy rates? Where are the water use data and calculations that would help to understand this? How do we go about testing/verifying the occupancy/water use/ N loading assumptions? Check specific embayments/sub-embayments?*

- Response: Not in task.

***Orleans Future:***
*We are looking at a project with a ~30 year horizon. What does the future population structure of Orleans look like? Knowing the current nitrogen loading and mass balance is important. How do we think it will change over 20-30 years? Is the trend to more 'second' homes that are occupied for only 3-5 months per year? Or, will the second homes of today be occupied year around as the baby boomers retire? lf the babyboomers retire here, what does that mean with regard to occupancy? Year around? Summer, with winter residence elsewhere? What infrastructure is needed to support that population in terms of public and, importantly, retail shops and services? How does this impact the nitrogen loading of the embayments in 20 years?*

### *1.7.2 Chapter VIII Comments*

## ***Subject: Summary Chapter VIII***

***Reviewer Questions:***

***p. 200, ¶ 4:***

*Dissolved Oxygen. SMAST describes the high level of oxygen stress in the sub-embayments. "These small enclosed basins tend to have higher nitrogen levels and high rates of sediment metabolism associated with their circulation and focus of watershed nitrogen loads." Consequently, SMAST relates bio-activity stress due to low oxygen to elevated nitrogen levels. It is correct that septic nitrogen in the form of ammonia or urea* *consumes oxygen in their oxidation to nitrates, but is the oxygen stress totally related to increased septic nitrogen concentrations?*

- Response: The embayment processes do not distinguish the different sources of N. Oxidation of ammonium can occur in the groundwater, with oxygen consumption there; direct inputs of ammonium could lead to oxygen consumption, but it is likely not a major oxygen sink. *Oxygen stress in the embayments is mainly due to the oxidation of algae and terrestrial organic matter.* The algae clearly depend on bioavailable N (mostly nitrate and ammonium) from groundwater, surface water and atmospheric sources.

***p. 201, ¶ 1:***
*"Salt marsh creeks (that do not empty at low tide) frequently become hypoxic in summer as a result of high organic matter loading associated with marshes. Even pristine salt marshes can exhibit this behavior." Don't the sub-embayments, such as Meetinghouse and Areys Ponds collect organic matter? Is it possible that the hypoxia in these "A ponds" is caused by similar mechanisms to those in the marshes?*

- Response: I have little direct familiarity with these sites, but oxygen sags often occur in marshes. Inputs of marsh or terrestrial organic matter into the ponds clearly could lead to hypoxia in the ponds, depending on residence time and vertical mixing.

***p.202, ¶ 1:***
*"As for the oxygen and chlorophyll indicators and the distribution of sediment metabolism, the enclosed basins (Group A, above) are generally significantly to severely impaired relative to the benthic infaunal habitat quality." It appears that to accept this premise that the impairment is related to low oxygen and chlorophyll, one must accept the fact that septic nitrogen is the primary cause of deplete oxygen. Is it possible that the same mechanisms that occur in marshes occur in the Group A subembayments?*

- Response: Yes. Distinguishing algal sources and external (i.e., marsh) detrital sources of sediment oxygen uptake is difficult, but not impossible. However, the current data set would mainly have to use chlorophyll settling as a way to examine algal organic matter inputs; to do this would require some modeling.

***p. 204, ¶ 1 and 2:***
*"the restoration target should reflect both recent pre-degradation habitat quality and be reasonably achievable." "The threshold nitrogen level for an embayment represents the tidally averaged water column concentration of nitrogen that will support the habitat quality being sought."*

- Response: This is the general wording used in other estuarine restoration efforts. The loading is likely a purer reflection of how to control algal biomass, concentration is affected both by inputs, uptake, and physics.

***p. 204 , ¶ 5 :***
*"After the sentinel sub-system (or systems) is selected, the nitrogen level associated with high and stable habitat quality typically derived from a lower reach of the same system or an adjacent embayment is used as the nitrogen concentration target." Is this a reasonable approach?*

- Response: Not in task, beyond my expertise.

***p. 205, ¶ 1:***
*What is the support for the notion that dissolved organic nitrogen is nonreactive in the marine environment? What are the sources of dissolved organic nitrogen?*

- Response: Non-reactive is an unfortunate term. A considerable proportion of DON may be bioavailable on longer time frames. In the context of this study, it's a matter of how fast it degrades versus how quickly it is washed out of the system. DON comes from terrestrial inputs and the decomposition of organic matter produced in the estuary.

***p. 205, ¶ 2:***
*The nitrogen threshold of 0.16 mg bioactive nitrogen/liter was set based on a Dec. 2003 MEP Report for Bassing Harbor. What if it were 0.17? Or 0.18? How is the determination made? Note that the data in Chapter VII, Table VII-7, eelgrass areas declined from 246 to 114 acres between 1951 and 2000. Was the concentration of bioactive nitrogen less than 0.16 mg/liter during this 50 year period? Especially from 1951 to the early 1980s when the building boom occurred? Again, is bioactive nitrogen the only real culprit?*

- Response: Not in task.

***p. 205, ¶ 3:***
*"Ryder Cove represents a system capable of fully supporting eelgrass beds and stable high quality habitat based upon the 1951 – 2000 surveys. At present, this basin is transitioning from high to low habitat quality in response to increased nitrogen loading." So… if Bassing Harbor has had high quality water column in terms of bioactive nitrogen until recently, why did the eelgrass population decline between 1951 and 1995? Are there other potential causes of eelgrass decline that are not included in the SMAST assessment?*

- Response: Not in task, there clearly are other reasons for eelgrass loss at the regional scale, but others are more expert on this.

***p. 206, ¶ 1:***
*"Unfortunately, total nitrogen within this system appears to be very high. In fact, the whole of lower Pleasant Bay appears to contain very high levels of total nitrogen.*

*Analysis of the composition of the watercolumn nitrogen pool within these embayments revealed that the concentrations of dissolved inorganic nitrogen (DIN) and particulate* *organic nitrogen (PON) were the same as for the Stage Harbor System. In fact, the level of these combined pools (DIN+PON) was lower in Bassing Harbor (0.133 mg N L-1) than in the Stage Harbor (0.158 mg N L-1) and the mouth of Oyster River (0.160 mg N L-1). Note that the mouth of the Oyster River exhibits a documented stable healthy eelgrass habitat (MEP 2003). It appears that the reason for the higher total nitrogen levels in the Pleasant Bay waters results from the accumulation of dissolved organic nitrogen. The bulk of dissolved organic nitrogen (DON) is relatively non-supportive of phytoplankton production in shallow estuaries, although some fraction is actively cycling. It is likely that the high background DON results from the relatively long residence time of Pleasant Bay waters relative to the smaller systems. This allows the accumulation of the less biologically active nitrogen forms, hence the higher background.*

*Decomposition of phytoplankton, macroalgae and eelgrass release DON to estuarine waters as do salt marshes and surface freshwater inflows." (underlines added) The quotation indicates that the very high total nitrogen levels in Pleasant Bay are not expected or well understood. The text "explains" the phenomenon using the phrases "It is likely" and "It appears" throughout. It seems that the explanation is a pure conjecture without any facts to back it up. Since the crux of this matter is about how much nitrogen is in Pleasant Bay, how it moves in and out of the bay and how it impacts the flora and fauna in the bay, it would seem important to have and understand the facts about the nitrogen levels in the bay.*

- Response: I agree. Certainly residence time is part of this, but not the whole explanation.

***p. 206, last ¶:***

*"moving into the mouth of The River (PBA-13) and the lowermost basin of Pochet (WMO-03) eelgrass coverage appears to have declined since 1951, although eelgrass is still present. This loss of beds indicates that the habitat quality has become impaired, but since eelgrass remains, the impairment is judged to be "moderate."*

*(see p.182, para. 2: "….smaller eelgrass areas in Pochet and fringing shallow areas in The River and Meetinghouse Pond. …..However, it is clear from the 1951, 1995 and 2001 temporal sequence that the eelgrass areas in each basin, except Chatham Harbor, are declining in coverage. In The River and Pochet the eelgrass areas were always patchy and in the shallows. By the 2001 survey this pattern continues, but the beds appear to be declining, although they persist.")*

*Given the inferiority of the 1951 photos and the lack of any field verification, the thesis that eelgrass has been declining from 1951 to 2001 corresponding to an increasing rate of nitrogen introduction to the bay is lacking in credibility. Furthermore, the report does not present convincing evidence that 0.16 mg/L is a critical nitrogen level. Where is the body of scientific research showing the relationship between nitrogen concentration and eelgrass success?*

- Response: Not on task.

***p. 208, ¶ 2:***

*"While these systems [drowned kettle ponds] may not be supportive of eelgrass habitat, they are generally capable of supporting healthy benthic animal habitat. Infaunal animals are sensitive to the organic matter loading and resultant periodic oxygen depletions associated nitrogen overloading. Since these conditions typically occur at higher nitrogen loads than does the shading of the bottom by increased phytoplankton production (principal cause of eelgrass loss), the nitrogen threshold level for healthy benthic animal habitat is higher than for healthy eelgrass habitat." How important, in relative terms, are "organic matter" and the "nitrogen concentrations" in supporting a healthy benthic habitat? SMAST appears to consider the loss of eelgrass to be solely attributed to bioactive nitrogen in the water column, and ignores other mechanisms that contribute to eelgrass loss!*

- Response: Not on task.

***p. 208, last ¶:***

*After describing successful amphipod communities in the Orleans ponds where the bioactive nitrogen concentration varies apparently varies from 0.2 to 0.4 mg/l, the report concludes that 0.21mg/l should be established as the threshold concentration for benthic infauna. Why 0.21? Why not 0.28 or .030?*

- Response: Presumably the model suggests that higher levels promote more oxygen stress.

***p. 209, final 2 sentences:***

*"Therefore restoration success will be gauged by reaching the target at the sentinel station and at the secondary stations for eelgrass (Ryders Cove) and infauna. Overall there are three primary (PBA-12, PBA-03 and CM-13.) and 8 secondary target stations within this System, the largest embayment on Cape Cod."*

*This states that both the sentinel station and the secondary station must meet targets. The targets are shown in Table VIII-2 which contains both Bioactive Nitrogen thresholds and Total Nitrogen Thresholds for all 8 secondary stations, 6 of which are in Orleans. This seems inconsistent with the statement on p. 204 i.e.,*

*"……to first identify a sentinel station within the embayment …….is selected such the restoration of the one site will necessarily bring the other regions of the system to acceptable habitat quality levels."*

*These multiplicity of requirements and seemingly conflicting statements need to be resolved.*

- Response: Agreed. It is not clear that the SMAST document is detailed enough to be of help on this question.

### 1.8 CLARIFICATION/EXPLANATION OF ISSUES/QUESTIONS RAISED BY ORLEANS WASTEWATER MANAGEMENT VALIDATION & DESIGN COMMITTEE

Our purpose here is to specifically address the questions raised by the WMV&D Committee on the Draft Report, rather than embed these specific concerns in the nutrient flux text. In some cases, several questions are merged here and some concerns that are implicit in the committee questions are addressed here in an explicit manner.

- What are the controls and impacts of denitrification? What is the role of oxygen? What are the consequences of ignoring this process in both measurement and modeling? Denitrification often represents one of the major losses of nitrogen in coastal ecosystems (e.g.. Seitzinger 1988; Nixon et al. 1996; Boynton et al. 1995; 2008). In many estuarine sediments, the main source of the nitrate for denitrification is the process of nitrification, in which ammonium is oxidized to nitrate (e.g., Figure 1). This process requires the presence of oxygen in surficial sediments where nitrification takes place (e.g., Henriksen and Kemp 1988; Cornwell et al. 1999); the aerobic zone can often be on the mm depth scale. Consequently the close proximity of nitrifying and denitrifying bacteria results in the diffusion of the produced nitrate into reducing sediments where it can be denitrified. Under restricted oxygen, denitrification is generally minimized (i.e., Rysgaard et al. 1994; Kemp et al. 1990; 2005). If we presume the nitrogen inputs are correct, higher dispersion coefficients are required to remove the nitrogen from the bay complex, since the only loss term is advective losses out of the system. With a low water residence time, denitrification rates may be lower than advective losses of nitrogen, but we have no direct way to discount the potential importance of this internal loss term. Similarly, sediment nitrogen burial is unassessed.

- Can the relationship between sediment denitrification and oxygen concentrations be quantified? It is not at all clear that the relationships between water column oxygen and denitrification observed in some estuaries can be made relevant to all estuaries. DiToro's (2000) modeling formulation generates a surface aerobic (oxygenated) zone in the surface of sediment by combining estimates of sediment oxygen demand and overlying water oxygen concentrations with a diffusive oxygen model. Sediment oxygen demand data is not available for Pleasant Bay and thus a simple application of DiToro's nitrogen sub-model is not possible. The aerobic zone is key to the modeling of nitrification and thus denitrification. It is the efficiency of nitrification that is the linchpin of this modeling approach, and is in fact, the key to how important fluxes of $N_2$ are relative to fluxes of ammonium from sediments (Kemp et al. 2000; Owens 2009). In general, higher oxygen concentrations in the water column will promote higher rates of coupled nitrification denitrification.

- Within the ponds, aren't depth gradients going to generate large differences in sediment nitrogen regeneration? In the ponds, increased algal biomass, lower light penetration and (sometimes) greater depth will generally lead to a decreased importance of benthic microalgae. However, reactive particulate material will be "focused" into greater water depths, resulting in higher rates of sediment metabolism and nutrient efflux at greater water depths. Normally, the small surface area of the ponds would result in a diminished importance of small scale variability in pond benthic processes. However, given the importance of these ponds regarding non-attainment of dissolved oxygen standards, some shallow water benthic flux measurements are warranted depending on whether the model can handle the extra spatial data.

- How important are external loads of particulate organic matter (including particulate nitrogen), particularly in the ponds and other marginal areas? This question has been asked in several different ways. We have no independent way to assess these impacts. Clearly, the decomposition of "natural" organic matter (i.e., litter from forested and marsh areas) can result in both oxygen depletion and nutrient regeneration. Unfortunately, there are no simple ways to assess this in field-based measurements. The composition of sediments using C and N concentrations, ratios and stable isotopes could provide important clues as the origin of organic matter in the sediment, but the sediments are likely over-represented in poorer quality organic matter. Algal organic matter remineralizes much quicker and may be important to remineralization rates, if not bulk sediment organic composition. Clearly, such inputs will be most important in the ponds.

- Can the Redfield ratio be utilized to calculate the necessary reduction of phosphorus? Algae have characteristic elemental ratios, with the ratio of carbon to nitrogen to phosphorus being 106:16:1, meaning for each atom of phosphorus there are 16 atoms of nitrogen and 106 atoms of carbon. Such ratios are often utilized as an indication of whether nitrogen or phosphorus limits the productivity of algae (i.e. Fisher et al. 1999; Conley et al. 2009). However, given 1) the total absence of a phosphorus mass balance, 2) the tenuous nature of the nitrogen mass balance, and 3) the need to understand the ratio of bioavailable N and P, elemental ratios will unfortunately be of relatively little use.

- What is the relationship between nitrogen loading and benthic nitrogen fluxes in the model and how might it be better formulated? The nitrogen in the sediment results from:. 1) the inputs of both readily bioavailable dissolved nitrogen that results in algal growth; and 2) inputs of particulate nitrogen from the watershed. With higher nitrogen inputs to Pleasant Bay, there will be more nitrogen inputs to the sediment and more nitrogen remineralization in the sediments. The fate of the nitrogen depends on oxygen concentrations in the water column and sediment (see below).

- How responsive will the sediments be to changes in nitrogen inputs? Key to this question is how fast nitrogen inputs to the estuary change after changes in the delivery of nitrogen to groundwater (i.e., installation of sewage treatment). The lag in the concentration and flux of groundwater nitrogen is likely to result in a slow decrease in nitrogen inputs; the sediment response is likely to be relatively rapid in comparison to the groundwater response.

- Explain the need for benthic fluxes during winter months and with lighting. Of these two "needs", the more important of the two is the utilization of illuminated incubations. From the literature and our own work in shallow water sediments, it is clear that sediments receiving modest amounts of light to the sediment surface experience photosynthesis by benthic algae. The importance of this process is modified by benthic grazing by invertebrates, tide-related changes in illumination, and the importance of resuspension of surface sediments by wind and tide. Without actual measurements, our hypothesis regarding the likely importance of benthic microalgae is untested. If more advanced biogeochemical modeling is implemented in Pleasant Bay, these data will be critical. The seasonal pattern suggested by SMAST appears reasonable, but it is untested in this system. If Orleans implements these studies, we suggest work take place both in the open bay and in at least one pond. Regardless, the scale of these studies should be limited to a test of whether illumination and cooler weather measurements sufficiently change our view of the benthic biogeochemistry; a more comprehensive spatial/temporal measurement program is not warranted unless required by future modeling efforts.

- Are all the benthic habitats in the Pleasant Bay system similar? This question is addressed here because there is a tremendous diversity of habitats, ranging from small ponds to shallow muddy embayments to open estuarine areas (both shallow and deep). The "upstream" environments such as ponds and channels have a closer proximity to terrestrial organic matter and are generally more turbid; it might be expected that benthic microalgae are not important in these environments. In the main bay(s), the source of organic matter to the sediment is more likely to be algae grown within the basin and greater light penetration will result in an increased importance of benthic microalgae. Even in the main bays, there will be a gradient from sediment illuminated during daylight hours, to sediment that receives light mainly at low tide, to sediments that generally see no light. Hypoxia or low oxygen concentrations in the water column would generally be restricted to ponds and deepwater areas.

- Is the model responsive to changes in benthic biogeochemistry? No. In Figure 2, decreased external inputs of nitrogen lead to increased denitrification and lower sediment effluxes of fixed nitrogen. Thus, the MEP report's simple relationship of benthic flux to loading described in section 1.4.3 (above) does not include sediment denitrification, and is likely to overestimate ammonium fluxes from sediments that are currently impact by lower dissolved oxygen in the water column. Thus, external nitrogen removal may be especially effective in the improvement of pond and marginal sediment areas that currently have higher fixed nitrogen effluxes.

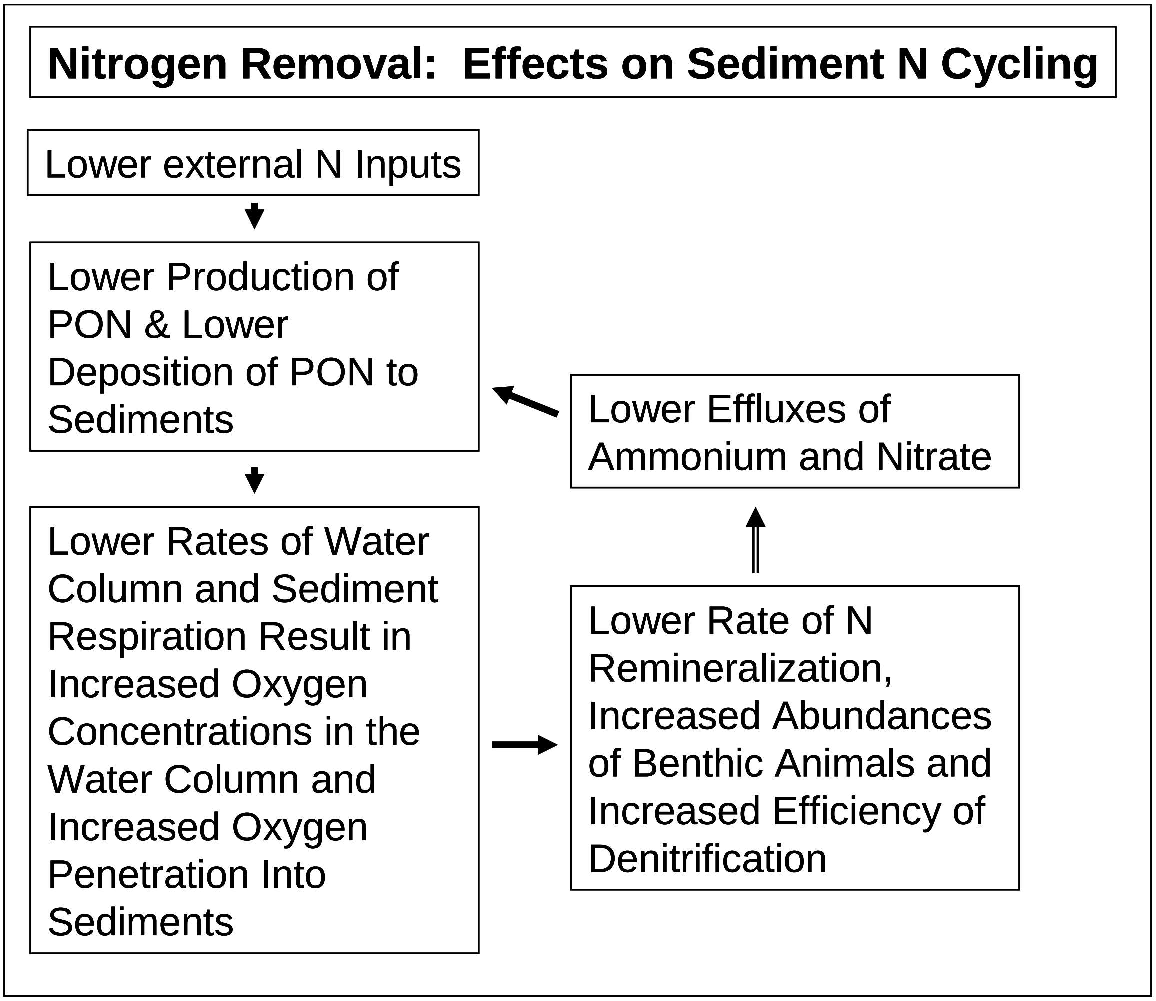


**Figure 2.** **Proposed trajectory of changes in the sediment nitrogen cycle with decreased nitrogen inputs. The feedback between oxygen and nitrogen biogeochemistry in areas with hypoxia will result in a non-linear relationship between external inputs and the efflux of bioavailable nitrogen from the sediments.**

One final key question is regarding the effect of benthic microalgae:

> Lack of benthic flux measurement in light [effect of microalgae] is a key finding. The impact on the "true benthic flux," also referred to as the solute flux, is masked by the addition of a term to represent a modeled PON input to the sediment. Thus,
>
> **Benthic Flux$_{SMAST}$ = Solute Flux (N output) – PON (N input)**
>
> *Considering all the components in terms of micromoles/ square meter/hour, if Benthic Flux$_{SMAST}$ is stated in the MEP report as 200 and assuming the PON component is 20% (i.e. 40) then the SMAST measured solute flux would have been 240. WHG has stated that the solute flux is overstated by ¼ to 1/3 (25 to 33%). This means that the actual solute flux, considering the effect of microalgae, should be perhaps 0.7 times the SMAST solute flux or 168. Returning to the equation above, this means that the SMAST benthic flux would be 168 – 40 = 128 or 36% less than used in the MEP modeling. We request that WHG confirm the above and provide, in the final report, an explanation with an equation showing how the difference between the true benthic flux and the SMAST benthic flux term relate to PON. Additionally, please provide an opinion as to what input should be used in the MEP model: Benthic Flux$_{SMAST}$ (including the PON component) or true Benthic Flux (solute flux). How and where should the PON settling component be input to the model?*

- The calculation of a hypothetical net efflux of dissolved nitrogen is correct based on the assumptions.
- A 25-33 percent decrease of efflux would lead to a solute flux of 240 * 0.7 or 168. With the burial of 40, the SMAST number would indeed be 128.
- PON deposition is a modeled term and related to residence time and the water column concentration of PON. It affects the net sediment nitrogen efflux term as used in the modeling.
- Benthic flux$_{(conventional)}$ = Benthic Flux$_{(SMAST)}$ – PON Deposition$_{(SMAST\ modeled)}$
- The formulation of the MEP model requires their term. We have no issue with the use of this formulation, but recognize that Benthic Flux (SMAST) is dependent on a "black box" model of PON deposition. As such, we have no reasonable way to evaluate this data or compare it to other systems.
- The PON settling term is necessary to "feed" the sediments with nitrogen. Because there are no sediment "sinks" such as N burial or denitrification (that is our interpretation of the SMAST model), what goes to the sediment must necessarily return as bioavailable nitrogen in the model. That bioavailable nitrogen can grow more algae or wash out of the system.

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# 2.0 HYDRODYNAMIC AND WATER QUALITY MODELING

## 2.1 INTRODUCTION

This chapter presents the technical peer review of the numerical hydrodynamic and water quality modeling performed as part of the MEP work for Pleasant Bay in Orleans, MA. Specifically, a review was conducted on the information presented in the Linked Watershed-Embayment Model to Determine Critical Nitrogen Loading Thresholds for the Pleasant Bay System, Orleans, Chatham, Brewster, and Harwich, Massachusetts report (Howes, et al., 2006). Various other reports were compiled and reviewed in the context of this report as well. Although the scope of work in the RFP specifically asks for a review of the hydrodynamic modeling, it was essential to review the water quality model as well. The RFP requested evaluation of three specific items within the hydrodynamic model topic area:

a. The consultant will consider all aspects of the model, including but not limited to whether the boundary conditions are sufficiently defined, whether the assumption of uniform vertical concentrations in each cell in the grid [is reasonable], and whether the dispersion coefficients are reasonable.
b. In addition, the consultant should consider whether the model should be modified to include a counterclockwise circulation pattern through a tidal cycle in Pleasant Bay and estimate whether such a modification would have a significant impact on the calculated results.
c. Also, the consultant should review SMAST's calculation and use of system residence time for flushing rates in the semi-enclosed northern sub-embayments.

This chapter is divided into four specific sections, including:

- 2.2 Hydrodynamic Modeling Comments – this section focuses on the hydrodynamic modeling performed in the MEP report (chapter V), and the data collected to complete the hydrodynamic modeling effort.
- 2.3 Water Quality Modeling Comments – this section focuses on the water quality modeling performed in the MEP report (chapter VI).
- 2.4 Impacts to Water Quality due to Inlet Migration and Breaches – this section evaluates the potential impacts to water quality based on inlet migration as presented in the MEP report (chapter IX).
- 2.5 Additional Overall Comments – this section provides additional overall comments related to the hydrodynamic and water quality modeling.
- 2.6 Recommendations – this section provides targeted recommendations intended to reduce potential areas of concern and assist in building confidence in the MEP model.

Within these subsections, the specific concerns raised within the RFP, as well as a more comprehensive technical review, is provided. Each section includes primary comments, secondary comments, less significant minor comments, and direct responses to commentary and questions raised by the Wastewater Management Validation and Design

Committee (WMVDC). The primary comments are those that have the highest probability of influencing the results. The secondary comments may also influence the nitrogen concentration results produced by the model, but to a lesser degree. It is difficult, and for some cases impossible, to quantify the level of change (percent influence) without access to the data and our model results used to develop the MEP Pleasant Bay model. In some cases, analytical calculations were conducted to supplement the results in the MEP report in an attempt to address the limited data available for this review. The review is focused solely on the numerical hydrodynamic and water quality modeling of the embayment, and not the watershed delineation or groundwater modeling presented in chapter III and IV of the MEP report.

The review of the numerical modeling was based solely on the information provided in the MEP Pleasant Bay report. The data collected and used in the MEP study, as well as the hydrodynamic and water quality model, were not available for the peer review. As such, the peer review is limited by the lack of this data, and the ability to quantify items is problematic. Access to the data and modeling may further clarify the MEP linked model approach and results, eliminate some potential concerns, and provide the ability to provide quantifiable bounds on the MEP results that would provide design guidance for the Town of Orleans.

The review is comprehensive, identifying potential limitations in the modeling approach, independent of the bias or level of significance. Although the peer review is thorough, it is also important to recognize which of the identified issues may potentially have a significant influence on the modeling results (nitrogen concentrations) within Pleasant Bay. Therefore, although limited by the lack of data availability, an attempt is made to identify the comments that may have the most significant impact on the modeling results.

## 2.2 HYDRODYNAMIC MODELING

Overall, the 2-D hydrodynamic model is well formulated, accurately calibrated, and performs well in determining water surface fluctuations and tidally driven depth-averaged circulation in the Pleasant Bay system. The investigators were thorough in their approach and the hydrodynamic model development was comprehensive. The 2-D simulations seem to have been conducted well and seem to capture the main features of the tidal fluctuations and the tidally driven depth-averaged circulation.

Primary comments related to the hydrodynamic modeling are provided below.

### *2.2.1 Primary Comments*

#### *2.2.1.1 Influence of Potential Stratification Effects*

It is likely that three-dimensional effects are important in some locations within the system, particularly in the smaller embayments where tides are weak, freshwater inflows occur, and computed nutrient concentrations are large. Three-dimensional effects that potentially may influence the circulation and subsequently the nitrogen dispersion include:

1) stratification that creates a constraint between bottom waters and the surface, which slows renewal of dissolved oxygen,

2) development of a two-layer estuarine circulation with a relatively fresh outflow near the surface and a denser inflow near the bottom, which increases the effective longitudinal diffusivity of the system, and

3) 3-D effects produced by solar heating during summer, particularly in those parts of the system that are relatively deep (~ 5 m) with relatively weak currents.

Ability to quantify these potential 3-D effects, or verification that they do not exist, is absent in both the model simulations (which are 2-D depth-averaged) and the measurements. Based on the data presented in the MEP report, neither temperature nor salinity has been measured as a function of depth, which is a potential shortcoming in the data set. If observations throughout the water column had shown stratification in the upper embayments, the use of a 3-D model may have been warranted. Additionally, if observations indicated a lack of stratification, then the selection of a 2-D depth-average numerical model, as used, is likely adequate. Due to the importance of the MEP work to the Town, it may be reasonable to argue that 3-D measurements and 3-D modeling are more representative of state-of-the-art, especially if data observations revealed potential stratification within the system.

Some depth-resolved observations of salinity, temperature, and dissolved oxygen were collected by Horne and Horne (2001) in Areys Pond, a subembayment in the upper portions of the Pleasant Bay system. These historical data, observed prior to the MEP work, contain measurements of temperature, salinity, and concentration of dissolved oxygen at several depths at the deepest part of Areys Pond. In order to determine if stratification effects may be important in the upper estuaries in the Pleasant Bay system, the Horne and Horne (2001) data were used to evaluate potential density stratification in Areys Pond.

The Horne and Horne (2001) data indicate depth-averaged temperature increases from spring to summer (Figure 3), presumably as a result of solar heating in both Areys Pond and the adjacent bodies of water. The salinity also increases slightly from spring to summer (Figure 3), possibly due to the reduced freshwater discharge from streams and groundwater as the summer progresses.

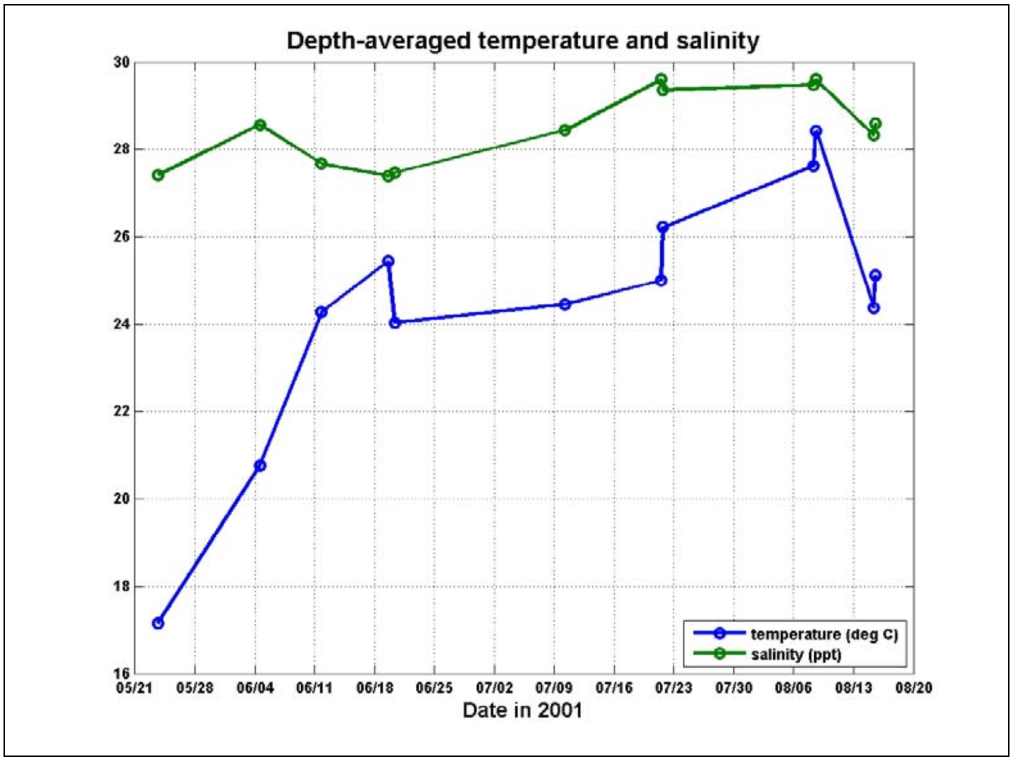


**Figure 3. Depth-averaged temperature and salinity at the deepest part of Areys Pond in 2001. Observations from Horne and Horne (2001).**

To assess potential vertical variations in the observed water column, vertical gradients of temperature and salinity (i.e., rate of change in temperature and salinity with depth) were computed using the depth varying data of Horne and Horne (2001). Figure 4 shows the vertical gradients for all measurement times for both salinity (green line) and temperature (blue line). In addition to the gradients, the 95% confidence levels are also presented as the bars extending vertically. Most of the gradient values are statistically significant since even the 95% confidence intervals are greater to or less than zero and are nearly always stabilizing. The gradients indicate that the temperature in Areys Pond decreases with depth, while the salinity increases with depth, such that relatively warm, fresh water overlies colder, saltier water in Areys Pond.

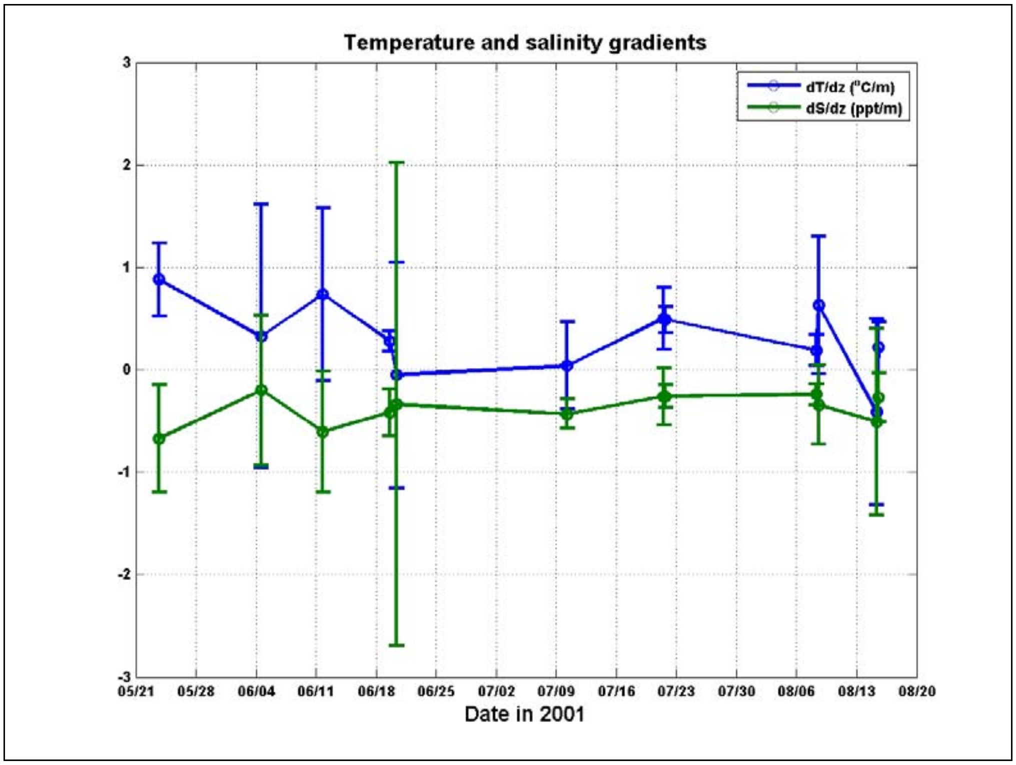


**Figure 4. Vertical gradients of temperature and salinity at the deepest part of Areys Pond. Error bars represent 95% confidence limits.**

To determine if Areys Pond stratification is persistent and stable, the Brunt-Vaisala frequency, or buoyancy frequency was calculated. The squared buoyancy frequency, $N^2$, calculated as a function of pressure, temperature, and salinity, is presented in Figure 5. The squared buoyancy frequency in Areys Pond is always positive (indicating a statically stable density distribution) and with only two exceptions does the 95% confidence interval drop below zero. The buoyancy frequency is consistently positive and indicates a stable stratification since the temperature and salinity gradients reinforce (i.e., they both have a stabilizing effect) each other. Although salinity is more important than temperature in producing the vertical density gradient, temperature is also a factor. The mean squared buoyancy frequency for the entire record is 3.6 $x10^{-3}$ $s^{-2}$, which is a value typical of many stratified estuarine environments (Turner, 1981).

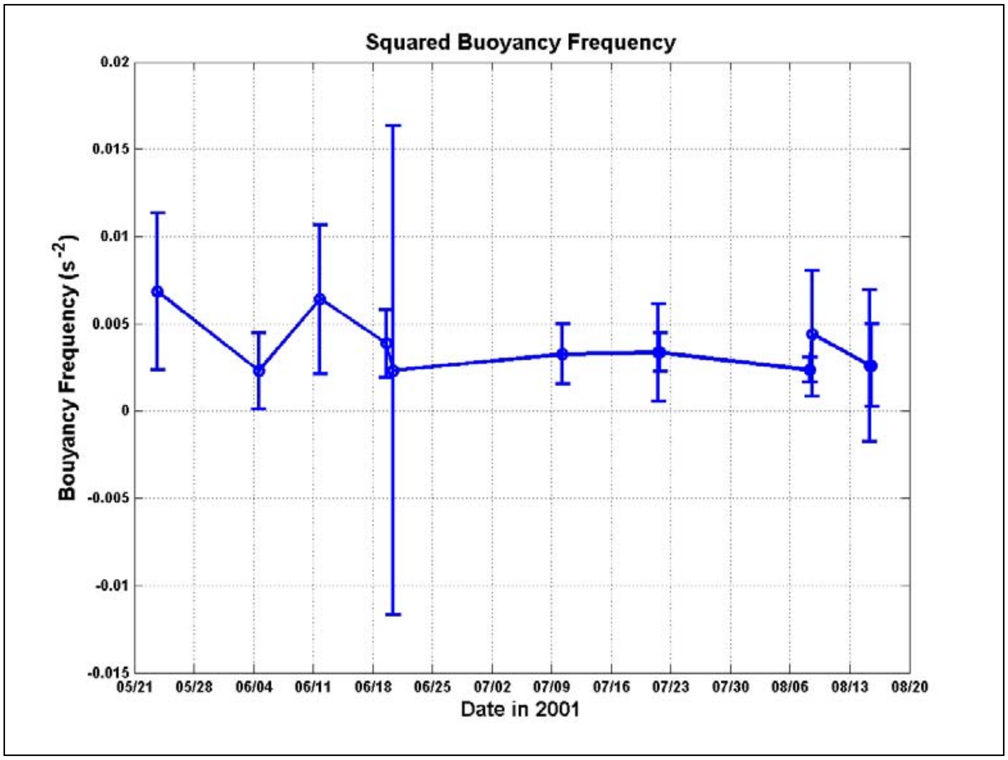


**Figure 5. Depth-averaged vertical squared buoyancy frequency at the deepest part of Areys Pond. Error bars represent 95% confidence limits.**

In order to determine the potential ability of the velocity gradient (*du/dz*) to overcome the ability of Areys Pond to remain stratified (or induce mixing), a stabilizing vertical density gradient can be estimated according to the Miles-Howard criterion by $Ri = N^2/(du/dz)^2$, where *Ri* is the gradient Richardson number. When *Ri* is less than ¼, then velocity shear is considered sufficient to induce mixing and overcome stratification. Numerous laboratory and field measurements have indicated that this theoretical result describes the mixing threshold in natural systems with reasonable accuracy (Turner, 1973). Assuming for simplicity that the velocity varies linearly with height throughout the water column, and that the velocity is zero at the bottom, this result corresponds to a depth-averaged velocity of *hN*. With the observed mean squared buoyancy frequency of $3.6 \times 10^{-3}\,s^{-2}$, and a water depth (*h*) of 4 m representative of the deepest part of Areys Pond where the measurements were obtained, the depth-averaged velocity required to produce mixing would need to be approximately 0.24 m/s. In reality, the depth-averaged velocity required to produce mixing may possibly be larger since the assumption of zero velocity at the bottom is an underestimate.

To determine if under normal conditions Areys Pond would be able to overcome its stratification, it is useful to estimate the velocity in the channel entering Areys Pond by means of a control volume continuity analysis. Without the hydrodynamic model or the modeling results available, a simple tidal prism approach was used to estimate the potential velocity in the Areys Pond system. The MEP report estimates that the tidal prism ($\Delta V$) for Areys Pond is approximately 2,623,000 $ft^3$. Assuming that the volume varies approximately sinusoidally with a radian frequency corresponding to the M2 tidal constituent ($\omega$ = 1.4 x$10^{-4}$ $s^{-1}$), the maximum flow rate in the entrance channel must be $Q=1/2\ \omega\ \Delta V$. Therefore, the corresponding depth-averaged velocity is 0.12 m/s, estimating the channel breadth and depth form Figure 3 of the Horne and Horne report as 90 and 5 feet, respectively. Thus, even in the inlet to Areys pond, where the fluid velocity must be much larger than the velocity in the pond itself, the flow is not strong enough to mix the water column against the stabilizing effect of the observed stratification. It is likely that not only Areys Pond, but also the adjacent bodies of water may also be stratified, and that this stratification persists because of the relatively weak velocities. Additionally, the presence of persistent stable stratification likely explains the fact that the observed concentration of dissolved oxygen is smaller near the bottom than near the surface (Figure 6), since the near-bottom water is likely isolated from exchange with the atmosphere.

Therefore, based on the limited data available for this review, it appears that stratification may be a significant process in the upper embayments of Pleasant Bay and that by not including potentially important 3-D effects in the hydrodynamic and water quality models could lead to inaccurate estimates of circulation and nitrogen dispersion in the upper sub-embayments. Although 3-D processes likely have minimal effect in a majority of Pleasant Bay, the numerical modeling results in the upper sub-embayments may be significantly influenced by 3-D processes.

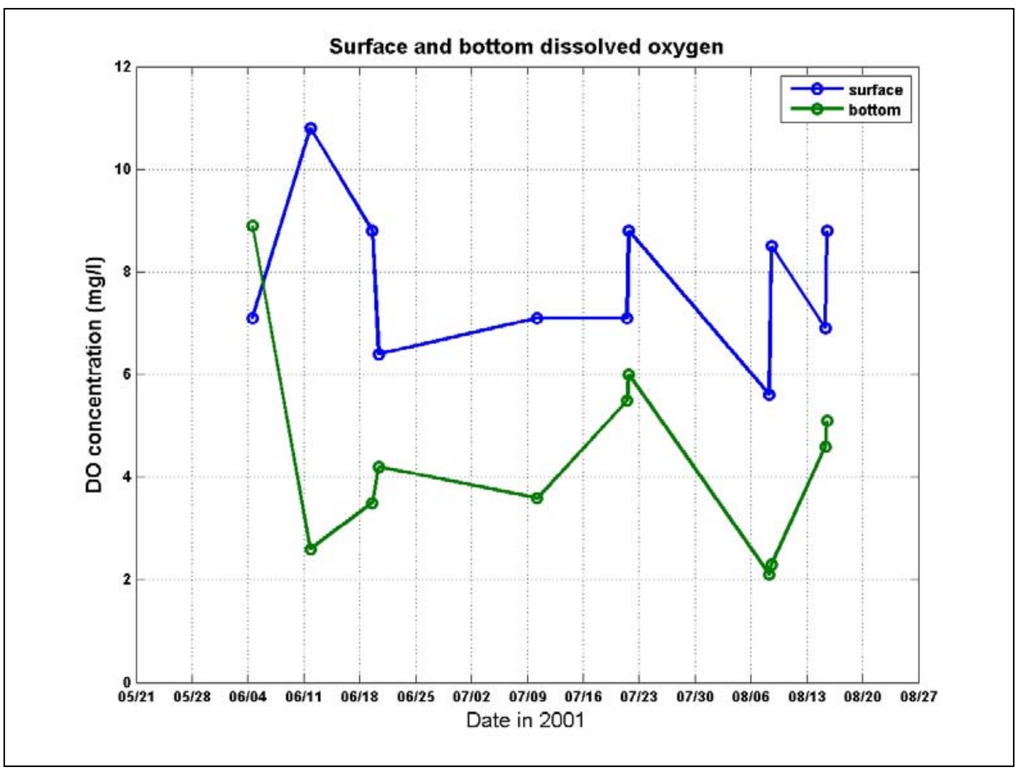


**Figure 6. Surface and bottom concentrations of dissolved oxygen at the deepest part of Areys Pond.**

### *2.2.2 Secondary Comments*

This section provides questions and/or comments that likely have a less significant impact on the model results as compared to the primary comments.

#### ***2.2.2.1 Tidal Attenuation in Upper Subembayments***

Many of the smaller sub-embayments in the Chatham portion of Pleasant Bay (e.g., Muddy Creek, etc.) have been studied in a separate MEP report (Howes, et al., 2003). Howes et al. (2003) study of these Pleasant Bay subembayments included more detailed bathymetric information and additional tide and current data to more accurately determine the tidal attenuation caused within these subembayments. This more local, site-specific data results in model calibration of the each subembayment to the observed attenuation occurring within each of these smaller subembayments. It is assumed the detailed sub-models calibrated and verified in this previous study are integrated into the larger Pleasant Bay study (Howes et al., 2006). Given the relative importance of all the smaller sub-embayments, where a majority of the water quality does not meet the required thresholds, it seems logical to ensure accurate tidal attenuation model calibration in the other smaller sub-embayments in the upper portions of the Pleasant Bay system (e.g., Areys Pond, Kescayo Gansett Pond, etc.). However, no tidal observations were conducted in these upper sub-embayments during the overall tide data collection effort. Tide data collection within these upper sub-embayments would have verified that tidal attenuation was adequately identified in the hydrodynamic model. Although this verification would improve model confidence, the lack of verification likely has a less significant impact on the overall modeling results than other factors.

#### ***2.2.2.2 Simulation of Anthropogenic Structures***

Historically, the RMA-2 hydrodynamic model was not capable of handling flow control structures (e.g., culverts, weirs, etc.), as the model was originally developed as a surface water, open channel hydrodynamic model. In more recent versions however, RMA-2 has been upgraded to include flow control structures. These upgrades have included the addition of 1-D control structures, which is available in version 4.20 and higher, and subsequently 2-D control structures, available in version 4.40 and higher (Donnell, 2006). The MEP Pleasant Bay report does not provide the version of the RMA-2 code that was used in the development of the Pleasant Bay hydrodynamic model, although it is assumed that RMA-2 version 4.5 was likely used.

Section V.2.2 of the MEP Pleasant Bay report discusses anthropogenic changes within the Pleasant Bay system consisting of structures built at the Route 28 crossings of Muddy Creek and Frost Fish Creek. The Muddy Creek Route 28 culverts consist of dual box culverts (2.6 feet by 3.7 feet), while Frost Fish Creek consists of 3 partially blocked 1.5 feet diameter culverts, as well as a single large culvert and weir structure upstream of the partially blocked culverts. The MEP report on Pleasant Bay does not contain any documentation on how these control structures were modeled within RMA-2, or if they were explicitly included at all. Additionally, (Howes, et al., 2003), which contains more detailed modeling of the Muddy and Frost Fish Creek , does not provide any additional discussion on the methodology for modeling the flow control structures. Howes et al.

(2003) does indicate that no 1-D elements were used in the modeling grid except for freshwater input areas. Therefore, flow control structures were not explicitly modeled using a 1-D control structure for Pleasant Bay. Since the methodology to simulate flow control structures in RMA-2 is an important aspect of the modeling of some of the Pleasant Bay sub-embayments, and since this is a relatively new feature in RMA-2, an explanation of the modeling approach for these flow control structures would be helpful.

In addition, the frictional coefficients used to represent the culverts are significantly higher than those published in literature (Chow, 1959; Barfuss and Tullis, 1994; Barfuss and Tullis, 1988; Bishop and Jeppson, 1975; Neale and Price, 1964; Tullis et al., 1990; Norman et al., 2001). Table 3 presents the typical range of frictional coefficients used for culverts, as presented in the Federal Highway Administrations Hydraulic Design of Highway Culverts (Norman et al., 2001). Partially blocked culverts may account for an increase in frictional coefficient; however, the 0.50 value is an order of magnitude higher than typical values.

If control structures were not utilized in the hydrodynamic grid, tidal attenuation may still have been adequately captured by the using standard 2-D elements with modified frictional coefficients. However, this approach may not adequately represent the actual dynamics of the flow control structure (especially if the culverts are fully flowing during portions of the tidal cycle), nor can it be modified to represent potential alternative structures with any level of design accuracy.

#### *2.2.2.3 Tidal Residual*

Based on Figure V-13 in the MEP Pleasant Bay report, there is significant tidal residual that exists after extracting 23 individual tidal constituents. Although only 23 tidal constituents are sometimes used by NOAA to compile their tide tables, the length of the 43 day tidal record should allow for the decomposition of more than 23 tidal constituents. For example, there are 35 standard tidal constituents that can be captured in a 40 day record. Considering the variation of the tidal residual is over 2 feet at Fish Pier (approximately 47% of the observed tidal range at the Fish Pier), it is feasible that some of this residual may actual be composed of tidal constituents that were not evaluated in the harmonic analysis. Although the tidal residual only accounts for approximately 10% of the overall energy, this could be better rectified through the inclusion of more tidal constituents, which may, in turn, enhance circulation slightly.

We understand that it is likely that a portion of the tidal residual is caused by atmospheric forcing (wind, rainfall, barometric pressure). Therefore, Figure 7 presents the climactic data observed by NOAA's National Climactic Data Center (http://www.ncdc.noaa.gov/oa/ncdc.html) at the Chatham Municipal Airport during the time period of the MEP tidal observations. The upper panel presents barometric pressure, the middle panel shows wind magnitude and direction, and the lower panel shows precipitation. These climactic data can be qualitatively used to assess the potential causes of the tidal residual. For example, the significant increase in water surface elevation on November 5 corresponds to a significant drop in barometric pressure on that day, while the increase in water surface elevation on November 29 may be caused by a combination of the significant rainfall and increased wind. However, the apparent low pressure system occurring on October 24, indicated as the cause of the largest tide residual (p. 105 of MEP report), does not seem to be the cause of this particular increase in water surface elevation. Therefore, there may be other factors that contribute to some of the tidal residual.

It is unclear if measured or predicted tides were used for the offshore tidal boundary conditions and for model calibration. However, if predicted tides, decomposed through harmonic analysis of the measured water surface elevation, are used to simulate the model, then the model could be improved through additional tidal constituents.

**Table 3. Manning's n Values for Culverts (Norman et al., 2001).**

| Type of Culvert | Roughness or Corrugation | Manning's n |
|---|---|---|
| Concrete Pipe | Smooth | 0.010-0.011 |
| Concrete Boxes | Smooth | 0.012-0.015 |
| Spiral Rib Metal Pipe | Smooth | 0.012-0.013 |
| Corrugated Metal Pipe, | 68 by 13 mm<br>2-2/3 by 1/2 in Annular | 0.022-0.027 |
| Pipe-Arch and Box | 68 by 13 mm<br>2-2/3 by 1/2 in Helical | 0.011-0.023 |
| | 150 by 25 mm<br>6 by 1 in Helical | 0.022-0.025 |
| | 125 by 25 mm<br>5 by 1 in | 0.025-0.026 |
| | 75 by 25 mm<br>3 by 1 in | 0.027-0.028 |
| | | 0.033-0.035 |
| | 150 by 50 mm<br>6 by 2 in<br>Structural Plate | 0.033-0.037 |
| | 230 by 64 mm<br>9 by 2-1/2 in<br>Structural Plate | |
| Corrugated Polyethylene | Smooth | 0.009-0.015 |
| Corrugated Polyethylene | Corrugated | 0.018-0.025 |
| Polyvinyl chloride (PVC) | Smooth | 0.009-0.011 |

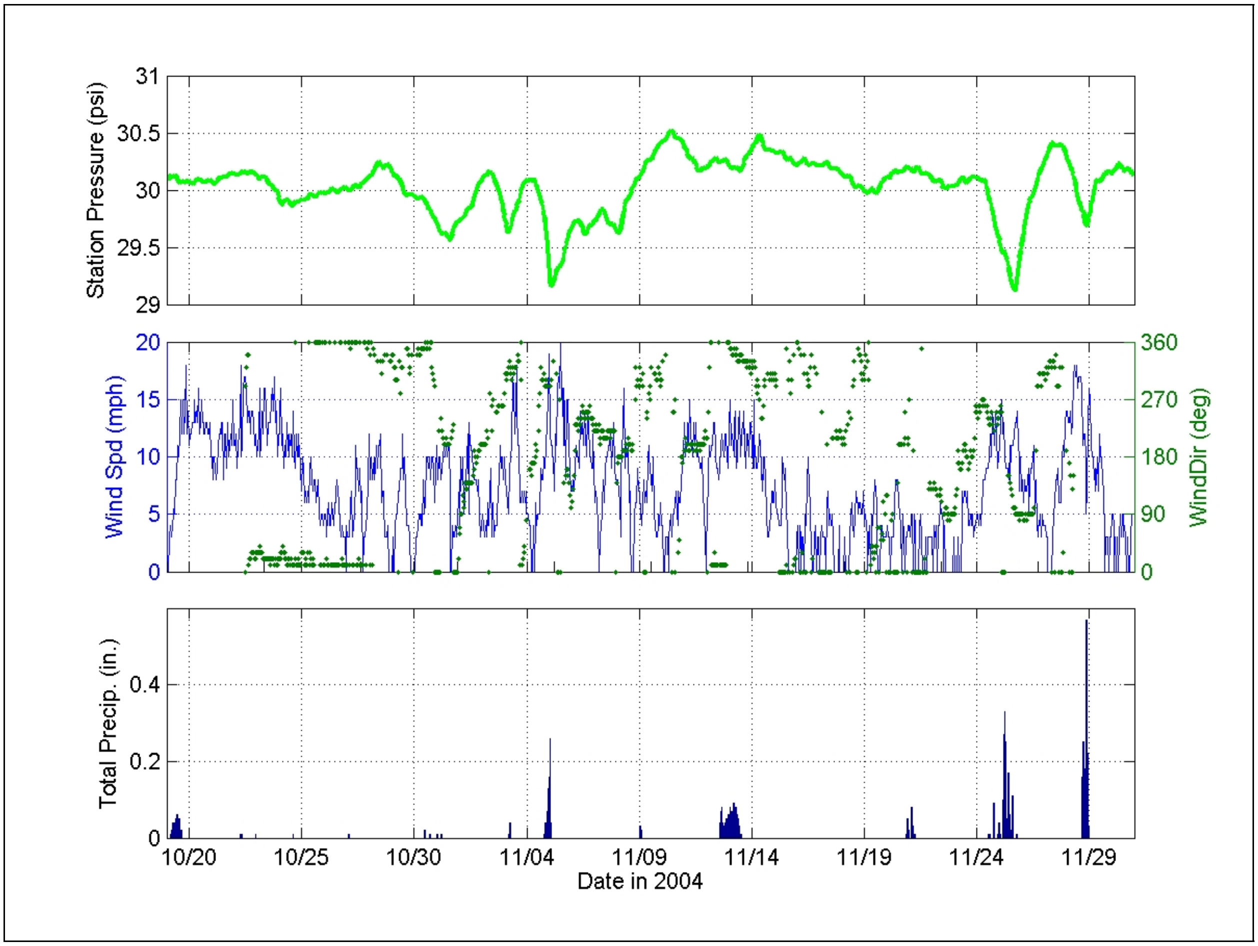


**Figure 7. Barometric pressure, wind speed and direction, and precipitation observations during the 43 day tidal deployment period presented in the MEP Pleasant Bay report. Data collected by NOAA's National Climactic Data Center at Chatham Municipal Airport.**

### *2.2.3 Minor Comments*

#### ***2.2.3.1 Data Collection and Analysis***

- The track lines and survey dates associated with the NOAA GEODAS data are not shown. It may be useful to show the added coverage that was provided by these data, as well as the relative time scales when they were collected. Compared to many other MEP modeled embayments, the bathymetric data coverage for Pleasant Bay appears to be less than other MEP modeling embayments, especially in the middle and upper eastern portions of the system. If the data are sparse, local shallow or deeper areas (subject to possibly increased stratification) may not be well-represented in the model.

- Tidal datums were computed from the 43-day tidal record at stations in the Pleasant Bay system. As indicated in the MEP report, the National Oceanographic and Atmospheric Administration (NOAA) maintains a tidal datum located at Lydia Cove in Chatham, MA, which is based on 19 years of tide data. The MEP Fish Pier (PLB2) tidal station is located close to NOAA's Lydia Cove station. Since the MEP data are limited to a 43-day record, differences are expected between the tidal datums calculated by NOAA's long-term record and those calculated from the short-term MEP data. For example, from the NOAA tidal station at Lydia Cove (http://tidesandcurrents.noaa.gov/), the tidal range (MHW to MLW) is 4.63 feet, while at the adjacent Fish Pier location in the MEP report, Table V-1, the tidal range is 4.3 feet. The difference is reasonable; however, it is not clear whether the time period selected for model calibration, and subsequently for nitrogen dispersion modeling, is representative of longer-term tide data. If the tidal range used in the modeling is less than the long-term average it is possible there is an under prediction of mixing potential, which may introduce a conservative bias to the analysis.

- Since the circulation dynamics of the system are a critical component of the hydrodynamic and water quality modeling, it would be helpful to present all the transect data (similar to Figures V-14 through V-17) in an Appendix. In this manner, the reader could be educated on the observed flow dynamics throughout the tidal cycle (e.g., slack tides, transition periods from flood to ebb, etc.) As it stands, there is minimal representation of the physical circulation processes in the system. Comparisons of data and model predictions (as stated below) would lend tremendous insight to the processes, and help the community better understand the issues.

#### *2.2.3.2 Hydrodynamic Modeling*

- The MEP report indicates that, "For example, the spread of pollutants may be analyzed from tidal current information developed by the numerical models." Perhaps it would be worthwhile to show circulation dynamics throughout the tidal cycle at other locations throughout the system, similar to the flood and ebb velocities shown at the inlet (e.g., Figures V-28 and V-29). This would help understand the overall circulation dynamics of the system, which is generally lacking in the report. Model animations of circulation, for instance, would benefit the community.

- There is no mention or discussion of potential sea level rise and its influence on the modeling or dispersion in the MEP report. Given the current rates of predicted sea level rise, the influence of future sea level rise considered in the hydrodynamic or water quality models should at least be addressed.

- It is unclear whether a 5 or 7 day period was selected for model calibration. On p.113, the second paragraph refers to a 5 day calibration period, while the third paragraph refers to a 7 day simulation. There are similar inconsistencies related to the length of the calibration period throughout the report. In either case, it should be clarified why only 5-7 days were selected. It also would be helpful to document why the 5-7 day period selected is believed to provide a reasonable representation of the average tide range when compared to the long term tidal records.

- It is unclear if surface wind forcing was included in the hydrodynamic model. In model setup, no discussion of a surface wind boundary condition is provided, and in model calibration, it also appears that wind was not included. It is reasonable to not include the impacts of wind on the model, since the observed short-term winds over the time period of model calibration are not likely representative of average conditions. However, when discussing the flow rating curves in the model validation on p.120, it appears wind may have been included in the model. "The 'bumps' and 'skips' of the flow rate curve (more evident in the model output) can be attributed to the effects of winds (i.e., atmospheric effects) on the water surface…" This seems to indicate that some wind effects were included in the model.

- In order to enhance the model verification to spatial and temporal changes in the currents, it would be worthwhile to compare the magnitude and direction of the depth-averaged current observations with the model velocity vector results. This would provide added confidence that the model is accurately identifying the flow dynamics.

#### *2.2.3.3 General Comments*

- p.83 - The MEP report indicates that, "Tidal flushing will be utilized as the basis for a quantitative evaluation of water quality." This statement may result in some of the confusion related to tidal flushing, since the flushing rates and residence times presented later in the chapter are really qualitative measures used to provide an initial estimate of the tidal flushing associated with each sub-embayment. Ultimately, the more detailed water quality modeling effort, which directly utilizes the results of the hydrodynamic model, more accurately (and quantitatively) describes how tidal flushing influences the nitrogen concentrations within the Pleasant Bay System.

- p.85 - There is a citation for Kelley et al. (2003) in the first paragraph that appears to be missing from the references.

- p.97 - Figure V-8 uses a PLB nomenclature for the tidal stations; however, this nomenclature is not used to refer to the tidal stations at any other location in the report. It may be useful to include a table that ties the geographic nomenclature used throughout the rest of the report with the PLB nomenclature used in Figure V-8.

- p.85 – The last paragraph indicates that "…data from the five TDR stations within the system were used to calibrate and verify the model performance…"; although there are 6 TDR stations within the system that are used for calibration, while ADCP data were used for model verification.

- p.110 - There is a reference to Howes et al. (2005) in the first paragraph that appears to be missing from the references.

#### *2.2.4 Response to WMVDC Comments*

This section provides direct response to commentary and questions raised by the WMVDC. WMVDC comments/questions are presented in italics, with our observations and responses provided below each WMVDC comment. These comments are addressed separately for completeness, and because the comments did not provide the basis for the independent peer review presented in other sections of this report. Absent this section, some of the issues of importance to the WMVDC would have not otherwise been addressed.

*The bathymetry surveys were conducted in 1997, 2000, and 2004. Would there have been any significant changes to depth in the upper ponds between 1997 and 2004?*

- Response: It is unlikely that any bathymetric changes occurred between 1997 and 2004 that would significantly impact the circulation dynamics or mixing characteristics of Pleasant Bay. However, it may be useful to show the added coverage that was provided by the NOAA data, as well as the relative time scales when they were collected.

*Seven tide gauges were located in the Pleasant Bay System, figure V-8. Why weren't any tide gauges located along the eastern edge of Pleasant Bay, that is, along the Barrier Beach? How does the hydrodynamic model interpret the tidal rise in the eastward direction for the tide gauges in the Western section of Pleasant Bay, i.e., PLB4 and PLB5?*

- Response: It is likely that the six tide gauges deployed within the Pleasant Bay estuary were all deployed on the western edge of Pleasant Bay due to ease of access, as well as the availability of various structures and features for securing the tide gauges. The locations of the deployed tide gauges allow for adequate calibration of the hydrodynamic model and do not create any difficulties in the ability of the model to simulate the water surface elevation and/or circulation throughout the system. Potential variations in the water surface elevation between the western and eastern end of the embayment is likely negligible, and the hydrodynamic model is capable of identifying any unique variation in water surface elevation or circulation through time varying calculations using the governing equations. Unique circulation patterns that may develop within Pleasant Bay still can be identified by the hydrodynamic model regardless of the placement of the tide gauges. However, it would be useful to see maps or animations of tidal current predicted by the numerical model for different stages during the tidal cycle (e.g., slack high, peak flood, slack low, peak ebb, etc.). These maps would illustrate any unique circulation features that may develop, as well as show the overall circulation dynamics of the system.

Therefore, although it is likely that circulation patterns are more complex than a simple north/south propagation of water, as documented by Horne and Horne (2001), these features should be identified by the hydrodynamic model. Additionally, it is likely that these smaller magnitude circulation features have minimal impact on nitrogen transport.

*Explain the reason for the difference between "Measured Tide" and the "Predicted Tide". How do these data relate to the July-August time-frame? Why were the measurements made in October-November, rather than in July-August, when most of the remainder of the report was addressed? Are the local velocities and gradients different in the summer months due to solar heating of the bottom sands and other materials?*

- Response: Measured tide refers to the actual observed time series of water surface elevation changes, while predicted tide refers to the decomposition of the observed time series of water surface elevation into known tidal constituents. These tidal constituents (e.g., M2, M4, S2, etc.) can be combined to create the time series of predicted tide that essentially removes all non-tidal processes (e.g., wind, rain, etc.) from the time series of water surface elevation. This allows for creation of a time series of only tidal processes, and eliminates any short-term, non-tidal factors from the observed data. It does not matter when the water surface elevation changes were observed, since there will be no change to the tidal-forced water surface elevations in various seasons. Tides, caused by the relative position of the Moon and Sun to the Earth, will not change based on the season. Therefore, the same tidally based water surface elevation changes will occur in all seasons.

*The data in the middle chart (Fig. V-14) show some negative cross-current velocities in the middle of the channel, showing significant mixing patterns. From the middle chart (Fig. V-15.), it appears that there was mixing in the cross-channel direction. Are the cross channel currents included in the model?*

- Response: Depth-averaged cross channel currents are included in the model. Again, it would be useful to see all temporal transects from the ADCP survey, which would provide a better understanding of the flow dynamics at the channel cross sections.

*The top chart (Fig. V-16) for the West Bay channel (entrance to The River from Little Pleasant Bay) velocities through the main channel of 1 to 2 ft/sec, but at the southwest bank of "backflow". The ebb flow in the West Bay Channel seems to be of moderate velocity (Fig. V-17). The middle chart appears to show reasonable cross current velocities, or good mixing. The charts also seem to show very low velocities along the bottom surface. Does the hydrodynamic model reflect these types of flows as well?*

- Response: The hydrodynamic model is capable of capturing all these types of flow. As discussed, it may be useful to present a comparison of modeled and observed current observations to ensure the model was identifying the temporal and spatial currents.

*Is the "two-dimensional" modeling sufficient throughout the Pleasant Bay System?*

- Response: Refer to section B.1 of this review.

*"Various friction and eddy viscosity coefficients were adjusted…" These parameters, or constants, were selected by trial and error, until a solution was obtained that satisfied the boundary conditions? Is there necessarily a single solution? There were no tide gauges on the east side of larger Pleasant Bay; would this cause a deficiency in the analysis?*

- Response: These parameters are adjusted until the modeled water surface elevations accurately match the observed water surface elevations throughout the Pleasant Bay system. The hydrodynamic model appears to incorporate a reasonable set of coefficients, within acceptable ranges (except perhaps for the culverts), that match the observed data. The lack of tide gauges placed along the eastern side of Pleasant Bay would not result in a deficiency in the modeling effort.

*The model was calibrated in November 2004. This is a period where the bottom surface is colder than during July and August. Would all of the coefficients used in the "model" be temperature sensitive, and therefore be different in July and August?*

- Response: No, the model coefficients are not temperature sensitive, and the calibration would be the same since the forcing tides are independent of seasonality.

*The Manning roughness coefficients were fairly close, except for Pleasant Bay and the culverts at Frost Fish and Muddy Creek. Is there an explanation?*

- Response: The Manning's roughness coefficients for all the embayments fall within the range of accepted frictional coefficients for natural systems. As discussed in section B.1, the methodology used to determine the closed conduit flow for the culverts of Frost Fish Creek and Muddy Creek is not presented. The frictional coefficients for the Frost Fish and Muddy Creek culverts are significantly higher than Manning's coefficients typically used to determine flow through a culvert (Chow, 1959; Barfuss and Tullis, 1994; Barfuss and Tullis, 1988; Bishop and Jeppson, 1975; Neale and Price, 1964; Tullis et al., 1990; Norman et al., 2001), as discussed in section B.1.

*In Figures V-19 through V-25, the report shows a comparison of the model computed tides and the observed water levels at various locations throughout Pleasant Bay. All this is based on measurements taken over five days beginning November 13, 2004. This* *was the period of calibration. How did the model predict the tidal behavior in a time period in future years?*

- Response: The model has been accurately calibrated to the tides that currently occur within Pleasant Bay. The forcing oceanic tides, which are based on the relative position of the sun and moon to the earth, will remain the same in the future. However, this does not mean that the circulation and dispersion within the Bay will remain the same in the future. Factors such as bathymetric changes, geometry changes, anthropogenic impacts, inlet migration, barrier beach breaches and overwash, and sea level rise may all influence the tides and circulation within Pleasant Bay. This is not something the model can be expected to predict. However, the model can be used to determine what may occur under these various scenarios, which is the real utility of numerical modeling. The hydrodynamic model, as calibrated, is a useful tool to project these future changes to the system, and has been used to evaluate various future scenarios already (e.g., inlet migration, breaches, etc.).

*In Figure V-27, the data are presented for the "mouth of the River" on November 23, 2003. Is the "mouth of The River" referred to here the same channel referred to in Fig V-17 as the "entrance to West Bay".*

- Response: Yes, we assume these are the same.

*The "system residence time" seems to be a strange measurement. Does this measurement mean to imply that it will take over a year to completely flush out Muddy Creek, Round Cove, Pah Wah Pond, Areys Pond and Lonnies Pond [Table V-8]? Doesn't this parameter suggest that the fluid from these ponds just move in and out with very little exchange with the incoming tide? Does this imply an assumption of plug flow in the embayments and sub-embayments? It does not seem to be logical. Does the Pleasant Bay system work this way? How is "system residence time" used in the hydrodynamic model? Is this an appropriate way to describe the transport of nitrogen from a sub-embayment to the Atlantic Ocean?*

- Response: The concept of residence time, at least as presented in the MEP report, requires clarification in terms of how it relates to the actual nitrogen modeling. In the end, it is our understanding that the flushing rates and residence times presented in the MEP report are qualitative measures used to provide an initial estimate of the tidal flushing associated within each sub-embayment. However, due to the quantitative presentation of tidal flushing and residence times, the MEP report can easily be misinterpreted. Ultimately, the more detailed water quality modeling effort (RMA-4), which directly utilizes the results of the hydrodynamic model (RMA-2), more accurately (and quantitatively) describes how tidal flushing influences the nitrogen concentrations within the Pleasant Bay System. The residence times presented in Table V-8 are not utilized in computation of nitrogen concentrations or dispersion. Therefore, it may be somewhat confusing to the average reader since the residence time does present quantitative values and the MEP report does spend a fair amount of time discussing these values. As stated in the MEP Report, "Residence times are provided as a first order evaluation of estuarine water quality." Since the MEP approach uses numerical water quality modeling directly to determine the nitrogen concentrations (which should be far more accurate than simple residence time calculations), the residence time calculations based on tidal prism become almost irrelevant. In fact, RMA-4 could be used to directly provide quantitative residence times values if the RMA-4 model was applied to determine flushing rates (Letter et al., 2008; EMS-I, 2000).

Although the calculations of residence time in the MEP report are simplified (based primarily on consideration of tidal prisms) and their rationale and application are somewhat questionable, given that these values are not ultimately used in the water quality modeling, they should not be viewed as a major concern. The flushing rates presented are simply mathematical estimates of each of the subembayment residence times that should be viewed as estimates and relative comparisons.

## 2.3 WATER QUALITY MODELING

### *2.3.1 Primary Comments*

#### ***2.3.1.1 Water Quality Boundary Conditions (Bioactive vs. Total Nitrogen)***

Of the approximately 33 MEP estuaries that have been completed to date, only the Pleasant Bay system (and its sub-embayments) has used bioactive nitrogen (as opposed to total nitrogen), for calibration and threshold development. The Bassing Harbor system (Howes et al., 2003), a smaller sub-embayment system of Pleasant Bay, also used bioactive nitrogen for model calibration.

This discrepancy (bioactive versus total nitrogen) is not well documented in the MEP Pleasant Bay report, at least in the modeling section of the report. For example, Table VI-2 in the MEP Pleasant Bay report provides the present loading conditions for total nitrogen modeling of the Pleasant Bay system; however, the water quality modeling is calibrated to bioactive nitrogen. Additionally, all modeling results, both present conditions and scenarios, present only bioactive nitrogen. This is likely a source of significant confusion to the average reader. For example, to the average reader, if the loadings are total nitrogen, then it would be intuitive that model-data comparisons should be in terms of total nitrogen. If the bioactive nitrogen is actual being modeled, then it would seem logical that a bioactive loading boundary conditions needs to be applied. As such, there needs to be clarification on the boundary conditions that were used in the water quality modeling, and the calibration technique applied.

In the Pleasant Bay MEP report, total nitrogen loading is applied and then compared directly to the bioactive nitrogen data, since the non-bioactive component of nitrogen (DON) is: (1) a component that is not included in the loading analysis; (2) is generated solely from sources in the water column, which RMA-4 cannot reproduce accurately; and (3) the DON is refractory (i.e., does not contribute significantly to phytoplankton production). As such, the DON portion of the data observations is not an important component of the total nitrogen. If this is true, then it is confusing that bioactive nitrogen is not used for model calibration and threshold concentrations in all MEP estuaries.

From the Bassing Harbor report (Howes et al., 2003), model calibration results are presented both for a total nitrogen simulation and a bioactive nitrogen simulation. The RMA-4 model does not calibrate well to total nitrogen results, and thus, the calibration technique was modified to compare to only bioactive nitrogen, such that the model comparison was improved. For these two simulations, it is assumed that the loadings stay the same (total nitrogen for atmospheric, watershed, and benthic flux), yet the modeling results are different (Figures VI-3 and VI-4 of the Howes et al, 2003 report) based solely on the background nitrogen level assigned at the boundary condition. Therefore, the background value assigned appears to have a significant influence on the overall nitrogen concentrations in the system.

Overall, the calibration technique, which appears to be different for Pleasant Bay then the remaining MEP estuaries, raises some concern over the ability of the water quality model to accurately simulate the total nitrogen concentrations that were observed. For example, this discrepancy indicates that RMA-4 has limitations simulating DON within the estuarine system, likely due to the lack of geochemical processes within the model (section 2.3.2.2). Ultimately, it may be completely reasonable to simulate only the bioactive portion of nitrogen, if DON is not an important contributor to environmental stress. This is outside the area of expertise of the numerical modeling reviewer.

#### *2.3.1.2 Selection and Sensitivity of Dispersion Coefficients*

The calibration of the water quality model to bioactive nitrogen and salinity was performed by adjusting dispersion coefficients until the model results of concentration closely matched the observed data. The reasonably accurate representation of the time-averaged 2-D concentration fields that was achieved with the 2-D model through the adjustment of the longitudinal dispersion coefficient suggests a high degree of tuning due to the high spatially variable in the model application. It is unfortunate that a model-data comparison of time-dependent concentrations (salinity or nitrogen) was not available and/or presented in the MEP Pleasant Bay report. The reported values of the model diffusivity are large in some areas, compared to those presented both in the RMA-4 Users Manual (Letter, 2008) and other sources (EMS-i, 2000). The larger values of diffusivity also contrast with many modern applications of 2-D and 3-D models in which the diffusivity is relatively small (i.e. controlled by the numerical scheme). The comparison with estimates of longitudinal diffusivity (*E)* in the Fischer et al. (1979) text are perhaps somewhat misleading, because the Fischer et al. (1979) values are laterally and vertically averaged over the cross-section of the channel. In other words, Fischer et al. (1979) values of dispersion were originally intended for assessments where the dispersion values needed to include diffusion caused by vertical and horizontal circulation dynamics. These dynamics were typically not explicitly included in the 1-D models that were considered state of the art in the late 1970s. This explains the potential order of magnitude differences between the RMA-4 recommended dispersion values (Letter, 2008) and those presented in Fischer et al. (1979). In theory then, the diffusivity (*D)* in a

2-D or 3-D model should arguably be much smaller than the Fischer et al. values because the scales responsible for dispersion are resolved by the model and thus do not need to be represented in terms of a large effective diffusivity. In other words, the 2-D model should be capturing a good portion of the horizontal dispersion dynamics that cause dispersion to occur. The larger dispersion coefficients that are required to calibrate the RMA-4 model may indicate that there are some important processes that are not being adequately captured by the hydrodynamic model (i.e., 3-D processes) or there is inadequate resolution to capture important 2-D processes.

However, larger than recommended dispersion values are not necessarily an indication of poor model accuracy. Higher than average dispersion coefficients typically indicate that the model requires increased mixing ability in order to reproduce match the observed data. If the selection of higher dispersion coefficients can be justified or explained by the lack of the model’s ability to resolve or represent known physical processes, then higher dispersion coefficients can be reasonably assigned to account for these non-simulated mixing processes. Therefore, the larger dispersion coefficients required to calibrate the RMA-4 model may indicate there are some important processes not adequately captured by the hydrodynamic model (i.e., 3-D processes), or there is inadequate model resolution to capture important 2-D processes. The higher dispersion coefficients may also indicate that the nitrogen input values (benthic flux, atmospheric, watershed load) are overestimated such that increased dispersion is required in order to match the observed nitrogen data.

The MEP Pleasant Bay report also does not discuss the potential variation in dispersion coefficients in the x and y directions, which are required input in the RMA-4 model. It is likely that the same dispersion coefficient was used for both $D_x$ and $D_y$; however, this should be stated in the MEP report.

Perhaps significantly more important than the actual magnitude of the dispersion coefficients is their spatial variability and highly sensitive nature. A sensitivity analysis was conducted by Howes et al. (2001) for certain parameters in the MEP linked model. The sensitivity analysis revealed that by far the most sensitive parameter in the overall linked model approach was the dispersion coefficient assigned in the water quality modeling. The Howes et al. (2001) sensitivity assessment found that by changing the dispersion values used in the calibrated model by 2 times or -0.5 times the selected value could change the nitrogen concentration anywhere from -19 to +93% depending on the location in the estuary. Therefore, even a small change in the dispersion coefficients would have a significant impact on the predicted nitrogen levels. Howes et al. (2001) expresses that although the nitrogen results are very sensitive to the selection of the dispersion coefficients, there remains a high confidence in the dispersion values used in the calibrated model since the modeled nitrogen values have to reasonably match the observed nitrogen data. However, based on the variations and limitations in the actual calibration and verification methodology applied in the water quality model (discussed in section 2.3.1.3), as well as the potential limitations of the RMA-4 model itself (discussed in section 2.3.2.2), it is not unreasonable to consider that modified dispersion coefficients could produce a water quality model that is still considered calibrated with measurably different nitrogen concentration levels.

The dispersion values must be selected to ensure the model results match, as closely as reasonable, the observed nitrogen values. Based on the information provided in the MEP Pleasant Bay report, the dispersion coefficients selected do allow for adequate prediction of model results to the observed data. Therefore, Howes et al. (2001) express confidence in the selection of the dispersion coefficients even though the results are highly sensitive to the dispersion values.

Woods Hole Group understands the dispersion coefficients must be chosen such that the model results match the measured data, and based on the given input conditions and the measured data, the dispersion coefficients may need to be similar to those selected by MEP. However, there is not likely one unique set of dispersion coefficients that can be selected to achieve a reasonable match between model results and measured data. Due to the large variability of the dispersion values, as well as the sensitive nature of the selection of the dispersion values on the RMA-4 nitrogen concentration results, Woods Hole Group is concerned that a small change in a dispersion value may result in a significant change in the nitrogen concentration results. Therefore, it seems reasonable to perform a site-specific sensitivity assessment on the dispersion coefficients for the Pleasant Bay estuary to help bound the nitrogen concentration results for existing conditions and future scenarios.

#### *2.3.1.3 Calibration and Verification of RMA-4*

The calibration and verification of the water quality model (RMA-4) was completed through comparison to samples taken of nitrogen and salinity at stations throughout the estuary. The MEP investigators did a reasonable job with model calibration considering the model needed to be developed for average estuarine conditions and there were temporal restrictions on the data (water quality data was not collected in concert with the hydrodynamic data). However, the calibration and verification process applied still leaves some concerns. Although the calibration and verification of the water quality model is reasonable, improved confidence in the model's ability to accurately simulate nitrogen for existing and future scenarios could be improved. Specific limitations include:

1) A significant amount of averaging of both the data samples and the model results may conceal specific model limitations - As discussed, it is unfortunate that the report includes only a time-averaged test of the ability of the model to simulate the measured nitrogen and salinity fields. The lack of a time-dependent comparison leaves uncertainties regarding the quality of the advective processes represented in the 2-D model. There is a considerable amount of averaging of both the data and the model results that may mask potential limitations of the model. Data samples are averaged over each season, and subsequently averaged again over all years. Model results are averaged over the entire simulation time period. The intent is to provide a reasonable comparison of average estuarine conditions. However, the averaging also makes it relatively uncertain how the model is performing during specific times of the tidal cycle or for specific temporal conditions in the estuary. It is uncertain whether the model has the capability to simulate the ebb and flow of waterborne constituents, such as salinity and nitrogen, with each tide. As such,

even though no time series observations were available, it would be useful to present the time series results (time-varying salinity and nitrogen concentrations) from the water quality model. The time-varying model results would provide additional assurance that the selected calibration methodology and averaging approach is reasonable. For example, the time-varying nitrogen concentrations at mid ebb tide in the time series of model results could be directly determined, and the methodology of simply selecting the mid-point between maximum modeled bioactive nitrogen and average modeled bioactive nitrogen values to adjust for sampling at mid ebb tide could be verified.

2) In addition to the extensive averaging, there is an inconsistent methodology for comparing modeled and measured results - In general, the methodology for calibration and verification of the water quality model appears to be inconsistent. For example, in the Pleasant Bay model calibration, the mid-point of the maximum modeled bioactive nitrogen and the average modeled bioactive nitrogen was compared to the mean measured bioactive nitrogen at each water quality monitoring station since sampling results were collected during mid ebb tide. However, for model verification, the modeled salinity values are not adjusted for the mid ebb tide sampling. For salinity, the average modeled salinity is compared to the averaged measured salinity. Further discrepancies exist in other MEP Reports. For example, in the Howes et al. (2003) study of Pleasant Bay sub-embayments in the Town of Chatham, the calibration targets were set such that the means of the measured data would fall within the range between the modeled maximum and modeled mean concentrations for station with a wide range of modeled concentrations, while those with less variability compared the mean value of both the model and data. This variation in the calibration methodology should be more clearly explained and justified to improve model credibility.

3) The calibration and verification process does not provide any quantification of the potential error or range associated with the nitrogen concentration results – The final nitrogen concentration results are provided as a single value with no associated error or bounds, and all potential scenarios (e.g., sewer cases, no anthropogenic load, build-out, etc.) are based on the single results. Given that the observed nitrogen samples, as well as the model results, indicate some significant variations, and given the significant averaging that is performed, it may be useful to provide a possible range of solutions based on a reasonable range of the observed data. This could be done by evaluating model results for a sensible selection of the upper and lower bound of the observed nitrogen samples.

#### *2.3.1.4 Background Nitrogen Concentration Level*

The background nitrogen concentration in the Atlantic Ocean region offshore of Pleasant Bay was set at 0.094 mg/L based on data collected at station PBA-17A in the summer of 2005. This value was calculated on a single summer of data, which introduces potential atypical seasonal processes. Considering MEP requires a minimum of three years of baseline field data within the estuary in order to be simulated in the linked-model approach, and multi-year averages are used for model-data comparisons, it would be reasonable that the background levels should also be sampled over multiple years. Although the data may not have been available at the time of the report, bioactive nitrogen concentrations observed at the same station (PBA-17A) and analyzed by SMAST during the summers of 2006 and 2007 were 0.079 mg/L and 0.071 mg/L, respectively. This indicates a potential short-term bias in the background concentration, and of the data observed, MEP uses the highest seasonal average. A reduction in the background concentration level may result in an approximately equal reduction of the bioactive nitrogen levels throughout the estuary based on the simulation of various background nitrogen levels applied in Howes et al. (2003) for Chatham subembayments within the Pleasant Bay system.

### *2.3.2 Secondary Comments*

#### ***2.3.2.1 Mass Conservation in RMA-4***

The RMA-4 model is sensitive to mass gain/loss across large closed boundary angles. The suggested minimum boundary angle between any two adjacent elements is 10° (EMS-i, 2000; Letter, 2008). This applies not only to the outer mess boundary, but also to the wet/dry interface. Correct grid development means that any part of the finite element grid that dries will create a new wet/dry interface that remains relatively smooth. The MEP Pleasant Bay model applies element/nodal elimination for areas in the Pleasant Bay system that experiences wetting and drying during tidal fluctuations. This can cause significant problems with the conservation of mass. Additionally, the Pleasant Bay model grid contains numerous locations where the elements do not maintain a boundary angle of less than 10° (e.g., many of the inlet elements to the smaller sub-embayments).

The MEP Pleasant Bay report does indicate that a continuity of mass equation check is utilized to ensure that mass is conserved (p.132); however, no details on the mass conservation approach or the percent error in mass conservation is presented. Considering the significant mass conservation errors that can occur in finite element models, it would improve model confidence if the mass conservation checks were quantified. For example, RMA-4 version 4.5 provides a mass conservation check that can be implemented across many continuity lines, and produces a summary table that gives a percent gain/loss value for the constituent (in this case nitrogen) being simulated. This would ensure that the nitrogen modeling is not experience a significant gain or loss in nitrogen concentration caused by the numerical errors within the model.

#### ***2.3.2.2 RMA-4 Model Selection***

RMA-4 was created to evaluate a substance that is evenly distributed vertically in the water, and evaluates the mixing and migration processes in depth-averaged flow regions. It will not provide accurate concentrations for stratified situations, and based on the data available, this may be the case for some of the smaller upper embayments within the Pleasant Bay system. One could also argue that 3-D measurements and a 3-D model are the state of the art for a project with large financial stakes such as the TMDL initiative for Pleasant Bay.

RMA-4 should not be considered state-of-the-art in water quality modeling, particularly for complex systems. Applications of the RMA-4 model have shown limitations in the models ability to model wetland nitrogen (Capps, 2001) and contaminant concentrations (Haralampides, 2000). Based on the work of Capps (2001), The RMA-4 models nutrients only superficially, thus, it is sufficient as a screening tool, but not robust enough to investigate wetland nitrogen processes. For example, RMA-4 can only simulate a first order decay rate, which oversimplifies the biochemical processes occurring as nitrogen evolves in an estuary, especially processes that may occur in shallow pond areas.

Although the MEP Pleasant Bay report indicates that the overall approach models total nitrogen as a non-conservative constituent, there are no dynamic biochemical processes that are occurring in the model. All of the boundary conditions and loading rates, including the benthic flux source and sinks, remain constant throughout the simulation. State-of-the-art models for nutrient mixing and transport are much more complicated, and involve numerous biochemical reactions, sources/sinks for each state variable, and interactive chemical processes. This includes depositional flux, diagenesis flux, and a wide range of sediment flux calculations between the water column and layers of the sediment. These types of dynamic sediment/water hydrodynamic and biochemical time-varying models are being regularly applied for TMDL studies in many other national regions (e.g., Christina River Basin, DE, Puget Sound, WA, San Diego Bay, CA, Anacostia River, DC, Lake Michigan, MI, Brunswick River, GA).

Overall, there are certain complex dynamics of Pleasant Bay that are not well-described by RMA-4.

### *2.3.3 Minor Comments*

This section provides questions and/or comments intended to request for clarification of the MEP report, and may not be significant in terms of the overall modeling results:

- Assuming a vertical uniform concentration of either salinity or nitrogen does not appear to be a reasonable assumption for the entire Pleasant Bay estuary system (as discussed in section 2.2.1). Although some limited conductivity, temperature, and depth (CTD) profiles (a single cast) were collected in some of the smaller Chatham subembayments such as Crows Pond, Taylors Pond, and Little Mill Pond (Howes et al., 2003), the MEP report presents no data to verify the assumption of vertical uniformity in the upper sub-embayments of Pleasant Bay system.

- The specification of the nitrogen loading boundary conditions is ambiguous and could be clarified for the average reader. For example, the MEP report indicates the benthic regeneration and direct atmospheric loads were evenly distributed among another subset of grid cells which form in the interior portion of each basin. It would be helpful if the subset of interior cells and rates of flux were presented.

- In order to project benthic loading for future scenarios, the benthic flux values are assumed to vary proportional with the watershed load. This approach seems to be oversimplified and should be verified through literature and studies. This is further discussed in the benthic flux review topic area in this report.

#### *2.3.4 Response to WMVDC Comments*

This section provides direct response to commentary and questions raised by the WMVDC. WMVDC comments/questions are presented in italics, with our observations and responses provided below each WMVDC comment. These comments are addressed separately for completeness, and because the comments did not provide the basis for the independent peer review presented in other sections of this report. Absent this section, some of the issues of importance to the WMVDC would have not otherwise been addressed.

*Is Pleasant Bay quiescent? Are there not significant currents producing mixing? If there were significant currents, would higher dispersion coefficients be needed to reflect the currents in Pleasant Bay?*

- Response: The circulation dynamics within Pleasant Bay are likely adequately described by the hydrodynamic model and these currents are directly input into the water quality model. Refer to section 2.3.1.2 of this review related to dispersion coefficients.

*Why do the multi-year averages present the "best" comparison? What happens if all values are included in the comparison?*

- Response: No comment

*Why is total nitrogen used as a "non-conservative constituent, where bottom sediments act as a source or sink of nitrogen…"?*

- Response: This statement simply refers to the specification of the benthic flux nitrogen loading rates in the water quality model. A negative benthic flux is a sink of nitrogen (removes it from that subembayment at the specified rate), while a positive benthic flux is a source of nitrogen (adds it to the subembayment at the specified rate). Therefore, the MEP report refers to this exchange as non-conservative.

*Has the model been used for winter conditions? If no, why not? Has SMAST considered year-around material balances in its calculations?*

- Response: It does not appear as though the model has been applied to winter conditions; however, this is reasonable since summer conditions represent the worst case for nitrogen impacts within the estuary system. The worst case season should be used to ensure that the nitrogen concentrations meet threshold levels throughout the year. If averages over all seasons were utilized, nitrogen concentrations would likely exceed the thresholds during the summer seasons.

*Is there only one set of dispersion coefficients that provide a solution to the inputs and boundary conditions, or are there multiple combinations? In other words, is there a unique solution to the mathematical computations?*

- Response: Based on the calibration and verification metric used in the MEP Pleasant Bay report, which involves significant averaging, there may be more than one combination of dispersion coefficients that would allow the model to fit the range of data observed. Refer to section 2.3.1.2 of this review related to dispersion coefficients.

*At each time step the model computes Nitrogen over the entire grid and utilizes a continuity of mass equation to check these results. Question: Is this a Mass Balance calculation?*

- Response: Refer to section 2.3.1.4 of this review.

*If the model was operated with "total nitrogen" inputs, how did the RMA-4 model predict the "bio-active" nitrogen concentrations?*

- Response: Refer to section 2.3.1.1 of this review.

*Describes that an initial N concentration is applied to the entire model domain. Model was then run for a simulated 28 day spin-up period. Question: Why and for what reason? What is a "spin-up period"?*

- Response: Model spin-up is a standard procedure for simulation of water quality constituents. This is required in order to prime the model for the nitrogen loading rates by applying a initial uniform concentration to ensure the model is at a stable concentration. The recommended spin-up time is usually at least twice as long as it takes for the velocity field to move through the simulation area. The 28-day period is adequate to ensure that the model has adequately stabilized to the initial condition.

*In the watershed loads, why were the contributions from "Natural Background Watershed Loads" [Pleasant Bay Report, Table ES-1a, Page ES-11.] not included? See Table VI-2, page 134. Or were they included?*

- Response: This question is unclear, since the reference to Table ES-1a is not referenced.

*The report states that the concentration of "bio-active" nitrogen in the Atlantic Ocean was 0.094 mg/liter in 2005. Were any other measurements made at any other time? Shouldn't measurements have been made in at least one or two other years? Were* *measurements made of "total nitrogen" in the Atlantic Ocean, since this was the basis for the input data? In more than just 2005?*

- Response: Refer to section 2.3.1.6 of this review.

*Nitrogen loads for each sub-embayment watershed were evenly distributed at grid cells that form the perimeter of the sub-embayment. Question: Was attenuation by the fringing marsh applied in the model?*

- Response: If the fringing marsh area is included in the model, the effects would be included. It is unclear where and how much of the fringing marsh is in the model domain; however, it is likely that this has a limited impact on the nitrogen dispersion.

*"Calibration of the bio-active nitrogen model of Pleasant Bay proceeded by changing model dispersion coefficients so that the model output of nitrogen concentrations matched measured data." What does this mean?*

- Response: Dispersion coefficients, which can be modified for each sub-embayment, were adjusted until the model results best match the observed, averaged data of nitrogen concentrations.

*How is the concentration for "bio-active" nitrogen, DIN+PON, derived from the total nitrogen input data? What is the conversion?*

- Response: Please refer to section 2.3.1.1 of this review, as well as the data tables of the sampling results, which provide direct laboratory measurements of DIN, DON, and PON.

*Table VI-2 (pg. 134) shows that the total contribution of nitrogen (total N) from the direct atmospheric deposition (85.69 kg/day) and from benthic flux (184.519 kg/day) is 2.1 times the contribution from the watershed load of 127.203 kg/day. Aren't the contributions from septic systems, fertilizer, etc., relatively small?*

- Response: This question is unrelated to the water quality modeling, and involves the development of the nitrogen loading values.

*What does "yearly data" mean? Were measurements made throughout a year? Or does it mean the "average of several years"?*

- Response: It is our understanding that yearly data means refers to the average of all years of observed nitrogen data (between 1995 and 2004). The data were first averaged over each year, and then the yearly averages were averaged to arrive at a single value. For each year, measurements were made during the summer months only.

*Why is the one sigma measured data variance so large, especially for: Meetinghouse Pond, Upper Muddy River, and Frost Fish [CM-14]? The variance on the yearly measurements is significantly large, especially at: Upper Pochet, Upper and Lower Muddy Creek, and lower Frost Fish. Why?*

- Response: This is likely because the observed data is highly variable. In order to determine the exact nature of the variability all data observations would need to be evaluated. The data have not been provided for this review.

*If the data represent the average of measurements made over several years, they would not describe trends in the concentrations, either increases or decreases. Aren't trends, if they occur, important?*

- Response: Trends in the nitrogen data observations may or may not be important depending on a wide variety of factors. The data have not been provided for this review.

*How were the fresh water flows determined? Are they assumed to be the same for every year? How do they vary with annual rainfall and with municipal well production?*

- Response: Freshwater flows were determined from the USGS groundwater model, as presented in Table III-1 in the MEP Pleasant Bay report. They are assumed to be constant in the hydrodynamic and water quality model. A complete review of the USGS groundwater model would be required to determine potential impacts of annual rainfall fluctuations or municipal wells.

*The first paragraph jumps right into conclusions of significant increases in nitrogen loads due to build-out. The impact is all about the assumptions used and there is no hint of how this was generated. What assumptions were used? Are there decision choices that could be made to lessen the impact?*

- Response: Discussion of the buildout scenario is discussed in chapter IV (p.44) of the MEP Pleasant Bay report. Please refer to that section to determine how the buildout scenarios were developed.

*The model shows very poor correlation with field measurements for Upper and Lower Muddy Creek. Why?*

- Response: We agree the model is less accurate in the Muddy Creek area, which could be caused by a wide variety of factors, including the freshwater input, the ability of the model to simulate the flow control structures, etc. The exact reasons would need to be more thoroughly explained by the MEP technical team.

*The benthic flux values in Table VI-6 are slightly higher than in Table VI-2. What was the methodology for determining the higher values in Table VI-6?*

*The benthic flux for Meetinghouse Pond is shown as 7.40 kg/day, compared to 14.365 kg/day in Table VI-2. Does that mean that the difference, 6.695 kg/day of total nitrogen (46.6%), derives from vegetative and marine matter decay? How was this determined? Is this calculation satisfied by an annual material balance?*

*For Areys Pond, the number is 3.70 kg/day in the "no-load" case, compared to 5.669 kg/day in Table VI-2, or more than half. This means that in the current day scenario, in Areys Pond, the contribution from the sediments during a summer month is 3.70 kg/day (61.7%) from natural matter and 2.30 kg/day from (38.3%) anthropogenic sources. How was this determined?*

- Response: All of these questions relate to determine the change in net benthic flux for the various scenarios. The methodology for determining the decrease or increase in benthic flux values is presented on p.144 of the MEP Pleasant Bay report. In order to project benthic loading for future scenarios, the benthic flux values are assumed to vary proportional with the watershed load. As discussed, this approach seems to be oversimplified and should be verified through literature and studies, and is further discussed in benthic flux review topic area in this report.

## 2.4 IMPACTS TO INLET MIGRATION AND BREACHES COMMENTS

### *2.4.1 Primary Comments*

It is clear the breaches of 1987 and 2007 have improved tidal flushing, and subsequently nitrogen dispersion, within the Pleasant Bay estuary system. However, the Nauset Beach-Monomoy Island barrier system is, and will continue to be a dynamic system that is shaped by a variety of physical processes and storm events. No one can determine with complete certainty how long the current system will maintain its current flushing ability, nor can anyone determine when the next storm may reshape the Nauset Beach coastline, create or close an inlet, or significantly change the shoals. Therefore, if the intent is to continue to allow the Nauset Beach system to be reshaped by the constant natural dynamics, then the preferred target design must be selected that reasonably meets the water quality limits under a majority of natural inlet scenarios (e.g., 1987 post-breach conditions). If an inlet management plan is implemented, then it may be feasible to reduce the uncertainty in the physical changes of the inlet and constrain the design level for a specific flushing scenario. Ultimately, the preferred design level that will meet the water quality needs to be determined by the Towns surrounding Pleasant Bay.

The calibrated and verified model was developed to evaluated potential future scenarios, and it is sufficiently flexible to evaluate a range of refinements and adjustments caused by naturally occurring changes to the system (i.e., future breaches, historical inlet locations, etc.). The model has already been employed to complete these types of assessments (Kelley and Ramsey, 2008) and shown relative changes to the existing conditions case. As long as there is confidence in the existing conditions model, then relative comparisons to other scenarios should produce reasonable results. However, at this point additional scenarios offer little additional information, until a specific target inlet scenario is identified. Woods Hole Group has no concerns in applying the model to simulate hydrodynamic effects of potential inlet scenarios for the Town of Orleans.

#### *2.4.2 Minor Comments*

This section provides questions and/or comments intended to request for clarification of the MEP report, and may not be significant in terms of the overall modeling results:

- It is unclear if the pre-1987 breach conditions represent the worst case. Based on the work of Giese (1988), there may have historical conditions that would have created even worse tidal flushing.

- In the supplemental memorandum regarding Pleasant Bay Water Quality Model Update and Scenarios (Kelley and Ramsey, 2008), Table 2 shows a +2.3% change from background at Frost Fish Creek. This appears to be an error.

- Although the worst case scenario changes the boundary conditions to Nantucket Sound, it does not change the background concentration of nitrogen. The background concentration in Nantucket Sound should also be evaluated to ensure the correct value is selected.

#### *2.4.3 Response to WMVDC Comments*

This section provides direct response to commentary and questions raised by the WMVDC. WMVDC comments/questions are presented in italics, with our observations and responses provided below each WMVDC comment. These comments are addressed separately for completeness, and because the comments did not provide the basis for the independent peer review presented in other sections of this report. Absent this section, some of the issues of importance to the WMVDC would have not otherwise been addressed.

*"The flushing analysis performed for the present study also incorporates existing conditions and the 'worst-case' pre-breach conditions." Does this statement refer to the 'worst-case' scenario of Chapter IX?*

- Response: It is unclear if the analysis in chapter IX is considered worst case conditions based on subsequent scenarios simulated (Kelley and Ramsey, 2008), as well as the historical location of the inlet. It appears as though the simulation in chapter IX is a representation of the pre-1987 breach conditions.

*"As a suggestion, an inlet management plan should be developed to address possible future water quality problems that could occur as a result of less-than-optimal configurations of the Pleasant Bay inlet." What is possible? What can or should be done to maintain the hydraulic efficiency of the Pleasant Bay inlet system while maintaining suitable habitats for the flora and fauna of the estuary? Does anything need to be done in the next 75 years?*

- Response: Development of an inlet management plan would involve a commitment to maintaining a relatively stable inlet that would allow for consistent tidal exchange and less variability in terms of the hydrodynamic characteristics of the system. This may consist of maintain an inlet at a given location and of a given size through structural controls (e.g., jetties) and/or continual maintenance (e.g., dredging, bypassing of sediment, etc.) or creation of improved inlets once the water quality levels were below a predetermined level. An inlet management plan would attempt to control the variability of the inlet migration, breaches, and shoaling, while keeping the water quality at a high level. An inlet management plan could be initialized sooner rather than later and would likely start with determining the feasibility of inlet management options.

## 2.5 ADDITIONAL OVERALL COMMENTS

The MEP Pleasant Bay study and report is a comprehensive and highly technical report. Overall the modeling is well conducted and produces reasonable results compared to the observed conditions. However, missing from the report is a simplified synthesis of the system dynamics, backed up by model and data, and describing how all the components of the MEP linked model approach work together to arrive at a final solution. This synthesis would indicate that the authors basically understand the system and that they have reason to believe that the model results are reasonable. The processes within Pleasant Bay as a whole are extremely complicated, and the modeling includes (or should include) representations of poorly understood processes (phytoplankton blooms, grazing by zooplankton, benthic exchange, uptake of dissolved oxygen, etc.). Average readers of the report are basically confronted with a situation in which they must choose to believe or disbelieve the complex model results. A qualitative description of the transport processes and estimates of the rates, backed up by data and model computations, might help convince readers that the system is understood well enough to make predictions about important quantities.

Additionally, it is reasonable that for an estuary of this size and complexity and importance that an evaluation and selection processes should have been conducted for the numerical modeling software. In Howes et al. (2001), a detailed assessment of approaches for the entire linked watershed-embayment methodology was conducted; however, this same comparison and selection process does not seem to have been conducted for the numerical models themselves. Understanding the strengths and weaknesses of the RMA series of models for Pleasant Bay specifically, including a sensitivity analysis, would help quantify the uncertainty in the model predictions.

## 2.6 RECOMMENDATIONS

This section provides specific recommendations that should be implemented to reduce the concerns or address the comments provided herein. The recommendations are targeted on the primary concerns that may have the highest probability of potentially influencing the nitrogen concentration results.

It must be understood that applying changes to the model do not have a direct linear relationship. In other words, simply changing a variable in the model may modify a number of variables within the model, and subsequently influence the results in a variety of ways. It is difficult to predict the impact on model results of modifying a certain parameter without access to the model. The linked-model approach has a significant number of complex modules that do not have a direct cause and effect relationship. For example, if nitrogen inputs to the water quality model (e.g., benthic flux values, background nitrogen concentrations) are modified for existing conditions of Pleasant Bay, then the water quality model would require recalibration and revalidation. Ultimately, the existing conditions water quality model would need to reasonably match the observed nitrogen samples, and therefore, any changes made to the input conditions would require recalibration of the water quality model to ensure the model correctly represents the observed nitrogen data. For example, lowering the benthic flux values (i.e., less nitrogen supplied to the system by the benthic flux), would require a modification to another parameter within the water quality model to ensure model results match the observed values of nitrogen. Assuming that all the other input values remain the same (and only the overall benthic flux input is changed), this would require a modification to the dispersion coefficients. For the case of lowering the benthic flux input, the dispersion coefficients would likely need to be lowered (less mixing) so that the modeled nitrogen concentrations (which would be lower due to the reduced benthic input) still match the observed nitrogen data.

Therefore, it is important to recognize that the water quality model needs to achieve agreement between the model results and measured data. As such, the existing modeled nitrogen concentration results within Pleasant Bay will not appreciably change when input values are changed. However, the modification of inputs and/or dispersion coefficients may have significant impact on the model results for potential scenarios (build-out, no anthropogenic loading, sewer scenarios, etc.) being simulated. For example, if the dispersion coefficients are changed due to a modification of a nitrogen input value, then those new dispersion coefficients would result in different nitrogen concentration results for an alternative scenario.

Based on the primary concerns identified herein, the following recommendations may be considered to assist in improving confidence in the overall hydrodynamic and water quality modeling.

- Currently, the possible stratification of the drowned kettle ponds within the Orleans portion of Pleasant Bay is based on limited data taken within Areys Pond. Therefore, in order to verify the potential stratification of the drowned kettle ponds, and their relative importance on the overall nitrogen concentrations, temperature and salinity data should be collected as a function of depth in two or more of the kettle ponds. This data collection would consist of simple Conductivity-Temperature-Depth (CTD) casts throughout a summer season(s). If the collected data revealed a consistent stratification, as in the Horne and Horne (2001) data set, then the existing hydrodynamic and water quality model could be extended to three dimensions in these specific subembayments. There are a number of options that could be considered to evaluate the 3-D processes in the ponds, including:

1) The existing MEP model could be expanded to three dimensions for the entire Pleasant Bay system using RMA-10 (3-D hydrodynamics) and RMA-11 (3-D water quality)

2) The existing MEP model could be expanded to three dimensions in the terminal ponds only. The RMA series of models has the ability to combine both a 3-D grid portion and a 2-D grid portion

3) An independent 3-D model of a terminal pond could be developed and calibrated to the new data collected in the terminal pond. This model would assess the processes and dispersion of the nitrogen within a terminal pond. An independent model could also incorporate additional nutrient cycle physics that are not described by the RMA-4 model.

- The background nitrogen concentration used in the Pleasant Bay modeling was based on a single year of data (2005). Additional data collected in the two years (2006 and 2007) following the MEP modeling effort indicated that the background nitrogen concentration observed in 2005 may have been larger than average. The water quality model (RMA-4) could be used to re-simulate the Pleasant Bay system using an average value of all the observed nitrogen values at the Atlantic Ocean sampling station or test for a reasonable range of expected background bioactive nitrogen concentrations of the Atlantic Ocean. This would require the model to be recalibrated.

- Potential recommendation on resolution of the concerns related to the model's inability to represent total nitrogen, and the subsequent MEP justification of using the model solely for bioactive nitrogen modeling is unknown. This approach may be reasonable, but is difficult to assess without access to the data.

- The relative uncertainty of the dispersion coefficients relates directly to the sensitivity of this dispersion parameter in the water quality model (Howes et al., (2001). Since the calibrated, existing conditions model must reasonably represent the observed nitrogen concentration data, or at least the range of averaged data (as shown in Figure VI-3 of the MEP Pleasant Bay report), it seems reasonable to assess the potential range of dispersion coefficients needed to match the range of the observed data. In this manner, upper and lower bounds on the nitrogen concentration results predicted by the water quality model could be provided and a reasonable assessment of the potential error associated with the nitrogen concentration results could be offered. This may prove useful to help guide the Town of Orleans in future design efforts. As such, a site-specific sensitivity analysis performed on the dispersion coefficient and on a reasonable range of nitrogen input data would help improve model confidence and provide a possible range of nitrogen concentration results.

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## 3.0 HEALTH OF EELGRASS AND BENTHIC COMMUNITY

The charge for this analysis as outlined in the Request for Proposals is summarized in the italicized paragraph:

*In the Pleasant Bay Report, SMAST relates the health of eelgrass (primarily in Pleasant Bay and Little Pleasant Bay) and the benthic communities to nitrogen levels in the adjacent waters. The Total Maximum Daily Load (TMDL) calculated by the Commonwealth of Massachusetts DEP for the Pleasant Bay System is related to defined nitrogen concentrations. Using the Orleans area of Pleasant Bay as the basis, the consultant will evaluate: a) the relationship between the health of eelgrass and the benthic community and the composition of the local water column, including, but not limited to, total nitrogen, bioactive nitrogen, oxygen and salinity; b) the relationship between local oxygen concentrations (including hypoxia/anoxia) and nitrogen in the water column; and c) the history of eelgrass, whose decline is attributed primarily to nitrogen in the Pleasant Bay System, and assess any other mechanisms that might contribute to the observed decline.*

### 3.1 OXYGEN AND NITROGEN IN THE WATER COLUMN

The basic relationship between nutrients and dissolved oxygen in the water column is: nutrients stimulate algal growth throughout the water column and on sediments - anywhere where there is enough light. Algae, which produce oxygen during daylight, respire using oxygen day and night, and upon death use oxygen in decay processes. With excessive nutrient loading and eutrophication, hypoxia/anoxia results especially in bottom waters where dead algae accumulate. When nutrients are sufficiently reduced so that algal production is also reduced, more healthy conditions can return. This has happened, for example, in Lake Erie. Recovery also depends upon the level of nutrients stored in the sediments, which will be recycled before being flushed from the system. Cleansing of the groundwater is slow but sensitive to weather, and will occur faster in years with heavy rainfall. There are sufficient references for the downward turn in estuarine health in response to excessive nutrient loading (i.e., more nutrients and more hypoxia (Anderson and Gilbert, 2002; Bowen et al. 2001; Valiela et al. 1992; Paerl et al. 1995; McClelland & Valiela, 1998; Smith et al. 1999)); however, there are limited actual cases of recovery yet realized in estuaries to date.

### 3.2 BOTTOM OXYGEN AND BENTHIC POPULATIONS

The following literature review supports the methods and conclusions that MEP reached regarding the composition and diversity of the benthic fauna in Pleasant Bay. The innermost parts of the bay are characterized by low diversity and the presence of opportunistic species or no infauna due to nitrogen over-enrichment.

Low oxygen concentration (hypoxic or anoxic conditions) in the bottom waters is widely used as an indicator of conditions in an estuary (Kemp, R. et al. 2005). Complete lack of oxygen (anoxia), even for a short period, allows very few macro-organisms, including eelgrass, to survive (Anderson, Glibert et al. 2002). Many small infauna are the most resistant; their absence is a good indicator of anoxia. *Capitella* and *Streblospeo* are the most resistant of these; they don't compete well with worms requiring better oxygen conditions so their presence is good indication that conditions are severely degraded. *Capitella* survives and reproduces only in sediments with relatively high organic matter, an indication of organic pollution such as results from nitrogen pollution (Ramskov and Forbes 2008). In Narragansett Bay benthic infauna of this sort are associated with pollution inputs and sediments with high oxygen demand (Valente, Rhoads et al. 1992). Samples from San Francisco Bay showed these polychaetes associated with sewage discharge (Thompson and Lowe 2004). Research in Korea found a similar result though the species there were different from those in the U.S. (Lim, Diaz et al. 2006). In a bay in Portugal cleanup of pollution resulted in the gradual decline of *Capitellids* and their replacement by a more diverse infauna (Cardoso, Bankovic et al. 2007). A study of 248 coastal and estuarine sites in the Gulf of Maine (Maine, New Hampshire, and Massachusetts) used 49 different criteria for benthic condition of which 10 gave strong ability to detect damaged sites. Of these three, which included abundance of *Capitella*, and faunal diversity gave over 80% ability to detect poor quality sites (Hale and Heltshe 2008). Another study in Sweden developed a benthic quality index in which *Capitella* was indicative of the lowest benthic quality (Rosenberg, Blomqvist et al. 2004).

Anoxia can be a highly variable condition. Anoxia is often present in the wee hours of the morning when respiratory processes have used the oxygen and photosynthesis has been absent for hours (Taylor and Howes 1994). However tidal exchanges can alter this, bringing in oxic waters in the pre dawn hours if currents are strong enough. The MEP graphs of oxygen illustrate this well for Pleasant Bay. But even so, a few hours of anoxia can prevent most organisms from surviving.

Decreased oxygen concentrations have a direct, adverse effect on eelgrass. Like most plants and animals, eelgrass requires oxygen to survive and grow. Although eelgrass produces oxygen during photosynthesis, the plants also require oxygen in the water column to keep their cells healthy. In addition, most of the oxygen produced during photosynthesis is not stored in plant tissue (Greve, 2003).

A number of studies have documented adverse effects of low oxygen on eelgrass. Holmer and Bondgaard (2001) showed a reduction in photosynthetic activity, leaf elongation rate, and the number of leaves per shoot in eelgrass plants exposed to low oxygen conditions. Greve (2003) showed a rapid response to reduced water column oxygen levels in eelgrass meristems - the meristem[1] quickly turns anoxic when the surrounding water is anoxic. Hypoxic conditions in sediment can affect eelgrass flower development (Plus et al 2003) and plant growth (Glaub et al. 2005).

Hypoxia is generally accompanied by increased hydrogen sulfide levels due to the activity of anaerobic sulfidic bacteria in sediments. Exposure to high sulfide levels leads to a complete cessation in photosynthetic activity in eelgrass after several days of exposure (Holmer and Bondgaard, 2001). In muddy (sulfidic) sediments, the leaves cannot transfer sufficient oxygen to their roots and rhizomes during hypoxia resulting in

[1] The meristem is the part of a stem or root from which new growth starts – it can be thought of as a root that grows new sprouts.

death by influx of sulfide into roots (Pedersen et al. 2004). This suggests a dual effect of low oxygen and high sulfides in eelgrass decline in eutrophic waterbodies.

Figure 8 is a modified version of Figure 18 in Kemp et al. (2005). It illustrates how nutrients (here nitrogen) affect algal growth. A greater abundance of algae uses more oxygen at night. When the algae sink and die, decomposition results in hypoxia/anoxia at depth. This, in turn, causes eelgrass disappearance and replacement of stable infaunal populations with *Capitellids* and other opportunistic infauna.

Examples of environmental decline due to excess nitrogen in large systems such as San Francisco, Narragansett and Chesapeake Bays are not directly comparable to Pleasant Bay. They have received greater nutrient inputs and are also much larger in area. They are mentioned simply to illustrate the damaging effects of excess nutrients.

In summary, the changes in eelgrass in the inner parts of the Pleasant Bay System are most likely due to eutrophication (nitrogen loading), and exacerbated in some areas by boat disturbance of the bottom. Wasting disease and changes in circulation and sediment distribution certainly have had effects in the more open parts of the Bay with the latter probably more important.

### 3.3 EELGRASS DISTRIBUTION

Eelgrass has been widely used, along with other sea grasses in areas where eelgrass is rare or lacking, as an indicator of estuarine health. This is partly because higher plants are relatively easy to see and map. They are also sensitive to some types of pollution, especially eutrophication (see Figure 8, modified Fig.18 (Kemp, R. et al. 2005)).

Eelgrass coverage using existing data is difficult to assess with much accuracy. Using only the ’95 and ’01 photos from the MEP report, it does seem that there has been a decrease, although the absolute amount of this decrease is difficult to quantify. There seems little doubt that eelgrass abundance and density has diminished in the innermost parts of the bay where there is less tidal flushing. This is consistent with the conclusion that the decrease in grass is a result of increased nitrogen loading.

Nitrogen stimulates dense algal growth, which produces hypoxia as it decomposes upon death. Algae, both micro- and macro-, also reduce light penetration to the bottom and to eelgrass both by floating in surface waters and by growing on bottom substrates including eelgrass.

A 42-year record in Chesapeake Bay showed a close connection between nitrogen loading and anoxia (eelgrass distribution was not measured) (Hagy, Boynton et al. 2004). Reduced light penetration in Waquoit Bay was linked with nitrogen pollution and a reduction in eelgrass shoot density and bed area. This suggests the eelgrass decline resulted from reduced recruitment and shoot death rather than reduced growth rate (Hauxwell, Cebrian et al. 2003, 2006). A thorough review of the scientific literature on eelgrasses decline and recovery round the world illustrates the strong connection between eelgrass abundance and nitrogen pollution and the complications that can arise in this relationship (Burkholder, Tomasko et al. 2007). Short and Burdick (1995) quantified the effects of excess nutrient loading on eelgrass in mesocosms. Excess nutrients caused a significant reduction in eelgrass growth, density, and biomass. The reduction in eelgrass biomass was linearly related to light levels. In addition plant morphology changed, with leaves becoming longer under high nutrient/low light conditions. These changes in eelgrass bed structure and biomass were brought about by stimulation of various forms of algae (microalgae, macroalgae, epiphytes) by excess nutrient loading.

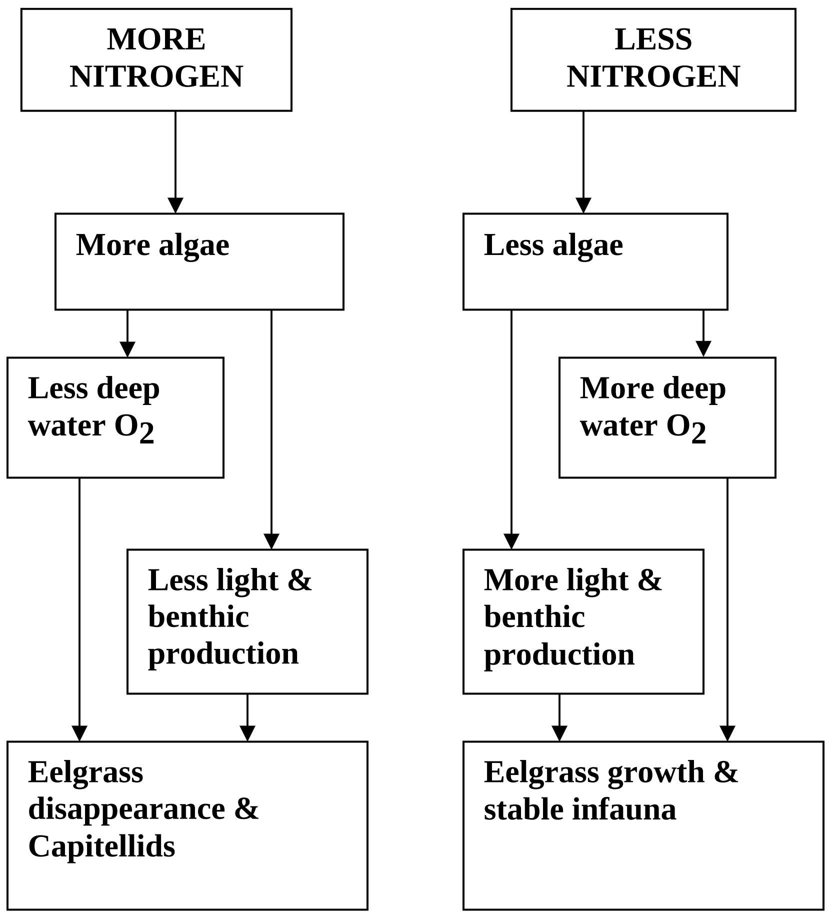


**Figure 8 Affect of nitrogen on algal growth.**

The MEP statement that the eelgrass in these inner areas has been replaced by macroalgae reinforces the conclusion that nitrogen pollution has been the cause of eelgrass decline in the inner bay (Anderson, Glibert et al. 2002; Bintz 2002). This has been found in other areas such as Waquoit Bay in Falmouth (Short and Burdick 1996; Bowen and Valiela 2001; Lee, Short et al. 2004). For example Short and Burdick showed eelgrass growth, shoot density and recruitment were all directly related to light levels. Algae were stimulated by nutrients, shaded eelgrass and resulted in vegetation change from eelgrass to various forms of algae, macroalgae, epiphytic algae growing directly on eelgrass leaves and phytoplankton. Also see below for Mumford Cove.

There are certainly losses of eelgrass as the result of new openings in the barrier island as sediments are moved and the grass is buried. The losses in these areas, near the new openings where flushing is greatest, are not the ones that are important in judging whether or not nitrogen loading is damaging eelgrass beds. That there was a large bay scallop harvest in '83 is consistent with good eelgrass beds in the more open parts of the system, but does not tell us about what was happening in the more confined parts. There were no N measurements in the bay in 1951, but we know there is more N in the system now because we know that the human population (both year round and seasonal) has increased, increasing the load of N to groundwater and hence to the bay. Valiela, Foreman, and Bowen (Valiela, Foreman et al. 1992; Bowen and Valiela 2001) have shown that there is more N entering the Cape's coastal systems in the latter years of the last half century.

Eelgrass wasting disease, responsible for the great declines in the 1930's, is still affecting eelgrass populations. The infection and effect on eelgrass populations is mostly limited to high salinity waters and is significantly less in brackish waters (Short, Muehlstein et al. 1987; Short, Ibelings et al. 1988; Short, Burdick et al. 1995). Since the definite decline of eelgrass in Pleasant Bay is in the slightly more brackish areas and the persisting populations are in the more saline parts of the Bay, the decline of eelgrass in the inner parts of the bay is consistent with the cause of the decline being primarily eutrophication rather than wasting disease.

Another possible influence on eelgrass abundance is herbivory. Some snails and amphipods can benefit eelgrass by consuming the algae growing on the eelgrass surface. This aids light penetration into the eelgrass leaves and thus their growth. In other circumstances, herbivores consume the eelgrass itself. The latter is important in more southern waters, but its importance in Pleasant Bay is not known.The only example we have found of eelgrass recovery following reduction of nitrogen loading is from Mumford Cove in Connecticut. Mumford Cove is a small (0.5 sq. km) Connecticut estuary. It had the secondary treated effluent from a sewage treatment plant discharging into it from 1945 with gradually increasing discharges until 1987 when the discharge was rerouted into the Thames River. Though the cove had initially been dominated by eelgrass, by 1987 the benthos was dominated by macroalgae and had nitrogen levels of ~0.7 to ~1.2 mg N/l. Within one year after the discharge was moved the macroalgae had decreased by 99% and after 15 years, eelgrass had returned to its pre-discharge condition. The nitrogen levels in the water after the diversion were 0.14 mg N/l (Vaudrey, Kremer, and Branco, 2007).

Another example from a freshwater system, the tidal portion of the Patuxent River, showed recovery of SAV (submerged aquatic vegetation) after reduction in nitrogen inputs. While not eelgrass, the SAV consists of aquatic plants that occupy a similar niche in freshwater ecosystems to that which eelgrass occupies in brackish and marine ecosystems. The Patuxent River is a part of the Chesapeake Bay system where 0.15 mg

N/l has been chosen as the suitable nitrogen loading for ecosystem health. Figure 9 illustrates the response of the SAV to the nitrogen reduction.

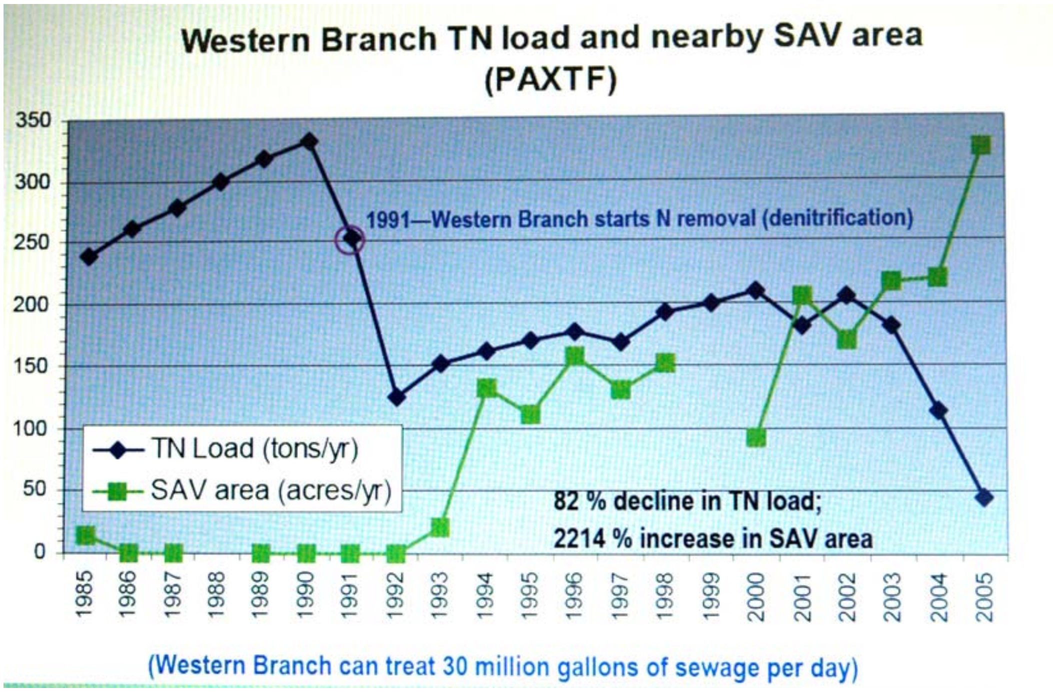


**Figure 9. Response of submerged aquatic vegetation (SAV) in the Patuxent River to reductions in nutrient loading. Not directly comparable to Pleasant Bay due to the freshwater nature of the system, and the large nutrient loadings involved.**

### 3.4 BIOACTIVE NITROGEN

Total nitrogen includes nitrogen in compounds that are slow to degrade and therefore do not release their nitrogen for use by plants (algae). Compounds such as lignin fit this category. In certain instances, it is not suitable to include these compounds when calculating the amount of nitrogen causing eutrophication. Bioactive nitrogen is nitrogen contained in compounds that are readily degraded, and release their nitrogen for uptake by plants. This includes that nitrogen already in inorganic form and usable by plants. When there is a high total nitrogen, it may be beneficial to focus on the bioactive portion in a system where there is flushing that will remove the resistant fraction. MEP did not include particulate organic nitrogen (PON) in their active fraction. If they had, the available nitrogen would have been larger, but it is not certain whether this would make a significant difference in the analysis. Some research suggests organic nitrogen (dissolved and/or particulate) may contribute to eutrophication in estuaries.

### 3.5 BOATING ACTIVITY

The potential influence of boating activity was raised as a topic of interest in the course of conducting the peer review. Stirring up the bottom with props, dragging anchor chains, etc. exacerbates poor conditions if the bottom is muddy and has high organic content (Crawford 2002). If the bottom muds do not have high levels of labile organics, the effects will be less serious. If high nitrogen inputs have increased organic levels, the effects will be greater. It is reasonable to conclude that high nitrogen inputs have contributed to the effects of boats in creating anoxic conditions in Pleasant Bay. Boat effects are highly localized, especially in shallow waters, where the stirred up mud settles fairly rapidly. Even if nitrogen inputs were brought to zero now, it would take years for the effects of boats to be reduced because the labile organics and nitrogen concentration in the mud will decline slowly under natural conditions.

### 3.6 RECOVERY TIME

It is important to recognize that even if all sources of nitrate into local ground waters (septic systems, paved areas runoff, lawn fertilization) were to cease today, it would still be on the order of ten years before the nitrate from these sources was no longer entering the estuarine waters. It would take that long for all of the contaminated ground water to flow into the estuary. But the longer cleanup of nitrogen sources is delayed, the longer it will take for the damaged estuarine areas to recover.

The most rational measure of the return of estuarine health is probably nitrogen level. Using the nitrogen levels in relatively healthy subsystems as TMDL’s is a reasonable approach, although for safety’s sake it would be wise to exercise an appropriate level of caution when setting the compliance limits. A seemingly healthy embayment with a higher nitrogen concentration could potentially be verging on a negative response. The return of eelgrass and recolonization of desirable benthic faunas are both chaotic events (Frederiksen, Krause-Jensen et al. 2004). If conditions are ideal, season, currents, salinity, etc., then it can happen rapidly. If not it can take time. But if the eutrophication has been controlled, then recolonization will happen when conditions for colonization arise.

### 3.7 RESPONSE TO VALIDATION COMMITTEE COMMENTS

We have also tried to address some of the questions in Gregory S Horne’s document from the Wastewater Management Validation and Design Committee of 11 November 2008. Because of variability in ecosystem processes, some of these questions could only be answered by having more measurements over time and having excellent coupled models dealing with phytoplankton production, circulation, and respiration. To the extent possible, some of these questions are addressed below.

## *Chapter VII Response to Comments*

***Reviewer Questions:***

***<u>Bottom Water Dissolved Oxygen</u>***

*Curiously, most stations in the northern portion of Pleasant Bay did not show any apparent correlation between DO and chlorophyll levels, as would have been expected. Moreover, several stations showed a semi-diurnal, perhaps tidal alternation in DO levels. Almost all of the stations showing seriously depleted levels of DO were in the northern sub-embayments or their tributaries. Yet these same stations also showed diurnal or semi-diurnal alternations with high DO levels in bottom water. How is the surface water that is oxygen enriched by phytoplankton photosynthesis displacing the* *oxygen depleted bottom water, or are these DO excursions reflecting tidal turnover of the enclosed sub-embayments?*

- Response: It is common for tidal currents to cause exchanges, replacements, and mixing that result in changes in water properties such as DO.

***Sediment Entrainment and Biochemical Oxygen Demand***

*The MEP report does not consider the impact of boating activity, although all of the data was collected during the peak of the boating season. Boating activity should have a significant impact on water quality, stirring up and entraining into suspension bottom sediment, which will result in an increase of organic matter and nutrients in the water column. This in turn will increase the biochemical oxygen demand (BOD) and biological activity of the water column resulting in DO depletion. How does boating activity (putting sediment into the water column) contribute to the daily oxygen excursion? What is the impact of boat mooring lines/chains and boat movements in and out of the embayments on dissolved oxygen levels1? Put another way, how much of the problem is boating and how much is due to controllable nitrogen loading?*

- Response: This is discussed in Section 3.5.

***Historical Eelgrass Distribution***

*Two paramount questions loom as a result of this historical analysis:*

*1- Disregarding the inaccurate and potentially spurious dataset from 1951, the 1995 and 2001 surveys required sufficient water clarity for early morning photography to accurately image the bottom of Pleasant Bay in order to reliably discriminate visually between barren and inhabited substrates. The obvious rhetorical question then remains: how can it be claimed that the decline in eelgrass distribution is a result of shading the substratum by the increased turbidity of enriched phytoplankton populations in surface waters due to nitrogen loading?*

*2- If the distribution of eelgrass has significantly declined in recent years, what ecological attributes might also affect eelgrass viability in addition to nutrient enrichment?*

- Response: We can never be certain about the details of what happened in the past because we were not monitoring the Bay. Disease may have had an effect. We know that changes in circulation have effects both through sediment movements and changes in flushing that modify N concentrations. We do know for sure, from many studies around the world, that excess N loading affects eelgrass negatively.

***Reviewer Questions***
***Re: Dissolved Oxygen:***
***p. 151, para.3 states:***
*"Dissolved oxygen depletion is frequently the proximate cause of habitat quality decline in coastal embayments (the ultimate being nitrogen loading)."*
*What is the relationship between nitrogen in the water column and "depleted oxygen concentrations"?*

*Chemically and biologically, how does nitrogen cause reduced oxygen concentrations?*

*Can oxygen depletion occur in coastal embayments in the absence of excess nitrogen?*

*What role does the capacity of suspended and substrate sediment to absorb oxygen have on the water column?*

- Response: Nitrogen causes hypoxia/anoxia by stimulating organic productivity (algal growth) in the water. Algae respire (use up oxygen) throughout the day. This is more than offset by photosynthesis, but **only during the day**. At night, algae respire and use up oxygen. When algae die their decomposition by bacteria uses up oxygen. This results in more oxygen consumption than production, thus hypoxia/anoxia. This may occur in the absence of excess N, but only in shallow, poorly flushed water bodies.

***p. 153, para. 1 states:***
*"Nitrogen enrichment of embayment water generally manifests itself in the dissolved oxygen record, both through oxygen depletion and through the magnitude of the daily excursion."*
*How does the chemistry and biology work to affect a reduction in oxygen?*

- Response: This matter is addressed in the response to the previous question.

***p. 154, para. 1 states:***
*"The general pattern is for the high level of oxygen stress (frequent hypoxia or anoxia) in the bottom waters of small enclosed basins (groupA) which tend to have higher nitrogen levels and high rates of sediment metabolism…."*
*How does the nitrogen concentration compare with sediment metabolism; can the latter dominate, even in the presence of elevated concentrations of nitrogen?*

- Response: Sediment metabolism is supported by organic inputs from somewhere, typically from plant production in the waters above the sediments.

***p. 154, para. 2 states:***
*"Salt marsh creeks (that do not empty at low tide) frequently become hypoxic in summer as a result of the high organic matter loading associated with marshes."*
*Is this independent of nitrogen loading, and could not this same effect be attributed to the small enclosed basins (group A)?*

- Response: Salt marsh plants fix nitrogen if it is deficient around their roots. This supports the high productivity of the marshes. The process stops if N is supplied from elsewhere. I do not know of nitrogen fixers that grow on pond bottoms except for blue-green bacteria, but they would also stop N-fixing if sufficient N were present from elsewhere.

***p. 157, Fig. VII-3, and p. 167, Fig. VII-23, Meetinghouse Pond:***
*What is the cause of the large changes in DO between mid-August and late August; and between early September and later in September?*

*Chlorophyll-a does not show a similar variation over the same periods. Why?*

- Response: This results from both changes in algal production and tidal circulation. We don't have enough data to give a definitive answer.

***p. 159, Fig, VII-8, and p. 169, Fig. VII-28, Areys Pond:***
*Fig VII-8 shows large daily oxygen excursions starting from 11 July to end July; however, the Chlorophyll-a levels drop significantly from levels prior to 11 July. What is happening?*
*Would you not expect the oxygen excursions to accompany high Chlorophyll-a levels?*

- Response: These sorts of changes result from algal die-off, changes in decomposition rates, changes in tidal flushing, etc. Without being there or collecting measurements at the time, we cannot give a definitive answer. The changes are not surprising or out of the ordinary.

***p. 159, Fig. VII-7, and p. 169, Fig. VII-27, Pochet:***
*Similar observation as for Areys above except DO excursions seem to change little over the 1 August to 20 September period whereas Chlorophyll-a spikes in mid-August and is relatively flat at just less than 5 μg/l before and after the mid-August period. What is happening?*

- Response: Same response as above.

***<u>Re: Eelgrass:</u>***
***p. 151, para. 3:***
*Eelgrass mapping has been used to indicate the health of the greater Pleasant Bay system. Between the first unvalidated measurements in 1951 and the second in 1995, 44 years elapsed with no measurements of eelgrass area or density. The same is true of observations in Bassing Harbor.*
*How can one conclude that the apparent decline is due only to increases in nitrogen when there is no available data on nitrogen concentrations during this time period?*

*What about the possible effects of disease and or other biotic interactions?*

*In 1983 Pleasant Bay had a boom harvest of more than 70,000 bushels of scallops which are dependent on eelgrass.*
*Would not this indicate that the eelgrass community was healthy in 1983?*

- Response: As indicated in other answers, the significant changes in eelgrass are those in the innermost parts of the Bay system. There is and has been plenty of eelgrass in the better flushed parts of the bay to support scallops.

***p. 182, ¶ 2 states:***
*"However, it is clear from the 1951, 1995 and 2001 temporal sequence that the eelgrass areas in each basin, except Chatham Harbor, are declining in coverage. In The River and Pochet the eelgrass areas were always patchy and in the shallows. By the 2001 survey this pattern continues, but the beds appear to be declining, although they persist."*
*Is it clear? On what objective basis?*

*Do the 1951 photographs provide sufficient resolution to support this conclusion?*
*Or to support the comment specifically regarding the Pochet and The River eelgrass habitats.*

***Paragraph 1, p188*** *dismisses the idea that other factors may have impacted the eelgrass over 50 years and states: "It is not possible at this time to determine the potential effect of shell fishing on eelgrass bed distribution."*

- Response: There is no absolute certainty about the cause of any decrease in the past that was not studied at the time. But we do know that N causes eelgrass declines from studies around the world. We do not have data to comment on the effects of current shellfishing on eel grass beds/bay health.

***p. 188, ¶ 6 states:***
*"It is possible to determine a general idea of short- and long-term rates of change in eelgrass coverage from the mapping data, although there are only 3 surveys. Over the 50 year period1951-2001 the Pleasant Bay System has lost ~583 acres of eelgrass habitat. Interestingly, the rate of loss has been relatively constant at ~11 acres per year. This loss has occurred as watershed nitrogen loading rates gradually increased several fold due to changes in land use within the Pleasant Bay watershed."*
*Considering :*

> *1. the inferior quality and scale of the photographs utilized to estimate eelgrass habitat area in 1951;*
> *2. an unknown margin of error on all three estimates (1951, 1995 and 2001);*
> *3. the report does not consider the impact of shellfishing activity or natural phenomenon such as disease and hydrology; how is it possible to conclude that eelgrass habitat has decreased "at a relatively constant" 11 acres per year?*

*On what data is the statement "nitrogen loading rates gradually increased several fold" based? How much is known about the nitrogen loading (natural and humanmade) in 1951?*

*In fact, the report states that eelgrass is present in all of the areas where it was in 1951. For The River and Pochet, the two areas suspected to have suffered decline in the 50 year period, the report acknowledges that the "eelgrass areas were always patchy" and in the following sentence states that " the beds appear to be declining". The report does not provide objective evidence for this conclusion; nor does the report give meaningful consideration to the possibility that other factors may have played a part in any changes in the eelgrass coverage.*
*Where can one find absolute and definitive evidence bearing on the relationship between dissolved nitrogen concentrations in estuaries and eelgrass viability? Does such exist?*

- Response: Perhaps it is best to ignore the 1951 data since the photos are of questionable validity for this purpose. There is also general uncertainty with interpreting eel grass acreage from aerial photos. Absent extensive historical field measurements, however, aerial photographs provide the available evidence to examine trends. Ignoring, the 1951 photos, we see a 25% decline in the '95 to '00 data for the Bassing Harbor sub-system, for example. The question of other causes is always there, and we have no way to go back to confirm what they could have been. We do know that N concentrations have an effect and could reasonably be the cause of the latter decline. We also know that the central part of the Bay has healthy eelgrass. The declines that matter are those in the upper, more enclosed parts of the system.

## *Chapter VIII Response to Comments*

***Reviewer Questions:***

***p. 200, ¶ 4:*** *Dissolved Oxygen. SMAST describes the high level of oxygen stress in the sub-embayments. "These small enclosed basins tend to have higher nitrogen levels and high rates of sediment metabolism associated with their circulation and focus of watershed nitrogen loads." Consequently, SMAST relates bio-activity stress due to low oxygen to elevated nitrogen levels. It is correct that septic nitrogen in the form of ammonia or urea consumes oxygen in their oxidation to nitrates, but is the oxygen stress totally related to increased septic nitrogen concentrations?*

- Response: Addressed in Section 1.7.2 of the Final Report.

***p. 201, ¶ 1:*** *"Salt marsh creeks (that do not empty at low tide) frequently become hypoxic in summer as a result of high organic matter loading associated with marshes. Even pristine salt marshes can exhibit this behavior." Don't the sub-embayments, such as Meetinghouse and Areys Ponds collect organic matter? Is it possible that the hypoxia in these "A ponds" is caused by similar mechanisms to those in the marshes?*

- Response: Addressed in Section 1.7.2 of the Final Report.

***p.202, ¶ 1:*** *"As for the oxygen and chlorophyll indicators and the distribution of sediment metabolism, the enclosed basins (Group A, above) are generally significantly to severely impaired relative to the benthic infaunal habitat quality." It appears that to accept this premise that the impairment is related to low oxygen and chlorophyll, one* *must accept the fact that septic nitrogen is the primary cause of deplete oxygen. Is it possible that the same mechanisms that occur in marshes occur in the Group A subembayments?*

- Response: Addressed in Section 1.7.2 of the Final Report.

***p. 204, ¶ 1 and 2:***
*"the restoration target should reflect both recent pre-degradation habitat quality and be reasonably achievable." "The threshold nitrogen level for an embayment represents the tidally averaged water column concentration of nitrogen that will support the habitat quality being sought."*

- Response: Addressed in Section 1.7.2 of the Final Report.

***p. 204 , ¶ 5 :***
*"After the sentinel sub-system (or systems) is selected, the nitrogen level associated with high and stable habitat quality typically derived from a lower reach of the same system or an adjacent embayment is used as the nitrogen concentration target."*
*Is this a reasonable approach?*

- Response: It is reasonable since the selected embayments do not show damage. A recent summary of eelgrass in Chesapeake Bay recommends a N threshold of <0.15 mg/l, similar to that recommended by MEP for Pleasant Bay. Data from Mumford Cove suggest a threshold of about the same, discussed in Section 3.3.

***p. 205, ¶ 1:*** *What is the support for the notion that dissolved organic nitrogen is nonreactive in the marine environment? What are the sources of dissolved organic nitrogen?*

- Response: Addressed in Section 1.7.2 of the Final Report.

***p. 205, ¶ 2:*** *The nitrogen threshold of 0.16 mg bioactive nitrogen/liter was set based on a Dec. 2003 MEP Report for Bassing Harbor. What if it were 0.17? Or 0.18? How is the determination made? Note that the data in Chapter VII, Table VII-7, eelgrass areas declined from 246 to 114 acres between 1951 and 2000. Was the concentration of bioactive nitrogen less than 0.16 mg/liter during this 50 year period? Especially from 1951 to the early 1980s when the building boom occurred? Again, is bioactive nitrogen the only real culprit?*

- Response: The first part of the question incorporates the 1951 data, which we believe have substantial uncertainty and may be inaccurate. We do not have 50 years of biogeochemical data for the bay, so can never know whether or not there are other culprits. However, based on what we do know and the response to the comment on p204 ¶ 5 above, 0.14 to 0.16 ml/l seems reasonable as a N threshold.

***p. 205, ¶ 3:*** *"Ryder Cove represents a system capable of fully supporting eelgrass beds and stable high quality habitat based upon the 1951 – 2000 surveys. At present, this basin is transitioning from high to low habitat quality in response to increased nitrogen loading." So… if Bassing Harbor has had high quality water column in terms of bioactive nitrogen until recently, why did the eelgrass population decline between 1951 and 1995? Are there other potential causes of eelgrass decline that are not included in the SMAST assessment?*

- Response: As suggested in the response to Chapter VII comments, it may be best to ignore the 1951 data since there is substantial uncertainty. Then we just see a 25% decline in the '95 to '00 data for the Bassing Harbor sub-system. The question of other causes is always there, and we have no way to go back to see what they could have been. We do know that N concentrations have an effect and could reasonably be the cause of the latter decline.

***p. 206, ¶ 1:***
*"Unfortunately, <u>total nitrogen within this system appears to be very high</u>. In fact, the <u>whole of lower Pleasant Bay appears to contain very high levels of total nitrogen</u>. Analysis of the composition of the watercolumn nitrogen pool within these embayments revealed that the concentrations of dissolved inorganic nitrogen (DIN) and particulate organic nitrogen (PON) were the same as for the Stage Harbor System. In fact, the level of these combined pools (DIN+PON) was lower in Bassing Harbor (0.133 mg N L-1) than in the Stage Harbor (0.158 mg N L-1) and the mouth of Oyster River (0.160 mg N L-1). Note that the mouth of the Oyster River exhibits a documented stable healthy eelgrass habitat (MEP 2003). It appears that the reason for the higher total nitrogen levels in the Pleasant Bay waters results from the accumulation of dissolved organic nitrogen. The bulk of dissolved organic nitrogen (DON) is relatively non-supportive of phytoplankton production in shallow estuaries, although some fraction is actively cycling. It is likely that the high background DON results from the relatively long residence time of Pleasant Bay waters relative to the smaller systems. This allows the accumulation of the less biologically active nitrogen forms, hence the higher background.*
*Decomposition of phytoplankton, macroalgae and eelgrass release DON to estuarine waters as do salt marshes and surface freshwater inflows." (underlines added) The quotation indicates that the <u>very high</u> total nitrogen levels in Pleasant Bay are not expected or well understood. The text "explains" the phenomenon using the phrases "It is likely" and "It appears" throughout. It seems that the explanation is a pure conjecture without any facts to back it up. Since the crux of this matter is about how much nitrogen is in Pleasant Bay, how it moves in and out of the bay and how it impacts the flora and fauna in the bay, it would seem important to have and understand the facts about the nitrogen levels in the bay.*

- Response: Total nitrogen includes nitrogen in compounds that are slow to degrade and therefore do not release their nitrogen for use by plants. Compounds such as lignin fit this category. So it is not suitable to include them when calculating the amount of nitrogen that is causing eutrophication. Bioactive nitrogen is nitrogen contained in compounds that are readily degraded, so release their nitrogen for uptake by plants. This includes nitrogen that is already in inorganic form and usable by plants. So when there is high total nitrogen it is essential to use just the bioactive portion in any system where there is flushing that will remove the resistant fraction. MEP did not include PON in their active fraction. If they had, the available nitrogen would have been larger; I do not know if this would have made a significant difference. We do not know why the TN is so high within the system. We agree it would be good to know "why", but not knowing "why" does not make the number wrong.

***p. 206, last ¶:***
*"moving into the mouth of The River (PBA-13) and the lowermost basin of Pochet (WMO-03) eelgrass coverage appears to have declined since 1951, although eelgrass is still present. This loss of beds indicates that the habitat quality has become impaired, but since eelgrass remains, the impairment is judged to be "moderate."*
*(see p.182, para. 2: "….smaller eelgrass areas in Pochet and fringing shallow areas in The River and Meetinghouse Pond. …..However, it is clear from the 1951, 1995 and 2001 temporal sequence that the eelgrass areas in each basin, except Chatham Harbor, are declining in coverage. In The River and Pochet the eelgrass areas were always patchy and in the shallows. By the 2001 survey this pattern continues, but the beds appear to be declining, although they persist.")*
*Given the inferiority of the 1951 photos and the lack of any field verification, the thesis that eelgrass has been declining from 1951 to 2001 corresponding to an increasing rate of nitrogen introduction to the bay is lacking in credibility. Furthermore, the report does not present convincing evidence that 0.16 mg/L is a critical nitrogen level. Where is the body of scientific research showing the relationship between nitrogen concentration and eelgrass success?*

- Response: Research from other areas suggests that 0.15 mg N/l is an appropriate N concentration. This is about what MEP suggests. References to this are contained throughout the report. The data in the report from Applied Coastal Research and Engineering, Inc. indicate a decrease of 0.015 mg/l between 2004 and 2008 as a result of the breach but these results are from the model. Other recent monitoring data suggest the bio-active N concentration has reduced in recent years, for instance at Namequoit Point. A decrease in N concentration at Namequoit Point would be expected once the breach opened.

***p. 208, ¶ 2:*** *"While these systems [drowned kettle ponds] may not be supportive of eelgrass habitat, they are generally capable of supporting healthy benthic animal habitat. Infaunal animals are sensitive to the organic matter loading and resultant periodic oxygen depletions associated nitrogen overloading. Since these conditions typically occur at higher nitrogen loads than does the shading of the bottom by increased phytoplankton production (principal cause of eelgrass loss), the nitrogen threshold level for healthy benthic animal habitat is higher than for healthy eelgrass habitat." How important, in relative terms, are "organic matter" and the "nitrogen concentrations" in supporting a healthy benthic habitat? SMAST appears to consider the loss of eelgrass to be solely* *attributed to bioactive nitrogen in the water column, and ignores other mechanisms that contribute to eelgrass loss!*

- Response: There is no absolute certainty about the cause of any decrease in the past that was not studied at the time. But Short and others have shown that plants at full salinity are much more susceptible to the wasting disease than those at brackish conditions. Their data only distinguish salinities above and below 15ppt so disease could potentially affect most of Pleasant Bay.

***p. 208, last ¶:***
*After describing successful amphipod communities in the Orleans ponds where the bioactive nitrogen concentration varies apparently varies from 0.2 to 0.4 mg/l, the report concludes that 0.21mg/l should be established as the threshold concentration for benthic infauna. Why 0.21? Why not 0.28 or .030?*

- Response: The website "*www.mass.gov/dfwele/dmf/programsandprojects/hubline/***eelgrass***_update_021408.pdf"* gives results from eelgrass restoration in Boston harbor after the sewage cleanup but there are no N data. The only complete answer we have is discussed in Section 3.3.

***p. 209, final 2 sentences:***
*"Therefore restoration success will be gauged by reaching the target at the sentinel station and at the secondary stations for eelgrass (Ryders Cove) and infauna. Overall there are three primary(PBA-12, PBA-03 and CM-13.) and 8 secondary target stations within this System, the largest embayment on Cape Cod."*
*This states that both the sentinel station and the secondary station must meet targets. The targets are shown in Table VIII-2, which contains both Bioactive Nitrogen thresholds and Total Nitrogen Thresholds for all 8 secondary stations, 6 of which are in Orleans. This seems inconsistent with the statement on p. 204 i.e.,*
*"……to first identify a sentinel station within the embayment …….is selected such the restoration of the one site will necessarily bring the other regions of the system to acceptable habitat quality levels."*

*These multiplicity of requirements and seemingly conflicting statements need to be resolved.*

- Response: Because the Bay is large and has many subsystems, they feel that several sentinel stations in different parts of the Bay are needed. The statement on P. 204 probably refers to each subsystem, not to the Bay as a whole.

## REFERENCES SECTION THREE

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# APPENDIX A BIO SKETCHES

**Kirk F. Bosma, P.E., M.C.E**

**Team Leader/Coastal Engineer**

**Project Role: Task 2 Hydrodynamic Modeling Technical Lead/Project Manager**

Mr. Bosma is a Coastal Engineer at Woods Hole Group (since 1997) with specialization in the areas of numerical modeling, sediment transport, littoral processes, and beach profile evolution and analysis. He received his M.C.E. in the field of Coastal Engineering from the University of Delaware in 1996 and his B.S. in the field of Civil Engineering from Calvin College in 1994. Mr. Bosma currently serves as the Team Leader of the Coastal Sciences, Engineering, and Planning Division and has managed projects and engineered solutions related to beach nourishment, beach management, coastal structures, inlet stabilization, water quality, environmental permitting, and wave, tide, and current data collection. He is an active member of the American Society of Civil Engineers, American Shore & Beach Preservation Association, and the Association of Coastal Engineers.

Mr. Bosma also has extensive experience developing and employing numerical models for sediment transport, nearshore spectral wave transformation, particle transport, bathymetric evolution, and two- and three-dimensional hydrodynamic processes, and then analyzing the results. In addition, he has implemented technically advanced data analysis techniques to assess the coastal and oceanographic environment, including wave, tide, current, sediment and particle transport processes. As such, developing both data collection and numerical modeling programs, Mr. Bosma regularly uses the results of each to help guide coastal engineering design (e.g. beach nourishment, inlets, and coastal structures), determine impacts of dredging, define estuarine processes, develop marsh restoration plans, and complete sediment fate and transport studies. He has extensive experience in utilization of Unix- and PC-based software packages and programming languages to present, analyze, and solve engineering and scientific problems.

Recent types of coastal projects Mr. Bosma has managed include: beach nourishment plans, wave, current, and sediment transport modeling, beach nourishment design, marsh restoration projects, coastal engineering design, hydrodynamic mixing and transport studies, and borrow site impact assessment. Mr. Bosma is currently the project manager for three of the largest coastal restoration projects in the northeast, that include evaluation of a variety of coastal protection alternatives, implementation of a comprehensive data collection program, and hydrodynamic, hydraulic, wave and sediment transport modeling.

**Heidi J. Clark, M.S., M.F.S., Ph.D.**

**Environmental Scientist**

**Project Role: Overall Literature Reviews/Technical Support**

Ms. Clark is an environmental scientist with extensive experience in coastal ecology, environmental damage assessment, and habitat restoration. She has ten years experience in environmental science and consulting, with projects ranging from environmental damage assessment to seagrass restoration to dune revegetation. Ms. Clark remains active in coastal scientific research as well. She is currently working on a long-term study of the effects of shellfish aquaculture on water quality as part of a multi-investigator project at the Woods Hole Oceanographic Institution.

Prior work in landscape construction affords Ms. Clark an understanding of the many factors involved in habitat restoration and coastal construction projects. Her continued work in research and consulting has allowed Ms. Clark to develop research, writing, and project management skills. She has worked on both individual and multi-investigator projects, and has a proven ability to work effectively with personnel from government agencies, research scientists, individuals from civil society, and consultants. Ms. Clark has MFS and Ph.D. degrees from Yale University.

**Jeffrey C. Cornwell, Ph.D.**

**Biogeochemist/Research Professor**

**Project Role: Task 1 Benthic Flux Technical Lead/Project Manager**

Jeffrey Cornwell's university laboratory at the University of Maryland Horn Point Laboratory has carried out a large number of sediment biogeochemical projects in lakes/reservoirs, marshes and estuaries since the late 1980's. Techniques have included 1) utilization of state of the art core incubation techniques for sediment-water solute and gas exchange, 2) measurements of denitrification using membrane inlet mass spectrometric analysis of gas ratios, 3) estimation of sedimentation and nutrient burial using radionuclide-based sediment dating, and 4) characterization of dredge materials for utilization for marsh creation.

Cornwell has also served on the Boston Harbor/Massachusetts Bay Model Evaluation Group, serving as a benthic biogeochemical expert. He has also participated in the evaluation of the coupled physical/biogeochemical model for Florida Bay (for the South Florida Water Management District). More recently, he has been the local organizer and a member of the steering committee for a workshop on the measurement of denitrification (see denitrification.org).

Dr. Cornwell is a member of the American Chemical Society; American Geophysical Union; American Society for Limnology and Oceanography; Estuarine Research Federation; Geochemical Society; Sigma Xi; and the Society of Wetland Scientists.

**Nathan Dill, M.S.C.E.**

**Coastal Engineer/Modeler**

**Project Role: Technical Support for Task 2 Hydrodynamic Modeling**

Mr. Dill received a Master of Science in Civil Engineering from Louisiana State University in 2007, and Bachelor of Arts with a major in Physics from Bowdoin College in 2002. His academic studies have given him a strong background in physical sciences with particular focus on free surface flow, water wave mechanics, sediment transport, coastal morphology, numerical modeling, and high performance computing. He is an expert with a broad suite of numerical models, and has extensive experience working directly with the computer code that supports these models. Previous experience as a high school Physics teacher has helped him hone his communication skills and ability to present abstract physical ideas in an easily understandable way. He also has prior service with a large A&E Corporate (URS), which gives him a broad base of experience. Professionally, he has gained experience in developing and applying numerical models in both PC and high performance computing environments for coastal restoration projects and also hurricane storm surge related problems. Additionally, he is skilled at applying standard software tools and developing efficient project specific tools to support data analysis in support of modeling projects.

### Robert Hamilton, Jr., M.C.E, Vice President

### Senior Civil/Coastal Engineer

### Project Role: Project Manager

Mr. Hamilton is a Civil/Coastal Engineer and Vice President for Business Development at the Woods Hole Group. He's been with the Woods Hole Group since 1994, and has previously served as Coastal Engineer, Business Unit Director, and V.P. for Scientific Operations. He is also a Director for the Northeast Shore & Beach Preservation Association as well as the Marine & Ocean Technology Network. He earned a B.S. in Civil Engineering from Lehigh University, and a M.S. from the University of Delaware Center for Applied Coastal Research. He is focused on development of business relationships and multi-disciplinary project and client management. His extensive market and contracting experience includes government agencies, architectural/engineering partners, offshore oil and gas producers, private owners/developers, power utilities, and manufacturing industries. He has strong technical, analytical, and problem-solving skills combined with an effective leadership, communication, negotiation, and personnel management approach.

His technical expertise is on solving problems related to shoreline erosion, coastal structures, water quality, environmental permitting, and the transport and dilution of thermal discharges and contaminants released into the marine environment. He also has multi-jurisdictional regulatory experience, including preparing EIS documents under NEPA, and has served as an expert witness. Mr. Hamilton's technical skills include numerical modeling and analyses of nearshore wave refraction, diffraction and breaking, sediment transport and shoreline change, and two- and three-dimensional hydrodynamic processes, including plume dispersion and mixing zones. His field skills include site assessments, wave and tide data collection, bathymetry data collection, beach profile surveying, and scientific SCUBA diving. Recent types of coastal projects in which Mr. Hamilton has participated are related to beach nourishment, salt marsh habitat restoration, dredging and dredged material disposal, and environmental resource impact assessment and management planning (including benthic and submerged aquatic vegetation). Mr. Hamilton will be the overall Project Manager, which is compatible with his current role as Project Manager for a DEP contract intended to optimize and develop a protocol for the MEP linked-model approach.

**Michael S. Owens, B.S. (M.S. candidate)**

**Biogeochemist**

**Project Role: Technical support for Task 1 Benthic Flux**

Michael Owens has 20 years of sediment biogeochemical experience and has been directly responsible for carrying out all of the denitrification measurements at Horn Point Laboratory working with Dr. Cornwell. He also has extensive experience in pore water and solid phase analyses. His M.S. will be completed in 2009; his work will show the vital importance of relatively small benthic animals on sediment processes in Chesapeake Bay mesohaline sediments.

**Nadine A. Sweeney**

**Administrative Manager/Senior Publications Specialist**

**Project Role: Project Administration**

Ms. Sweeney has been with Woods Hole Group since 1993, and has a complete understanding of the corporate management systems and procedures. She is also primarily responsible for the publication of high-quality deliverables, including layout, formatting, and production. She has a wide range of publication software skills, including expertise with MS Word, Excel, and PowerPoint, as well as graphics software experience with Adobe Photoshop and Illustrator. For operations management support, Ms. Sweeney is a MS Project specialist, including applications for individual project scheduling and planning, as well as program roll-ups for resource tracking across multiple projects, clients, and departments within Woods Hole Group. She also helps define and track work breakdown structures in support of Project Managers for individual projects. In support of financial management, Ms. Sweeney acts as the Project Manager's liaison with the Woods Hole Group accounting and finance department. Ms. Sweeney creates project and client files within the Wind-2 corporate financial management system, including entry and tracking of budgets for individual tasks and subtasks. With her experience and knowledge of management processes, Ms. Sweeney also has the intangible ability to effectively encourage Project Managers in their duties. Specifically, she helps Project Managers review and produce monthly (at minimum) project budget update reports, client invoices, and progress reports as required.

**John M. Teal, Ph.D., P.W.S.**

**Ecologist**

**Project Role: Task 3 Eel Grass and Benthic Community Technical Lead/PM**

Dr. John M. Teal is an ecologist who has spent most of his professional career at Woods Hole Oceanographic Institution. His areas of expertise are broad and include:

- wetland and coastal ecology
- submerged aquatic vegetation and benthic habitats
- salt and brackish marsh ecosystem structure and function
- fish nursery value, nutrient cycling, hydrology, productivity, eutrophication
- marsh restoration
- pollution effects and environmental risk

- groundwater influences on water bodies
- ground water contamination with nutrients
- wastewater treatment by natural and artificial wetlands
- petroleum pollution and hydrocarbon biogeochemistry
- nutrient dynamics
- marine birds and over-ocean migration of land birds
- coastal marine ecology including dune and beach ecology
- physiological ecology of fishes
- aquaculture and fisheries

He has served as a qualified expert witness in federal and state courts. He is the author of co-author of over 140 scientific publications. His 1969 book -- Life and Death of a Salt Marsh (Boston: Little Brown) -- has introduced the general public to the mysteries and importance of these fragile ecosystems.

Since 1993, Teal has been the principal wetlands consultant for the 20,000 acre salt and brackish marsh restoration and preservation project being conducted by Public Service Electric & Gas of New Jersey on the shores of Delaware Bay. This project involves the restoration of marshes degraded by both *Phragmites* invasion and/or by diking (restriction of circulation and exchange with the bay). His involvement began in the initial permitting stages, and has continued through the restoration planning, construction, and follow-up through an adaptive management program.

He has been a consultant to the Wetland Restoration and Banking Program in Massachusetts and has consulted with numerous groups on salt marsh restoration including the Massachusetts Water Resources Authority and the US Army Corps of Engineers. He has consulted on marsh restoration projects in North Carolina, Louisiana, and California.

Teal is a Certified Wetland Professional, past president of the Society of Wetland Scientists and has received the 1999 National Wetlands Award for Science Research (co-sponsored by the Environmental Law Institute, U.S.EPA, U.S. Fish & Wildlife Service, and National Marine Fisheries Service) and the 1999 Odum Award from The Estuarine Research Federation. He also has served on multiple National Research Council expert committees.

### John H. Trowbridge, Ph.D.

### Senior Scientist

### Project Role: Comparative Analysis for Task 2 Hydrodynamic Modeling

Dr. Trowbridge has more than 22 years of academic and consulting experience in hydrodynamics, sediment transport, coastal processes, and oceanographic measurement programs. Dr. Trowbridge received his Doctorate in Oceanographic Engineering from the Massachusetts Institute of Technology (MIT) and Woods Hole Oceanographic Institution (WHOI) in 1983, his Masters of Science in Civil Engineering from MIT in 1979, and his Bachelor of Science in Civil Engineering from University of Washington in

1977. He currently is a part-time employee at Woods Hole Group, in addition to a full-time Senior Scientist at the Woods Hole Oceanographic Institution.

Dr. Trowbridge has been actively involved in the quality control and technical oversight facets of numerous Woods Hole Group projects. He has designed oceanographic measurement programs for the evaluations and improvement of coastal waves, currents, and sediment transport. He has developed simplified conceptual and analytical approaches to provide preliminary estimates, facilitate feasibility studies, design complex measurement programs, analysis methods for extraction of robust statistics from measurements. He has developed a code for numerical simulation of wave-driven currents and alongshore sediment transport on beaches.

Dr. Trowbridge has vast experience in the scientific background used to develop and simulate aquatic systems. Additionally, he plays a significant role in model design, development, and model implementations. His expertise in design and interpretation of field measurements and interpretation of field measurements is an essential element for understanding coastal systems and evaluating and improving models.

**Lee L. Weishar, Ph.D., P.W.S.**

**Senior Scientist/Coastal Engineer**

**Project Role: Corporate QA/QC**

Dr. Weishar has more than 25 years experience in the fields of oceanography, coastal engineering, sediment transport, and nearshore processes. He serves on many professional societies, such as the Society of Wetland Scientists and Estuarine Research Federation, and is currently the Chair for the ASCE Wetlands Engineering Guidelines Subcommittee. For the past 15 years he has specialized in coastal engineering and wetland/marsh restoration. Dr. Weishar specializes in the integration of biological, ecological, and hydraulic data to develop wetland restoration designs and to ensure that the design will meet the restoration objectives. Additionally, Dr. Weishar specializes in evaluating the potential impacts of proposed restoration projects on existing wetlands and adjacent transitional, buffer, and upland areas.

Dr. Weishar's current work involves the restoration of both large and small scale salt marshes. He has been involved at the design, permitting and construction phases of the project for more than a decade. During the preliminary design phases of large scale projects, Dr. Weishar spearheads the preliminary hydraulic design and hydrodynamic analyses that proves to the client that a large scale restoration was feasible. Dr. Weishar has worked to perform critical examinations of the marsh restoration performance through frequent onsite visits and analytical analyses. Dr. Weishar helped pioneer the applications of Ecological Engineering and Adaptive Management in the field of marsh restoration.

Dr. Weishar provided the initial preliminary diagnostic hydraulic modeling and resource area evaluations to determine the feasibility of the large scale (10,000 acre) restoration program in the Delaware Bay. These investigations examined the potential effects of the restoration project on ground water, septic systems, and private drinking wells. During this project Dr. Weishar presented the restoration design to the public during frequent meetings with the stakeholders. He also worked closely with local boards to answer their concerns about the large scale wetlands restoration project. The successful implementation of this project required close interaction with the New Jersey state environmental agencies, New Jersey Attorneys Generals Office, National Fish and Wildlife, US Army Corps of Engineers, National Marine Fisheries, and concerned citizens. He also has recently supported wetland restoration projects in Scarborough, ME, South Cape Beach, MA, Nonquit, MA, and others. He is an active publisher of conference papers and proceedings, as well as peer reviewed journals.

## APPENDIX B RESPONSE TO COMMENTS ON THE DRAFT REPORT

**FOLLOW-UP QUESTIONS AND COMMENTS**
**CONCERNING THE WOODS HOLE GROUP DRAFT REPORT**[2]
**<u>Submitted to the WHG by the WMV&DC</u>**
**May 14, 2009**

### EXECUTIVE SUMMARY

- **At p. ES-3 . . .**

  *Action is required and the design process is underway…….which may take two years or so"*

  This statement is incorrect. Design work for Phase 1 (not Pleasant Bay) is budgeted in the Capital Plan for FY11. Design work for the Pleasant Bay watershed is planned around Meetinghouse Pond for 2014 and the Pleasant Bay Area for 2023-2026. <u>Please avoid statements which have to do with Orleans project timing and focus on WHG scientific findings.</u>

  **<u>Response</u>**: The language has been corrected in the Final Report.

- **At p. ES-6 . . .**

  *Although there are biases in both directions, the biases likely produce a conservative representation of the system.*

  The committee sees no evidence to support this statement of conservatism. The large errors stated by Dr. Cornwell and Mr. Bosma suggest that the N input to the system is lower that was modeled. We would appreciate an explanation from you and Mr. Bosma concerning how this affects the resulting TMDL's.

  **<u>Response</u>**: The term "conservative" in this context was intended to indicate that the modeled nitrogen inputs may exceed actual natural conditions. The language was modified in the Final Report to add clarity.

- **At p. ES-6 . . .**

  *Sewering is the primary solution for Pleasant Bay, which is a costly course of action.*

Remediation of most of Pleasant Bay will occur in the last phases of the overall CWMP (2015-2026). Sewers may not be the most cost effective approach in that time frame. WHG has not been asked to comment on the Orleans Plan.

[2] Underlining denotes questions and requests for clarification, additional information, or specific follow-up actions.

**Response**: The language in the Final Report has been modified to recognize sewering as one contemplated future solution, which is subject to the ongoing wastewater facilities planning process.

- **At p. ES-7. . .**

  *In this regard, although there are certain areas where the work could have been approached or presented differently, many of the findings of this peer review should not be interpreted as flaws in the MEP or SMAST work.*

This statement appears to be inconsistent with many potential problems or shortcomings that were pointed out by WHG in the body of the WHG report.

**Response**: This statement is offered to emphasize that the SMAST work was conducted in accordance with a standard MEP protocol applied for stressed estuaries state-wide. Although the peer review team believes there are technical limitations of this approach for Pleasant Bay, there was no evidence discovered through the peer review that the work conducted by SMAST for Pleasant Bay was flawed or erroneous. As with all findings in the peer review, this statement is subject to the limitations associated with not having access to the models or data. Access to the data and models would permit a more rigorous review. No significant changes were made to this text in the Final Report.

- **At p. ES-9 . . .**

  *The models applied to the Pleasant Bay system are the same models applied for other MEP sites.*

This statement is correct. <u>However, we would appreciate an explanation of why Bioactive N was only used in Pleasant Bay when Total N was used in all other 32 Massachusetts estuaries.</u>

**Response**: A statement was added to the Final Report to clarify this significant difference for the SMAST work in Pleasant Bay as compared to other MEP sites. The issue of bioactive nitrogen is also emphasized elsewhere within the peer review. A full explanation for why SMAST elected to use bioactive nitrogen for Pleasant Bay would best be obtained from SMAST directly.

- **At p. ES-12 . . .**

  *The peer review generally supports the findings of MEP with regard to eelgrass and benthic communities, and adds confidence to the notion that benthic communities can recolonize and eelgrass can recover when nutrient loadings are reduced.*

This statement is inconsistent with the presentation and discussion of the 7 May meeting. The following are illustrations of the inconsistency.

On May 7, Dr. Teal said that he could find no comparable site to Pleasant Bay where a 0.16 Mg/L resulted in remediation of eelgrass and benthic infauna.

Dr. Teal also recommended ignoring Pleasant Bay eelgrass coverage data from 1951-1995 because it was unreliable. In the discussion, he said the 1995-2001 data was good to within 15%. It should be pointed out in the final report that it is mathematically impossible to conclude that the eelgrass has declined 5% in that time frame.

**Response**: The text has been modified in the Final Report to state that existing data for Pleasant Bay and for similar systems are limited.

- **At p. ES-13 . . .**

  *As discussed above, action is ongoing and required by the Town of Orleans to comply with federal mandates for TMDLs. This peer review should, in no way, imply that progress should be slowed, or that more study is required to proceed. In fact, the peer review team would encourage the Town to move forward strongly. The MEP work provides a valid basis to proceed, and there should be confidence in the overall solution path. In fact, the response of the system to reduce nutrient loading might be better than anticipated. Since it will take time to realize the benefits, delays will only postpone the desired improvements.*

Statements concerning Orleans scheduling of activities are outside of the scope of the work charged to WHG. As pointed out previously, the design phase will not begin until FY11 and will extend out to FY2026. Further, the statement that the MEP provides a valid basis to proceed appears to be inconsistent with WHG findings of issues that we believe need to be addressed before we build a system for Pleasant Bay. There may be more time to follow up on activities that reduce the uncertainty and risk.

**Response**: The intent of this paragraph is to emphasize that, although the peer review uncovered sources of uncertainty and makes recommendations for resolving this uncertainty in Pleasant Bay, the peer review recommendations are not intended to slow the wastewater facilities planning process. We agree there is time for follow-up activities as part of the planning and design process to reduce overall uncertainty and optimize the design. Many of the recommendations in the peer review are intended to advance this objective. By stating that the MEP work provides a valid basis to proceed was not intended to imply that the full long-term design can proceed based solely upon the MEP findings. This would be inconsistent with the peer review findings, and the text has been modified in the Final Report to clarify this position.

**At p. ES-14 . . .**

*Regardless of whether any additional work is completed to supplement the MEP Report, the town should be prepared for an incremental adaptive management approach for complying with TMDL requirements.*

Recommendations concerning how the Town of Orleans should respond to DEP requirements were outside the scope of work for the WHG consultancy.

**Response**: This language was offered in response to the portion of the Request for Proposals (RFP) that stated,

*"Based on engineering studies, the Town of Orleans has adopted a draft comprehensive wastewater management plan (CWMP) and has begun to plan a sewer system to comply with the TMDLs. Before committing to such a large project, it is important that the Town of Orleans: conduct a peer review of the SMAST work to validate that the calculated reductions in nitrogen discharged into Pleasant Bay are required to significantly improve water quality and habitat viability."*

In response to this aspect of the RFP, the review team was compelled to communicate how we envision the results of the peer review and recommendations can be used in the context of the overall CWMP process. Although we feel the SMAST work provides a sound basis for planning purposes, it does not provide all the information necessary to design a long-term wastewater management plan, nor is the extent of required solutions clear at this time. We have identified potential uncertainties and bias in the work since the standard MEP approach is not sufficient for some of the Pleasant Bay complexities. To help better understand and reduce uncertainties, we've recommended certain next steps that we encourage the Town to pursue to supplement the information in the MEP report. We do not offer these recommendations with any intent to slow the CWMP process. Rather, we feel the recommendations can be pursued in parallel with the Town's ongoing wastewater facilities planning and design process.

We also emphasize there is inherent uncertainty with this type of work, and strongly encourage the Town to pursue an incremental plan or phased approach for complying with the federally-mandated TMDLs. We suggest the plan should also incorporate a long-term adaptive management approach flexible enough to allow later phases of the wastewater management plan to be adjusted based upon the response of the estuary to earlier phases. This will require a long-term commitment to monitoring, as well as long-term flexibility in the implementation strategy since it will likely take a decade or more for the benefits of the first phases to be realized in the estuary. This may involve longer-term Town planning and financing strategies than for typical public works projects; however, such strategies are beyond the scope of this contract or the peer review team's expertise. We offer this perspective based strictly upon our technical expertise, knowledge of the natural processes at work, and inherent uncertainty, which may affect Town policy. We agree that comments on Town interaction with DEP is outside the scope of this contract, and this language has been removed from the Final Report. Other changes were made to clarify this section of the Final Report.

**SECTION ONE**

**<u>Response</u>**: Written comments on the Draft Report received from the WMV&D Committee after the May 7 public meeting are summarized below. A response to these comments is offered in Section 1.8 of the Final Report.

*Lack of benthic flux measurement in light [effect of microalgae] is a key finding. The impact on the "true benthic flux," also referred to as the solute flux, is masked by the addition of a term to represent a modeled PON input to the sediment. Thus, Benthic* $Flux_{SMAST}$ *= Solute Flux (N output) – PON (N input)*

*Therefore, considering all the components in terms of micromoles/ square meter/hour, if Benthic* $Flux_{SMAST}$ *is stated in the MEP report as 200 and assuming the PON component is 20% (i.e. 40) then the SMAST measured solute flux would have been 240. WHG has stated that the solute flux is overstated by ¼ to 1/3 (25 to 33%). This means that the actual solute flux, considering the effect of microalgae, should be perhaps 0.7 times the SMAST solute flux or 168. Returning to the equation above, this means that the SMAST benthic flux would be 168 – 40 = 128 or 36% <u>less</u> than used in the MEP modeling.*

*We request that WHG confirm the above and provide, in the final report, an explanation with an equation showing how the difference between the true benthic flux and the SMAST benthic flux term relate to PON. Additionally, please provide an opinion as to what input should be used in the MEP model: Benthic* $Flux_{SMAST}$ *(including the PON component) or true Benthic Flux (solute flux). How and where should the PON settling component be input to the model?*

*How is the impact of microalgae on solute flux (true benthic flux) expected to vary between the rivers, small embayments (drowned kettle ponds) and the main bay areas? What factors impact the variation?*

*During the meeting, the process of denitrification (production of dinitrogen (nitrogen gas)) in sediment was discussed with reference to the diagram on page 4 of the draft report. The process of denitrification (conversion of nitrate to nitrogen gas) is a reduction reaction. We understood in the May 7 meeting that under the right conditions denitrification could be a significant means of removing nitrogen from the bay system. <u>Please explain in the final report the water column/sediment conditions that are needed to promote denitrification.</u>*

*Page 7, next to last paragraph, "Characterization of high N effluxes (>100 micromole/ m2/hour) as a new loading is inappropriate." <u>Please explain this in the final report.</u>*

*During the May 7 meeting, WHG clearly stated that total nitrogen should be balanced in the model including mass balances for bioactive nitrogen and PON. Another way to say* *this is that all nitrogen needs to be accounted for: the nitrogen going into the system and the nitrogen leaving the system. WHG identified some significant uncertainties in the nitrogen inputs (background concentration, benthic flux) and also some significant uncertainties in nitrogen leaving the system (burial in sediment [portion that becomes unavailable to the water column] and dinitrogen gas generated by sediment denitrification).*

*Please explain how significant the failure to account for burial and denitrification may be (quantification) and what impact including these losses would be expected to have to the modeled system.*

*How does the failure of SMAST to assess PON (other than by estimates) impact the nitrogen mass balance? How does it impact the model reliability?*

*WHG described the multiplicative effect of nitrogen input reduction as perhaps 10-20% (i.e. reducing input by a 1 kg/day is expected to reduce benthic flux by 1.1 to 1.2 kg/day). In addition, it was stated that benthic flux is a response to other inputs; cycling rate is dependent on inputs.*

*Please confirm the quantification of the multiplicative effect in the final report.*

*WHG stated that organic material (e.g. leaf detritus) is an insignificant nitrogen contributor but may be a significant BOD load and thus result in reduced dissolved oxygen.*

*Please include a statement on this in the final report.*

*Please copy the committee and George Meservey on WHG letter (if the question is sent by letter) requesting SMAST clarification on bioactive nitrogen vs. total nitrogen as a modeled parameter.*

*Dr. Cornwell indicated in our discussions at the draft review meeting that the critical step in benthic flux measurements is oxygen potential at the sediment-water column interface. He further indicated that in the absence of oxygen, denitrification, or the formation of dinitrogen, ($N_2$ gas),* **does not occur***. It would be very useful if Dr. Cornwell could provide some general conditions, if possible, in which a significant fraction of the nitrogen in the water column could be converted to dinitrogen gas that would be discharge to the atmosphere, rather than to the Atlantic Ocean eventually.*

*Is there any way Dr. Cornwell could semi-quantify the oxygen conditions at the interface of the water column and the sediments that would result in maximum generation of nitrogen gas?*

*Dr. Cornwell discussed the expectation of different benthic flux values at the sediment interface depending on water column depth. It appears that a single benthic flux value was used in the SMAST "linked computer model" for each terminal pond. But is it*

*possible that there is a large gradient in benthic flux values from the periphery to the center of a pond? Will such a variation make a significant difference in the computations? Should more extensive benthic flux measurements be made for at least one Pond in Orleans to check the data?*
*On page 3 of the draft report, Dr. Cornwell referred to the "Redfield ratio" of about 16 as the ratio of nitrogen to phosphorous. In Pah Wah Pond (PBA11) the ratio is about 57. Can this information on the nitrogen/phosphorous ratio be used to calculate the reduction of nitrogen required? Can a comment be included in the final Report?*

*Will WHG explain the need or benefit of measuring benthic fluxes during winter months and with lighting as opposed to completely dark experiments?*

*It appears that the Pleasant Bay Report ignores nitrogen input from surface vegetation runoff sources, such as leaves and pine needles, and the collection of dead seagrasses, etc., in the terminal ponds. Also, there are large accumulations of organic "muck" at the bottom of the terminal ponds. Can the contribution from these sources be semi-quantified from documented sources?*

**SECTION TWO**

*First order risks (≥ 20%) identified by WHG and impacting the model operation include:*

- *Benthic flux input (Water Quality Model)*
- *Atlantic Ocean background nitrogen concentration (Water Quality Model)*
- *Stratification of drowned kettle ponds (Hydrodynamic Model)*
- *Dispersion coefficients (Water Quality Model)*

*Please provide a simple explanation of what steps would be required to address these issues and resolve associated uncertainties by using corrected parameters in the existing model.*

- **Response**: The existing conditions hydrodynamic and water quality model, or at least the existing conditions data collected as part of the MEP, would be required to directly resolve the uncertainties associated with most of the "first order risks" identified in this comment. Without at least the MEP data, assessment of these risks could only be on a qualitative level. Specifically, the steps needed to quantify the level of uncertainty for each risk are briefly described below.
    a. The evaluation of benthic flux input, and the possible overestimation of the benthic flux input, is discussed in detail as part of Task 1. As discussed in Task 1, the first step to reduce the potential uncertainty associated with the benthic flux input would be to conduct illuminated laboratory testing in order to determine the relative sensitivity of the benthic flux values. If after completing additional laboratory testing, the uncertainty remains or the benthic flux values prove to be overestimated, then the water quality model (RMA-4) could be used to assess the impact of the updated benthic flux values on the model results through adjusting the benthic flux values input in the model.
    b. As discussed in the peer review, the background nitrogen concentration used in the Pleasant Bay modeling was based on a single year of data (2005). Additional data collected in the two years (2006 and 2007) following the MEP modeling effort indicated that the background nitrogen concentration observed in 2005 may have been larger than average. The water quality model (RMA-4) could be used to re-simulate the Pleasant Bay system using an average value of all the observed nitrogen values at the Atlantic Ocean sampling station.
    c. Currently, the possible stratification of the drowned kettle ponds within the Orleans portion of Pleasant Bay is based on limited data taken within Areys Pond. Therefore, in order to verify the potential stratification of the drowned kettle ponds, and their relative importance on the overall nitrogen concentrations, temperature and salinity data should be collected as a function of depth in 2 or more of the kettle ponds. This data collection would consist of simple Conductivity-Temperature-Depth (CTD) casts throughout a summer season(s). If the collected data revealed a consistent stratification, as in the Horne and Horne (2001) data set, then the existing

hydrodynamic and water quality model could be extended to three dimensions in these specific subembayments. These two steps would address the risk associated with the potential stratification in the kettle ponds.

d. The relative uncertainty of the dispersion coefficients relates directly to the sensitivity of this dispersion parameter in the water quality model (Howes et al., (2001). Since the calibrated, existing conditions model must reasonably represent the observed nitrogen concentration data, or at least the range of averaged data (as shown in Figure VI-3 of the MEP Pleasant Bay report), it may be reasonable to adjust the dispersion coefficients within the water quality model (RMA-4) to match the range of the observed data to provide upper and lower bounds on the nitrogen concentration results predicted by the water quality model.

*As understood from the May 7 discussion, such steps would result in changing the dispersion coefficients until the model inputs match the observed nitrogen concentrations. <u>As the dispersion coefficients actually used by SMAST per the 2006 report depart significantly (perhaps by as much as a factor of 10) from the RMA-4 user manual guidance and it is known that the nitrogen concentrations are very sensitive to the dispersion coefficients, would such steps result in a further departure from the RMA-4 guidance?</u> [Note: The purpose of this question is to explore whether or not there might be a simple (relatively easily implemented and low cost) method to respond to the risks listed above (a method which would be less radical than using a different model, i.e. one which would significantly reduce the overall risk and still stay within the current MEP linked-model).]*

- **<u>Response</u>**: If inputs to the water quality model (e.g., benthic flux values, background nitrogen concentrations) are modified for existing conditions of Pleasant Bay, then the water quality model would require recalibration and revalidation. Ultimately, the existing conditions water quality model would need to reasonably match the observed nitrogen samples, and therefore, any changes made to the input conditions would require that the water quality model is recalibrated to ensure that the model correctly represents the observed nitrogen data. For example, lowering the benthic flux values (i.e., less nitrogen supplied to the system by the benthic flux), would require a modification to another parameter within the water quality model to ensure model results match the observed values of nitrogen. Assuming that all the other input values remain the same (and only the overall benthic flux input is changed), this would require a modification to the dispersion coefficients. For the case of lowering the benthic flux input, the dispersion coefficients would need to be lowered (less mixing) so that the modeled nitrogen concentration (which would be lower due to the reduced benthic input) still matches the observed nitrogen data.

  For the risks a through c identified above, the recalibration would result in lowering of the dispersion coefficients in all cases. Therefore, the dispersion coefficients would be closer to those typically presented in the RMA-4 literature and for 2-D and 3-D numerical model applications.

It is important to remember that the water quality model needs to achieve agreement between the model results and measured data, and therefore, the existing modeled nitrogen concentration results within Pleasant Bay will not appreciably change when input values are changed. However, the modification of inputs and/or dispersion coefficients may have significant impact on the potential scenarios (build-out, no anthropogenic loading, sewer scenarios, etc.) being simulated. For example, if the dispersion coefficients are changed due to a modification of a nitrogen input value, then those new dispersion coefficients would result in different nitrogen concentration results for an alternative scenario.

Although the RMA-4 model does have certain limitations in modeling water quality constituents such as nitrogen, we are not suggesting that a different model need to be applied to resolve the uncertainties listed above.

*What does the use of dispersion coefficients, which depart from the developer's guidance by as much as a factor of 10, suggest to WHG? What might the causes for this be?*

- **Response**: Larger than recommended dispersion values are not necessarily an indication of poor model accuracy. Higher than average dispersion coefficients typically indicate that the model requires increased mixing ability in order to reproduce the observed data. If the selection of higher dispersion coefficients can be justified or explained by the lack of the model's ability to resolve or represent known physical processes, then higher dispersion coefficients can be reasonably assigned to account for these non-simulated mixing processes. Therefore, as described in the peer review, the larger dispersion coefficients that are required to calibrate the RMA-4 model may indicate that there are some important processes that are not being adequately captured by the hydrodynamic model (i.e., 3-D processes) or there is inadequate model resolution to capture important 2-D processes. This also may mean that the nitrogen input values (benthic flux, atmospheric, watershed load) are overestimated such that increased dispersion is required in order to match the observed nitrogen data.

*Would you expect the implementation of such steps to provide improved reliability in the model's ability to predict the response to changes in nitrogen inputs? Why?*

- **Response**: If there is confidence that there are improved values of background concentration, benthic flux, or modified dispersion coefficients that could be applied, then the model would result in an improved ability to predict nitrogen concentrations in Pleasant Bay. At minimum, the steps recommended would provide reasonable upper and/or lower bounds on the nitrogen concentration model results. Essentially, the steps recommended would quantify the level of significance of each of the identified concerns. That would result in an improved confidence in the overall model.

*The draft report states (p.37) that "Howes et al expresses that although nitrogen results are very sensitive to the selection of the dispersion coefficients, there remains a high confidence in the dispersion values used in the calibrated model."* *<u>Does WHG have a high level of confidence in the dispersion values on a basis other that reliance on Howes et al? What is the basis of WHG's confidence or lack of confidence?</u>*

- **<u>Response</u>**: The dispersion values must be selected to ensure that the model results match, as closely as reasonable, the observed nitrogen values. Based on the information provided in the MEP Pleasant Bay report, the dispersion coefficients selected do allow for adequate prediction of model results to the observed data. Therefore, Howes et al. express confidence in the selection of the dispersion coefficients even though the results are highly sensitive to the dispersion values.
- **<u>Response</u>**: Woods Hole Group understands that the dispersion coefficients must be chosen such that the model results match the measured data, and based on the given input conditions and the measured data, the dispersion coefficients need to be those selected by MEP. However, due to the large variability of the dispersion values, as well as the sensitive nature of the selection of the dispersion values on the nitrogen concentration results, Woods Hole Group is concerned that a small change in a dispersion value may result in a significant change in the nitrogen concentration results. Specifically, the significant sensitivity to the dispersion values coupled with the large range of measured nitrogen values used for calibration purposes suggests that site-specific sensitivity analysis of the dispersion coefficients could be warranted for Pleasant Bay. A site-specific sensitivity analysis on the dispersion coefficients used in Pleasant Bay would result in improved confidence in the model results and provide a quantified range of nitrogen concentration results.

*<u>What would WHG do if it encountered the same situation (model calibration required dispersion coefficients which were approximately a factor of 10 higher than the levels advised by the model developer) in a client modeling engagement? How would it be addressed?</u>*

- **<u>Response</u>**: As discussed, larger than recommended dispersion values are not necessarily an indication of poor model accuracy. Higher than average dispersion coefficients typically indicate that the model requires increased mixing ability in order to reproduce match the observed data. If larger than expected dispersion coefficients were required in a modeling situation, Woods Hole Group would (1) try to identify possible physical processes that are not being adequately captured by the numerical model, (2) increase model resolution or dimensions to account for potential physical processes that were not captured by the model, (3) ensure the accuracy of the water quality constituent inputs to verify that overestimated input values are not requiring a unnatural increase in mixing potential to match the observed data, (4) perform a sensitivity analysis on the water quality constituent input data for a range of reasonable values to determine the impact on the selection of the dispersion coefficients, and (5) perform a sensitivity analysis on the dispersion coefficients.

*Considering the first bulleted item under Task 2 above, is the problem of the magnitude of the dispersion coefficients required to produce a calibrated model an outgrowth of the Pleasant Bay system complexity exceeding the modeling system's capabilities? What other reasons can WHG offer for the use of dispersion coefficients well beyond the designer's recommended levels? The fact that dispersion coefficients required to make the model fit the observed nitrogen data are about* **10 times higher** *than one would expect raises some troubling issues. Is it conceivable that the 2-D model does not correctly describe the "real world"? What types of errors would cause this inconsistency?*

- **Response**: The complex nature of the Pleasant Bay system likely does have an influence on the values of the dispersion coefficients. Potential reasons for increased dispersion values include, but are not limited to:
  - Inadequate model resolution
  - Inadequate model dimensions (e.g., 3-D processes are not included)
  - Overestimated nitrogen input

  The 2-D model by definition cannot completely represent actual estuaries, which are 3-dimensional. However, in many cases a 2-D model provides a reasonable representation of the real world system. The possible causes of this inconsistency have been discussed both in the peer review and in responses to similar questions above.

*On May 7 the presentation included a discussion concern about the process of model calibration due to (a) the use of averaging; (b) inconsistent methodology; and (c) failure to assess maxima and minima.*

*Please include a discussion and explanation of these concerns in the final report.*

- **Response**: This discussion and explanation is presented in section 2.3.1.3 of the peer review report.

*On May 7, there were several times when concern was expressed about the way the model was calibrated for bioactive nitrogen and concern was expressed about the model's ability to "work" using bioactive nitrogen.*

*Please expand on these concerns by identifying specific reasons for the concern and its impact on the operation of the model.*

- **Response**: This discussion is provided in section 2.3.1.1, although this is a topic of discussion that spans all three tasks and does not directly fall within the numerical modeling review task.

*Mr. Bosma indicated the difficulty in reviewing the "linked-model" without having the input information. <u>This fact should be emphasized in the final report and a recommendation to reduce uncertainties for Orleans would be very useful.</u>*

- **<u>Response</u>**: The importance of the input data in the assessment and evaluation of the numerical modeling is emphasized throughout the peer review report. In order to provide quantification of many of the uncertainties, the Town of Orleans should continue to pursue acquisition of all the data used to develop the linked model.

*From the WHG draft Report and the discussion on May 7$^{th}$, several uncertainties and biases have been identified in the SMAST linked-computer model for Pleasant Bay. <u>Can WHG comment in the Final Report on the need for an audit of the input data and for running the computations with revised input to determine the consequent output?</u>*

- **<u>Response</u>**: Woods Hole Group does not believe an audit (directly checking that t values have been correctly entered into the model input files) of the input files is necessary, since we are confident that the MEP investigators likely QA/QC'd their model input files. Woods Hole Group does recommend model simulations that may provide additional output that may give added confidence to the Town of Orleans to help provide ranges of reasonable results and provide guidance in the Town's decision making process. For example, the sensitivity simulations recommended in the peer review may provide better quantification of the level of uncertainty that exists within the model results.

*WHG calculates that there is vertical stratification in the terminal ponds. <u>How would the Town of Orleans validate this observation by measurement?</u>*

- **<u>Response</u>**: In order to verify the potential stratification of the drowned kettle ponds, and their relative importance on the overall nitrogen concentrations, temperature and salinity data should be collected as a function of depth in 2 or more of the kettle ponds. This data collection would consist of simple Conductivity-Temperature-Depth (CTD) casts throughout a summer season(s). This recommendation is provide in the overall recommendations of the peer review report.

*Mr. Bosma indicated on May 7$^{th}$ that the 2-dimensional model may oversimplify the dispersion of nitrogen in the deeper, terminal ponds, and that a 3-dimensional model may be more appropriate. <u>Can WHG recommend an approach to evaluate or compare the application of more current 3- dimensional models versus 2-dimensional models as a predictor of nitrogen concentrations for deeper ponds?</u>*

- **<u>Response</u>**: If additional data collected reveals that the terminal ponds are stratified, then there are a number of options that could be considered to evaluate the 3-D processes in the ponds, including:

1) The existing MEP model could be expanded to three dimensions for the entire Pleasant Bay system using RMA-10 (3-D hydrodynamics) and RMA-11 (3-D water quality)

2) The existing MEP model could be expanded to three dimensions in the terminal ponds only. The RMA series of models has the ability to combine both a 3-D grid portion and a 2-D grid portion

3) An independent 3-D model of a terminal pond could be developed and calibrated to the new data collected in the terminal pond. This model would assess the processes and dispersion of the nitrogen within a terminal pond.

**SECTION THREE**

*Regarding the "Sentinel Species". . . Eelgrass has been selected by the Massachusetts Estuary Project of the Mass. DEP as the "sentinel species" to observe in monitoring the health of estuaries in southeastern Massachusetts. During the past 15 years or so eelgrass has inhabited about half of the subaqueous portion of Pleasant Bay, a very widespread and healthy distribution for New England estuaries. As noted above, there is no historical evidence of eelgrass ever successfully inhabiting the northernmost sub-embayments in the Bay. According to the SMAST-MEP report, the areal coverage of eelgrass elsewhere in the Bay decreased by about 90 acres, or approximately 5% over the 6 year interval from 1995 to 2001. The SMAST-MEP report asserts "It is almost certain that a primary cause of the observed eelgrass decline results from increasing water column nitrogen levels within these environments,,,"(p. 193). Unfortunately, there are no reliable data on nitrogen concentrations within Pleasant Bay prior to the year 2000 to support this claim.*

*We and the Woods Hole Group agree that other possible detriments to the health and viability of eelgrass include such disparate agents and events as:*

*1- burial by storm and overwash deposits,*
*2- remobilized substrate exhuming eelgrass roots and inhibiting propagation,*
*3- commercial shellfish harvesting resulting in habitat disturbance,*
*4- infection by wasting disease and other pathogens,*
*5- grazing by herbivores and parasitism by epibionts.*

*Since all of these are common within the Pleasant Bay system, is there any rational basis for ranking the relative significance of these various potential causal factors, and claiming that the likely cause of eelgrass decline is septic effluent? Although abnormally high nitrogen concentrations in Pleasant Bay may have had an adverse impact on the health of eelgrass in certain areas, we cannot ignore the potential adverse impact of the various other detrimental factors. Since a variety of known and unknown adverse factors may impact any estuarine species, would it seem to be unwise or prudent to rely on any "sentinel species" as an indicator of any single adverse environmental attribute?*

- **<u>Response</u>**: This is discussed in Sections 3.2 and 3.3

*Regarding Threshold Nitrogen Concentrations* **. . .** *We understand that nitrogen is the limiting nutrient for primary productivity in marine ecosystems. This environmental parameter clearly is a double edged sword, in that too little nitrogen inhibits productivity and too much nitrogen promotes eutrophism with all its unpleasant side effects and detrimental consequences. The major trophic category that depends on nitrogen is phytoplankton, which constitutes the base of the marine food pyramid. Phytoplankton in turn are the limiting food resource for zooplankton, which together with the phytoplankton make up the main food resource for suspension feeding bivalves such as mussels, clams, scallops and oysters. It is not clear at what point nitrogen concentrations change from being an attribute for efficient trophic exchange and productivity in a marine environment to being a detriment promoting eutrophism.*

*More pertinent to the present situation in Orleans, where is the empirical body of evidence that supports establishing a definitive limiting threshold of 0.16 mg/l bioactive nitrogen concentration in estuarine ecosystems? Why is 0.14 mg/l of bioactive nitrogen acceptable, while 0.18 mg/l is unacceptable, a difference of only 40 ppb? Is our knowledge that definitive and precise?*

- **<u>Response</u>**: This is discussed in Section 3.3. Since a value of 0.14 to 0.15 mg/l has worked, the town of Orleans might want to use these values as a conservative approach, as recommended by MEP.

*In the May 7 meeting the first slide presented: Key Issues/Biases relates to possible factors in eelgrass decline. For three of the factors, the column: Recommendation simply states "ignore". The use of the word "ignore" is confusing at best and can be misleading. The use of the word "ignore" suggests that it is known that the factor plays no role and, therefore, is not worth considering. Dr. Teal acknowledged that any or all of the factors such as wasting disease, grazers, etc. may have played a role in changes in eelgrass area over the 50-year period from 1951 to 2001; however, from Dr. Teal's remarks in the meeting, his opinion on a variety of causes other than nutrients was that there is no data or experience to quantify or assess their impact. This observation concerns semantics and the impression created. The use of "ignore" creates an impression that is clearly inconsistent with Dr. Teal's statements. <u>If indeed there is no data or experience on which to base quantification or otherwise assess their impact, it would seem more appropriate to indicate that there is no recommendation.</u>*

- *WHG was requested to provide references to situations comparable to our situation in Pleasant Bay where similar in-situ nitrogen loads (diffuse non-point source) existed and similar bioactive nitrogen threshold values were applied and there resulted recovery in eelgrass and benthic infaunal habitats. <u>If there are such reference examples please provide the comparable details on nitrogen concentrations and loads and the time line for recovery or, if no reference examples exist, please clearly state so.</u>*

- *Dr. Teal should note in his final report that that he was unable to find any source of information on the health of eelgrass and benthic communities that is equivalent in nature to the Pleasant Bay Estuary. Dr. Teal should note emphatically that he has been unable to find any independent source of relevant information that confirms the DEP water quality criteria (specification) of (1) 0.16 mg bioactive nitrogen/liter at PBA12, the sentinel station for Pleasant Bay, to assure the health of eelgrass, and (2) the 0.21 mg bioactive nitrogen/liter in the sub-embayments to assure the health of the benthic communities.* ***[In fact, sampling data shows that in the last 5 years, the nitrogen concentrations in Pleasant Bay at PBA12 have been at or below the 0.16 mg/liter DEP specification.]***

- *Dr. Teal should clarify his examples where health and extent of eelgrass improves with decreasing nitrogen inputs* ***to explain that raw sewage discharges, with very high BOD requirements, are the primary source of degradation of a river or water body, and not nitrogen.*** *This information is misleading and leads the uninformed reader to an incorrect conclusion.*

- **Response**: This is addresses in Section 3.3 with new information.

***Pertaining to "Degraded" Northern Sub-Embayments*** *. . .*
*Most of the so-called "degraded" benthic habitats in the Pleasant Bay system (characterized by lack of eelgrass, hypoxic bottom water, depaupered micro-infauna dominated by Capitella) are within the semi-enclosed sub-embayments, or drowned kettles at the northern extremity of Little Pleasant Bay. It is true that these semi-enclosed sub-embayments are characterized by inferior water quality and poorer flushing than the larger lagoonal basins in the rest of the Pleasant Bay system, which are not characterized by hypoxia and eutrophication. SMAST-MEP attributes this contrast to anthropogenic sources of nitrogen, chiefly from septic effluent. However, such contrasts may also be attributed to inherent differences between the semi-enclosed sub-embayments and the remainder of the Bay system.*

*All of the northern sub-embayments are floored with thick accumulations of organic-rich, fetid, black mud, while most of the rest of Pleasant Bay's substrate consists of silty or well washed sand. The reason for this lies not only in modern processes of deposition and/or pollution, but more importantly in the geologic evolution of these sub-embayments from ice-block kettles over the past several thousand years. During this evolution the drowned kettles have accumulated a long and thick record of organic-rich, anoxic mud, and when disturbed or agitated this mud exerts a strong influence on the chemistry of the overlying water column. This situation may not have anything to do with septic effluent, and it may not be amenable to remediation.*

- **Response**: Kettle Hole ponds do accumulate organic matter on their bottoms, but this accumulates slowly if the ponds are not eutrophied. They can have very healthy benthic faunas living in anoxic sediments as long as the overlying waters are oxic.

*One of the characteristic aspects of the sub-embayments is the extreme oscillation in the dissolved oxygen concentration of the bottom water. The oxygen concentration varies between hypoxia of less than 2 mg/l to moderately oxygenated at more than 6 mg/l on a semi-diurnal or diurnal basis (p. 157-161 of the SMAST-MEP report). Since these measurements were made beneath the euphotic layer in the central and deepest parts of the sub-embayments, it seems unlikely that they are reflective of daily photosynthesis-respiration cycles. The frequency of semi-diurnal oscillations suggests that they may be reflective of tidal turnover of the contained water column. These oscillations into hypoxia or anoxia account for the highly stressed benthic habitat and depaupered infaunal community associated with the central parts of the sub-embayments. In spite of the fact that about half of the volume of each sub-embayment is tidally flushed twice a day, the central deepest part may approximate a quasi "dead zone".*
*How can we be assured that this is a natural phenomenon or an anthropogenic attribute that could be remediated? During intervals of anoxia could nitrogen reduction through denitrification to dinitrogen gas make the sub-embayments efficient nitrogen sinks to the atmosphere, thus reducing concentrations of bioactive nitrogen within the Pleasant Bay system?*

***Pertaining to Nitrogen Loading from Groundwater (****in response to comments raised by Dr. Teal****)*** *. . .*
*When septic effluent in Orleans has filtered through the oxidizing vadose zone to the water table, it is entrained and diluted with groundwater flowing within the Monomoy aquifer. This is an unconfined, relatively homogeneous and essentially isotropic aquifer in which the groundwater flows seawards under the impetus of the hydrostatic head produced by the differential elevation of the water table. There is no appreciable physical inertia for the effluent to fall through the groundwater, so most of the effluent remains within the upper portion of the seaward flowing groundwater. Thus, the effluent approaches the ground surface level as it flows towards the shoreline at grade, ultimately discharging at or very near the shoreline. Due to the significant density difference between fresh water in the aquifer and the underlying seawater that surrounds the Cape, there is no reason for significant amounts of fresh water to flow under the salt water upon which it floats, and emerge from under Pleasant Bay.*

*All of the assertions concerning nitrogen loading of Pleasant Bay from groundwater are modeled on hypothetical assumptions based on measured data from various other areas. There are no site- or time-specific data on the concentration of the various species of nitrogen dissolved in the groundwater within Orleans, and no attempt has been made to calibrate the model with real analytical data from the groundwater. This is a major omission in our knowledge concerning nitrogen loading in Pleasant Bay.*

*Most of the shoreline of Pleasant Bay is bordered by relatively mature mixed hardwood and coniferous forest, with extensive fringing salt marshes along the shoreline. This is* *especially true bordering the more quiescent and less developed sub-embayments in the northernmost portion of the Bay system. Nitrogen is a critical nutrient element required for primary productivity, and it is actively attenuated from groundwater by growing vegetation. There has been no apparent attempt in the SMAST-MEP analysis to consider the probable assimilation of nitrogen in discharging groundwater by coastal vegetation. Since coastal trees are rooted directly into the groundwater near the shoreline, and since marsh grasses have been shown to be efficient effluent scrubbers in numerous studies, how significant is nitrogen loss to plant uptake along the coast in reducing nitrogen levels in the groundwater before discharge to the Bay?*

- **Response**: Nitrogen uptake by plants has little effect on scrubbing unless the plant material is harvested as is done in artificial wetlands used for wastewater treatment. But even there most of the treatment is through the nitrification/denitrification cycle. This cycle almost certainly does have an influence on nitrogen discharge to the Bay but has not been measured.