Response to Independent Technical Reviewer Comments on *Subtask E. Summary of Hydrodynamic Analysis (March 27, 2018 Draft)*

October 1, 2020

Expert Technical Review Comments on the March 27, 2018 Draft E Memo

This section contains the original responses written by each of the technical reviewers.

Question 1-1

Comment on the overall organization, clarity, and general effectiveness of the memorandum. Is it clear what was done, why it was done, and what was learned? If not, state deficiencies and provide recommendations or suggestions on how the deficiencies might be resolved or improved (e.g., re-organization of the memorandum).

Comment 1-1 Bierman

No Comment Tracking ID #

Given its purpose, the overall organization, clarity, and general effectiveness of the memorandum are adequate. Overall, it is clear what was done, why it was done, and what was learned.

> *Response: Thank you for your comment.*

Comment 1-1 Brush

Comment Tracking ID #1

The memo is well-organized with a structure that was easy to follow, and I think overall it is effective at communicating what was done at a fairly high level and what was found. It ends with a very strong conclusions section. However there were a number of places where I felt greater explanation was warranted, or where the text was too confusing to fully understand what was done. I have detailed most of those in my response to Question 2 below, because they relate to my ability to assess the technical quality of the work.

The one item not covered in my response to Question #2 below is as follows: While not specifically relevant to Subtask E, not enough information was presented to understand Equation 1, and a proper citation was not provided. There was no derivation or discussion of the equation to allow for adequate review of its appropriateness for % reduction calculations.

> *Response: Thank you for your comment. We added additional detail to the memo to provide further explanation. Please refer to responses provided to reviewer's comments on Question 2 below. Additionally, we added more detail to what the equations terms mean and why the equation is appropriate to use.*

Comment 1-1 Janicki

No Comment Tracking ID #

The overall organization is good.

Response: Thank you for your comment.

Comment Tracking ID #2

The clarity of presented information is adequate but could use improvement: Why is the SWEM mentioned in the Introduction, then never again? Is it necessary only because the Scope for this Task specified this model? If so, providing that logic would be helpful as well as an explanation for not including that discussion in the model selection section. (Page E-1).

Response: We removed the mention of SWEM from the introduction, as it does not add anything to the narrative.

Comment Tracking ID #3

The Introduction would benefit from additional text clarifying the Percent Reduction equation. Specifically, how are C_w and S_{LIS} defined? (Page E-1).

Response: We added clarification regarding the equation, including C_w and S_{LIS}, in the text of Memo E. Note that C_w was updated to C_t to make terms consistent between Memo E and Memo H.

Comment Tracking ID #4

The addition of maps defining the extent of the embayments would be appropriate, so that the regions contributing to "...average salinities inside and outside the selected embayments..." could be defined. (Page E-8).

Response: We added an additional callout for Figure E-3, which provides a map of all the embayments.

Comment Tracking ID #5

The identification of areas of influence is confusing. The maps show isopleths of percent river water contribution, but the captions for Figures E-4 through E-6 identify these lines as depicting "percent dilution". (Pages E-11 - E-13).

Response: We updated the type of maps used to display the relative contribution of the three rivers and updated the figure titles.

Comment Tracking ID #6

Tables E-4 through E-6 define dilution factors for each embayment. The algorithm used to calculate the dilution factors is not presented.

Response: The algorithm used is provided in the Particle Tracking section of Memo E.

Comment 1-1 Justic

No Comment Tracking ID #

The memorandum is well organized and well written.

> *Response: Thank you for your comment.*

Question 1-2

Comment on the overall technical quality of the memorandum. Are the assumptions used in applying the NYHOPS model for "particle tracking," embayment contribution modeling, area of influence estimation, and salinity modeling reasonable? Is employment of the NYHOPS model for "particle tracking," embayment contribution modeling, area of influence estimation, and salinity modeling consistent with relevant existing and emerging scientific practices? Are the results reasonable, and are the conclusions justified and adequately qualified where necessary? Are the results consistent with sound ecological science? Do the embayment mixing values seem realistic and hydrologically valid?

Comment 1-2 Bierman

Comment Tracking ID #7

Given its purpose, the overall technical quality of the memorandum is adequate. The assumptions used in applying the NYHOPS model are reasonable and consistent with relevant existing and emerging scientific practices. The results for salinity dilution analyses and dilution factors for the Connecticut, Housatonic, and Thames Rivers for selected embayments are reasonable.

More detail could be provided on model spatial scale for the individual embayments. In Table E-2 it is stated that the standard model grid is 500 x 500 meters, but that small embayments may not be fully resolved. It would be useful to provide a table that contains the number and spatial scale of model grid cells in each of the selected embayments.

The issue of model spatial scale is important because the spatial scales relevant to eelgrass in the individual embayments are much smaller than that of the standard NYHOPS model grid. For example, the LIS-wide Eelgrass Habitat Suitability Index (EHSI) model (Vaudrey et al., 2013) is based on grid cells that are 30.48 x 30.48 meters. The EHSI sub-models used for six selected study sites are based on grid cells that are 7.62 x 7.62 meters.

It is stated on Page E-17 that the lack of vertical mixing and diffusive exchange among grid cells affected the estimates of river water movement throughout the LIS. The reason for this is not clear.

Did the NYHOPS model not represent vertical mixing and diffusive exchange, or was information on these processes not included in the NYHOPS model outputs?

> *Response: The standard NYHOPS grid varies in resolution, as described in the Methods section under the NYHOPS Model Download and Processing section and shown in Figure E-1. We corrected the text in Table E-2 that was leading to confusion. The NYHOPS model did not provide vertical velocities or diffusivity information, and we added text to clarify this.*

Comment 1-2 Brush

Comment Tracking ID #8

I will divide my comments into those related to choice of hydrodynamic model, salinity modeling, and particle tracking (and associated area of influence calculations). I am unclear which aspect of the memo "embayment contribution modeling" refers to, but my review assesses all aspects of the document. As noted above, I thought the Conclusion section was a nice summary of the results with appropriate caveats and qualifications.

Hydrodynamic Model Selection

The case for using the NYHOPS model was solid and convincing. The model appears to be state of the art and entirely in line with existing practice. There are some limitations but I agree with the assessment that the best available model and approach were used given the available time and resources. However there are a few key details that should be addressed to fully evaluate the approach:

- Table E-2 indicates that the NYHOPS developer "recently completed a successful effort to validate the model performance for flow, temperature, and salinity in LIS." It is difficult to know how much confidence to place in modeled salinity and currents without seeing those validation results.

Response: Thank you for your comment. EPA added an additional citation to Table E-2 that more thoroughly explains the model developed and validation.

Comment Tracking ID #9

- Table E-2 also notes that "Small embayments may not be fully resolved on a 500-m grid," and it is not clear from Figure E-1 if all of the selected embayments contain grid cells. If the latter was the case, salinity could not be computed for those embayments. The text additionally states that different embayments have different numbers of grid cells (which could lead to biased salinity estimates), and that this likely led to overestimation of dilution of the landward, freshwater end-members. That said, I agree that this is the best approach with available resources. However the issue of embayments without any grid cells should be addressed.

Response: Best professional judgement was used to ensure that all embayments are represented as accurately as possible. All embayments presented in Table E-3 and following contain at least one model grid cell, allowing at least a rough calculation of salinity.

Comment Tracking ID #10

- It is unclear if the individual embayments in the NYHOPS model were forced with reasonable estimates of local freshwater discharge. That is likely critical to an accurate simulation of salinity within the embayments and an accurate estimate of dilution with LIS water. If embayment discharge was not included, the computed dilution rates could be biased high. This should be clarified.

Response: We did not alter the underlying model, but instead accepted the constraints imposed by it, including freshwater forcing. To provide greater detail in that regard, we have provided a

> *more detailed citation on the model development and validation and added that reference to Table E-2.*

Comment Tracking ID #11

Salinity Modeling

As noted below, I have some concerns about the assumption of conservative mixing of TN. Putting that aside, I followed the calculations in this section and found it to be an elegant approach for estimating mixing. The approach is consistent with empirical estimates often employed by estuarine ecologists to compute bulk parameters like flushing time. As an aside, a hydrodynamic model is being used that computes velocity and therefore volume exchange, so I wondered why dilution was not computed directly, or via a particle tracking approach. Regardless, I find the approach entirely valid.

> *Response: Thanks for your comment. Please note that dilution was estimated using a particle tracking approach.*

Comment Tracking ID #12

(Note: I did have one question about equations 6 and 7. While these are not required to compute the dilution rates, it seems that implementing these equations requires one to know the value of C_0 for each embayment. This will not affect the results of the subtask, but since these equations were given, I wondered if it was possible to implement them with available data.)

> *Response: C_0 represents the concentration in the freshwater inflow. Implementing these equations does indeed require an estimate of the value of C_0 for each embayment. As stated in the text, salinity C_0 is assumed to be approximately equal to zero which is a defensible assumption for freshwater, allowing calculation of the dilution ratio. Equations 6 and 7 demonstrate how the dilution ratio could then be applied to a substance for which C_0 is not equal to zero, such as total N. C_0 estimates for total N can be obtained from monitoring or watershed modeling. Note that we did update some of the terms for consistency with Memo H equations. We also moved some of the equations from Memo E to Memo H, as they make more sense to include there.*

Comment Tracking ID #13

I was initially concerned about limiting the analysis to July-September, but I appreciated the sensitivity analysis using a broader temporal window, and agreed with the justification for using the more restricted time period. However a bit more justification on why this is a "critical time period" would be helpful.

> *Response: We expanded the seasonal window and ran multiple scenarios to examine the effects of different seasons, as described below:*

- *Scenario 1: Particles released for full year*
- *Scenario 2: Particles released from March through October*
- *Scenario 3: Particles released from July through September*
- *Scenario 4: Particles released from March through May*
- *Scenario 5: Particles released from March through May and monitored through October*

Comment Tracking ID #14

It was unclear to me why salinity was computed only over the top five vertical layers of the model. I assume this was related to depth but this choice was not justified. Since the model uses sigma (terrain-following) coordinates, this approach could be averaging salinity inside and outside of the embayments over different depth ranges. I assume all grid cells were averaged within each embayment, although this is not stated. The report also does not state how many cells outside of each embayment were used to compute external salinity.

In conclusion, I find the approach elegant and appropriate given available resources, and in line with existing scientific practice. I also find the estimated dilution factors reasonable given the small volume of these embayments and what I imagine must be small freshwater inputs and strong tidal mixing with LIS. However, there are a few issues that should be addressed to increase confidence in the computed values.

> *Response: Thank you for your comment. The salinity analysis was performed on only the top six layers because the bottom layers may include a salt wedge. The top layers were selected because they approximately correspond to the photosynthetic zone for which nutrient limits may be needed to prevent excess algal growth, so the appropriate dilution calculation should also be applied to this zone. If vertical stratification is not present, analysis over the top layers should yield the same result as analysis over all layers. Grid cells within each embayment were indeed averaged as stated in the comment. Model estimates of salinity immediately outside of each embayment used one to four grid cells depending on the width of the embayment mouth relative to the grid resolution.*

Comment Tracking ID #15

Particle Tracking

Particle tracking is a commonly used, state of the art approach for tracking river plumes and dilution of point source inputs, and the overall approach used with the NYHOPS output appears rigorous. Particles were released only in the upper six vertical layers, because "only those layers had significant net lateral particle movement in the model." This may be a problem because model estimates of particle transport would potentially be overestimated; i.e., if particles were released uniformly throughout the water column, those in lower layers would not travel nearly as far as those in surface layers. That said, model results show practically no influence of the rivers on the embayments, so addressing this issue would not likely affect the results in a significant way.

> *Response: We added clarification on the effect of excluding lower layers.*

Comment Tracking ID #16

A potentially more important issue is that the calculations did not allow for vertical exchange of particles. While I agree that the best approach was used given resource constraints, the lack of vertical exchange may be problematic in a system characterized by two-layer estuarine circulation.

> *Response: We added clarification of the effect of not having mixing and reinforced the constraints of the NYHOPS model.*

Comment Tracking ID #17

I found it very unclear how the actual percent dilution of particles was computed (pp. E-7 to E8). Particles were released every four hours over the entire growing season. It was not clear to me how resulting concentrations were averaged or integrated over each release, the entire season, each year, and the six depth layers. It was also unclear how the original release point cell concentrations were computed for the same reasons. I also wondered if movement of particles among sigma layers and cells with varying dimensions led to artificial concentration or dilution of particles in these calculations.

> *Response: For the reanalysis, we are now presenting area of influence (using heat maps) instead of isopleths of percent dilution that provides the full range of potential influence areas instead of one based on a specific percentage. One goal for this application is simply to identify those sample station data that can be used to characterize conditions in the "embayment" area influenced by the river. Based on reviewer feedback, we feel these heat maps will allow us to demonstrate that the samples we use are within the area of the river's influence better than a discrete cutoff. We also added additional detail to Memo E regarding the particle tracking analysis. Calculation of relative dilution is now explained as follows: "The particles moved according to the NYHOPS flow vectors for the nearest cell center, and each particle's motion was tracked. Particles leaving LIS were no longer tracked. The sum of particles present in each grid cell over the entire simulation duration was tracked. This sum was divided by the volume of each grid cell to calculate particle concentration. Relative concentration was calculated by dividing each grid cell's concentration by the release point concentration. This relative concentration estimates the dilution of the water in that grid cell compared to the Connecticut, Housatonic, and Thames rivers."*
>
> *We acknowledge that the lack of vertical flow vectors and calculation using only net daily lateral flow introduces some biases into the calculation. As stated, the analysis is "likely to produce an estimated zone of influence that is more compact than actually occurs, particularly at the edges."*

Comment Tracking ID #18

The choice of 40% river water / 60% dilution seems to be an arbitrary choice, even with the justification provided on page E-10. An alternative approach might be to express influence across the continuum of dilutions. (Note: I think there are typos on p. E-10 in that 'river water' was intended instead of 'percent dilution' in the three places this term appears. This is also the case in the legends for Figures E-4 to E-6. I also note that the justification on p. E-10 about spring runoff seems out of place since the model analysis is limited to July-September.)

> *Response: See response to comment tracking ID #17 for a response.*

Comment Tracking ID #19

The release points on Figures E-5 and E-6 seem too far up-river (≥ 2 miles) to represent discharge at the river mouth.

> *Response: We re-ran simulations with the uppermost grid cell in NYHOPS for a consistent choice of release points between the different rivers.*

Comment Tracking ID #20

I was also concerned about the increasing contours on Figure E-4 as one moves up-river, and the up-river pattern in Figure E-6 where the percentages decrease to 50, then increase to 90, and then decrease again to 80. These patterns were not explained, but I think they pose concerns for the model output.

Perhaps the grid resolution is not sufficient in the rivers for this analysis? (Admittedly the focus of the analysis is down-river from the release point, so this may not be an issue.)

> *Response: A possible reason for the increasing contours is that when particles moved upstream, the flow in those regions was not high enough to flush out those particles and they remain in those regions, thereby increasing the particle count and relative concentration.*

Comment Tracking ID #21

Finally, the calculation of river dilution factors (p. E-13, Table E-4) was not explained so I am unable to evaluate these findings. That said, the values are extremely low which is what I would expect. However, the underlying assumption is that concentrations originating from these rivers are "conserved and superposable" (p. E-16). I recognize that far-field impacts of river discharges is a common management question in LIS, but given that nutrients do not behave conservatively within estuaries, particularly over the large spatial scales between these river mouths and most embayments, I am not sure how much can be gathered from these types of calculations.

Overall, I think that there are a number of uncertainties in the particle tracking approach. I again agree that the best approach was used given the constraints. However, I think a number of the issues identified above should be addressed and more clearly explained before accepting the particle tracking results.

> *Response: We elaborated on how both dilution based on salinity (in the introduction) and based on particle tracking (particle tracking section) were calculated to provide greater clarity. We also provided additional text on the limitations of the underlying model used for the analysis.*

Comment Tracking ID #22

Comment 1-2 Janicki

The assumptions are clearly stated and reasonable. Use of the NYHOPS model is appropriate and results are reasonable and consistent with normal practices. The conclusions are justified and qualified appropriately. The embayment mixing values appear reasonable and valid, but are limited by the temporal and spatial resolution of the model used to derive them.

> *Response: Thank you for your comment. We ran additional scenarios that expanded the seasonal window, which helps address the commenter's temporal concern. Regarding the commenter's concerns about spatial resolution of the model, the NYHOPS model was the best available model given data and resource availability for this analysis, so the spatial resolution is limited to the resolution of the NYHOPS grid.*

Comment Tracking ID #23

Comment 1-2 Justic

The NYHOPS model appears well suited for the analysis of hydrodynamics and salinity in LIS. However, it is unclear why only the July-September period was used in the analysis. While it is true that this period coincides with the summer growing season, the freshwater inflows are minimal at this time, which has profound effect on the estimates of dilution as well as the estimates of the areas of influence of the Connecticut, Housatonic, and Thames rivers. One could argue that taking into account only the summer growing season underestimates the far-field influence of these rivers.

Further, the calculated dilution ratios are close to 1 for most embayments, as there are virtually no salinity differences between embayments and open areas during July-September (Table E-3, page E9). From an ecological point of view, it would be valuable to estimate the areas of influence during the spring runoff period, when the regions of riverine influence could be substantially larger compared to the July-September period. This is important because in shallow coastal systems, such as LIS embayments, a considerable portion of external nutrient loading can be taken up and stored in biota and sediments over weeks and months, and subsequently recycled to fuel pelagic and/or benthic food webs.

> *Response: EPA expanded the seasonal window and ran multiple scenarios to take into account the concerns expressed in this comment, as described below:*
>
> - *Scenario 1: Particles released for full year*
> - *Scenario 2: Particles released from March through October*
> - *Scenario 3: Particles released from July through September*
> - *Scenario 4: Particles released from March through May*
> - *Scenario 5: Particles released from March through May and monitored through October*

Comment Tracking ID #24

No clear justification was provided as to why the 40% dilution threshold was used to identify the areas of riverine influence (page E-10): "*A value of more than 40 percent dilution was used to define the area of influence because there is error around this value as a function of changes in flow*". Further, the attempt to extend the area of influence estimates for the growing season to higher flow periods does not appear to have a sound scientific basis: "*As such, 40 percent based on the growing season period would encompass an area more likely to include more than 50 percent dilution during higher flow periods (e.g., during spring runoff).*"

> *Response: See response to comment tracking ID #17 for response.*

Comment Tracking ID #25

The fact that a coupled hydrodynamic-biogeochemical model was not available for this study resulted in several assumptions that are not well supported by the available data, such as the assumed conservative behavior of TN (see my response to Question 3 below). Another unsubstantiated statement refers to the fate and residence times of riverborne nutrients (page E-17): "*Because of significant tidal flushing of water, nutrient loads from winter are likely retained only into the late summer primarily through storage in sediment and biota (dissolved nutrients will be flushed out).*" I cannot comment on whether a coupled hydrodynamic-biogeochemical model could have been implemented to aid in this study, but these uncertainties are important and if they remain unaddressed, can lead to later challenges.

> *Response: Developing a hydrodynamic-biogeochemical model was beyond the resources available for this analysis. We selected NYHOPS as the best available model to conduct the analysis. Questions regarding approximation of TN as a conservative substance are addressed below in the response to comment tracking ID #26. Regarding residence time, Bricker et al. (2007) estimate a hydrodynamic residence time for LIS of 2-3 months. This is primarily driven by high winter-spring flows so the assumption that a large part of the winter load will be flushed through the system is reasonable.*

Question 1-3

Is it ecologically valid to assume that total nitrogen (TN) is conservative (i.e., that it is not being removed from the system in significant amounts) for the purposes of this modeling effort?

Comment 1-3 Bierman

Comment Tracking ID #26

Nitrogen was not part of the hydrodynamic modeling effort described in Subtask E. The memorandum states that results from this effort for dilution of salinity will be used as a proxy for nitrogen dilution under the assumption that nitrogen within embayments is approximately conservative. This assumption is not generally valid, especially within embayments and nearshore areas. Not only will there be some settling and volatilization losses, as the memorandum states on Page E-1, but there will also be gains due to sediment diagenesis and the resulting sediment-water diffusion of nitrogen. These processes are complex and can vary in both space and time, especially between embayments and open water areas.

As an example, in their linked watershed-embayment modeling study of the Pleasant Bay System, Massachusetts, Howes et al. (2006) investigated sediment-water exchanges of nitrogen. Howes et al. (2006) Figure IV-20 (below) is a conceptual diagram showing seasonal variation in sediment nitrogen flux. During summer (i.e., the primary period of interest identified in Subtask E), sediment-water nitrogen flux is at maximum values. Howes et al. (2006) Table VI-2 (below) contains total nitrogen loads for individual sub-embayments. The loads for net benthic flux were based on site-specific measurements during the summer period. For most of the individual sub-embayments, and for the system as a whole, net benthic flux of nitrogen to the water column was larger than external nitrogen loads from the watershed itself. For the Pleasant Bay System, the assumption that nitrogen within sub-embayments is approximately conservative is violated by greater than a factor of two.

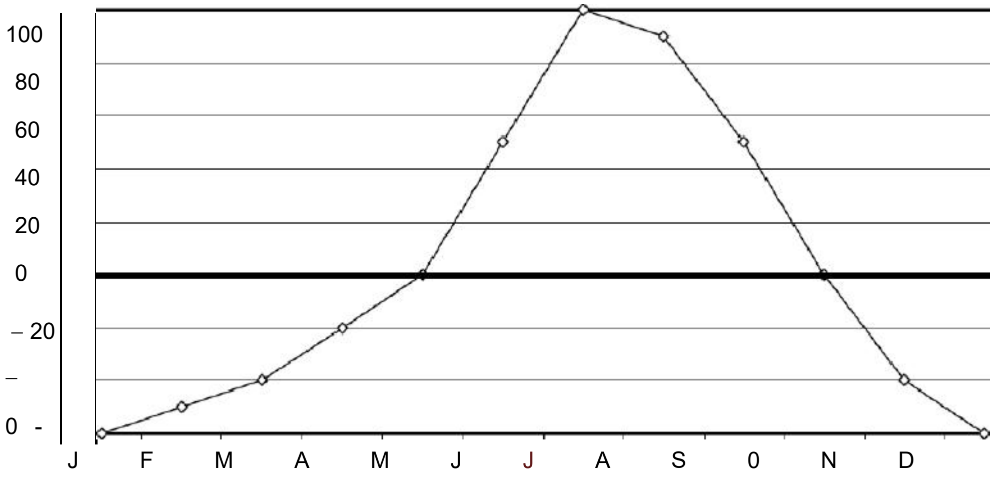

Figure IV-20. Conceptual diagram showing the seasonal variation in sediment N flux, with maximum positive flux (sediment output) occurring in the summer months, and maximum negative flux (sediment up-take) during the winter months.

Table VI-2. Sub-embayment and surface water loads used for total nitrogen modeling of the Pleasant Bay system, with total watershed N loads, atmospheric N loads, and benthic flux. These loads represent **present loading conditions** for the listed sub-embayments.

sub-embayment	watershed load (kg/day)	direct atmospheric deposition (kg/day)	benthic flux net (kg/day)
Meetinghouse Pond	6.197	0.584	14.365
The River - upper	2.773	0.288	6.263
The River - lower	3.879	2.241	10.480
Lonnies Pond	2.441	0.225	1.591
Areys Pond	1.304	0.181	5.996
Namequoit River	2.737	0.523	14.570
Paw Wah Pond	1.860	0.082	3.630
Pochet Neck	8.422	1.767	-0.791
Little Pleasant Bay	7.496	24.023	37.226
Quanset Pond	1.781	0.170	5.988
Tar Kiln Stream	6.123	0.066	-
Round Cove	4.225	0.170	8.416
The Horseshoe	0.638	0.063	-
Muddy Creek - upper	9.981	0.162	4.560
Muddy Creek - lower	8.477	0.205	-1.226
Pleasant Bay	23.159	19.153	149.013
Pleasant Bay/Chatham Harbor Channel	-	17.786	-40.192
Basing Harbor - Ryder Cove	9.819	1.296	9.356
Bassing Harbor - Frost Fish Creek	2.904	0.096	-0.154
Bassing Harbor - Crows Pond	4.219	1.389	0.612
Bassing Harbor	1.668	1.071	-4.976
Chatham Harbor	17.099	14.153	-40.208
TOTAL - Pleasant Bay System	**127.203**	**85.693**	**184.519**

Response: We agree that the processes are complex, that nitrogen is not fully conservative, and that recycling of nitrogen due to benthic fluxes can be important. However, given the lack of a full biogeochemical model of each embayment, the conservative assumption provides a reasonable approximation for estimating the relative importance of N derived from the local embayment watershed versus in-mixing from LIS and its major river inputs, especially given the rapid dilution and short residence time for most of the embayments studied. (Note that the analysis implicitly includes direct atmospheric deposition onto an embayment as part of the watershed load component). The 2011 Peer Review of the Massachusetts Estuary Project Linked Watershed Embayment Model found highly varying importance of net benthic exchanges for TN, ranging from negative to a majority of the total N load. The Pleasant Bay complex example cited in the comment illustrates the complexities of the analysis, but is not necessarily the most relevant example for small embayments in LIS as Pleasant Bay is the largest estuary complex on Cape Cod, with a large surface area, and has sub-embayments with long residence times. The accuracy of the estimates contained in the Pleasant Bay table provided in the comment is not known to EPA, and the table does not represent a complete mass balance as the accounting does not include exchanges with the ocean or losses to the atmosphere due to denitrification (see also response to comment tracking ID #27). As tabulated, 46% of the total net N load is attributed to benthic flux; however, 80% of that benthic flux is attributed to a single sub-segment. The benthic *flux may also contain significant contributions from direct groundwater discharges that derive from the watershed. Most of the nitrogen contained in embayment sediments will ultimately derive from loads from the watershed or from direct atmospheric deposition and a large net positive benthic flux independent of watershed sources would suggest that external loading rates have significantly declined over time. The simplified analysis with conservative assumptions is used to determine the loading rate that would be consistent with achieving a water column concentration target based on dilution. We continue to believe that this is a reasonable first-order approach for approximating watershed loading targets; however, we also recognize that the assumption that nitrogen is conservative introduces uncertainties into the analysis and that additional biogeochemical modeling and/or non-conservative mass balance analyses could be used to refine the estimates in future.*

It is worth noting that the assumptions of nitrogen conservativeness do not affect salinity modeling nor the particle tracking used for area of influence calculation and percent dilution described in the memo. The impact of this assumption is only relevant to any calculation of load reduction that might be made using this work. Changing these assumptions would affect how processing of nitrogen within each embayment affects water column concentrations. As such, we might revisit this assumption during any such future applications.

Comment 1-3 Brush

Comment Tracking ID #27

While I agree that the best approaches have been used given available time and resources, I am skeptical about the assumption that TN will behave conservatively. Estuaries, and perhaps especially the shallow, fringing embayments which are the focus of this work, are well known as major processors and transformers of nutrients as they move from land to sea. Particularly, estuaries are the sites of substantial removal of dissolved inorganic nitrogen (DIN) via denitrification. Additionally, N taken up by phytoplankton and benthic primary producers within these shallow systems may also represent an important sink, at least over the growing season identified here (July-September). While TN is likely to be more conservative than DIN, I am not convinced it can be considered conservative. The text notes that conservative behavior is likely over the spatiotemporal scales considered in the report; since these are small, likely rapidly flushed systems, this may indeed be the case. However, to confirm this, estimates of embayment flushing times would be helpful. Another option would be to create mixing diagrams for TN in the embayments where stations exist along the salinity gradient to test for conservative mixing. (Note: as an aside, I am unsure what is meant by the statement that “Larger losses are likely expected within the watershed ...”, or how that applies to conservative behavior within the embayments.) Overall, I do believe that this effort represents a reasonable first-order approach, but I recommend following this up with additional work that tests the validity of the conservative mixing assumption, or accounts for the potential non-conservative behavior of nitrogen.

Response: Thank you for the comment and discussion. Please see response to comment tracking ID #26 for general discussion regarding the uncertainties introduced by treating total nitrogen as conservative. The comment points out that denitrification can be an important sink of nitrogen in estuaries. If denitrification losses exceed rates of benthic nitrogen load that is due to diagenesis of organic material that is not ultimately derived from the watershed, then the treatment of nitrogen as a conservative substance would yield a protective upper bound on the needed load reduction from the watershed. As stated in responses to several previous comments, we agree that it would be preferable to refine the analysis with a more detailed biogeochemical

> *model that eliminates the conservative assumption, if resources would be available to complete such an exercise. The sentence in Memo E regarding losses in the watershed reads in full "Larger losses are likely expected within the watershed, which should be considered in allocation." This was intended to convey that there will also be denitrification losses within the stream network leading to the embayment. The use of the word "larger" is inappropriate here. Further, the commenter is correct that this statement does not relate to the conservative behavior question. The sentence is superfluous and has been removed from the Memo E text.*

Comment 1-3 Janicki

Comment Tracking ID #28

This assumption imposes a limit on the interpretation of the final results as it does not incorporate biological activity or sediment interactions. Comparison of predicted TN concentrations to observed TN concentration data would provide insight on the validity of the assumption of conservatism. Despite the potential shortcomings associated with this assumption, this exercise has value in the decision making process.

> *Response: We agree that the simplified analysis with conservative assumptions imposes a limit on the interpretation of the results. See responses to comment tracking ID #26 and ID #27. Comparison of predicted and observed TN concentrations might be informative on this topic, but would require development and calibration of watershed loading models for each embayment.*

Comment 1-3 Justic

Comment Tracking ID #29

This issue was briefly discussed on page E-1 of the Subtask E report but clear justification for why TN could be considered conservative was not provided. The reader is referred to the NYHOPS model documentation, where I could not find any reference regarding TN. Further, this topic is not discussed in the LIS Literature Review Memo. Thus, based on the documentation available, I cannot conclude that the assumption concerning conservative behavior of TN in LIS is justified.

I am not entirely familiar with LIS literature, but in the systems I have studied (e.g., deltaic Gulf of Mexico (GOM) estuaries) TN does not behave conservatively and its concentrations can vary over an order of magnitude due to varying sources (e.g., riverine, atmospheric, sediment resuspension, marsh erosion) and sinks (e.g., denitrification, burial) affecting both the inorganic and organic pools of nitrogen. Importantly, plots of TN versus salinity for GOM estuaries do not support the assumption that TN behaves conservatively.

TN is a critical component for estimating LIS nitrogen reductions and further analysis of the conservative/non-conservative nature of TN in LIS is recommended. Insights from other systems do not always reflect reality, but if they remain unaddressed, can lead to later challenges.

> *Response: See responses to comment tracking ID #25 through ID #28. We agree that it would be preferable to refine the analysis with a more detailed biogeochemical model that eliminates the assumption that TN can be treated as a conservative assumption, if resources would be available to complete such an exercise.*