Memorandum

To: Andrew Kolosseus (Water Quality Program)

From: Mindy Roberts (Environmental Assessment Program)

Cc: Anise Ahmed, Greg Pelletier, Skip Albertson, and Karol Erickson (Environmental Assessment Program)

Date: May 8, 2014

Subject: Response to external review comments received on the report *South Puget Sound Dissolved Oxygen Study: South and Central Puget Sound Water Circulation Model Development and Calibration*

We received 41 comments from the following individuals and organizations on the 2009 external review draft report *South Puget Sound Dissolved Oxygen Study: South and Central Puget Sound Water Circulation Model Development and Calibration*:

- Bruce Nairn, King County
- Heather Trim, People for Puget Sound and Futurewise
- Tony Paulson, USGS
- Ben Cope, EPA
- Bill Fox, Cosmopolitan Engineering
- Phil Crawford, US Army, Fort Lewis
- Lincoln Loehr, Stoel Rives

In 2010, the modeling team addressed these comments and proceeded with the water quality model development and application. However, Ecology did not finalize the circulation report until this month.

We addressed these through a combination of edits to the final report and detailed responses to the comments compiled in the table below.

Commenter	#	Comment	Response
Bruce Nairn (King County)	1	In general, the report is well written and shows that a good deal of effort has been put into calibrating the model. The model calibration presented in the report appears reasonable for the spatial extent and nature of the model. Specific comments on the report follow.	*Thank you*
	2	Title - suggest removing "and Central" from title. Model domain only includes part of Central Puget Sound, and focus of study and calibration is South Sound.	*The report name reflects the domain used in this assessment and also recognizes the connectivity of South and Central Puget Sound.*
	3	pg 8 - states that a 35 layer model was evaluated, but no summary of the model differences is included. What differences were observed between the models with different vertical resolutions?	*We limited the comparison between 17- and 35-layer models to predictions in water surface elevations (slight improvement with 35 layers), and model runtimes (the runtime for the 35-layer model was 3.5 times that of the 17-layer model). Language included to reflect this.*
	4	pg 17 - it would be more appropriate to cite the NDBC web site than a per. comm. from Nairn	*We agree*
	5	pg 32 Tidal Constituents - the model appears to agree well with the published values, however it is worth a short description of why you presented the comparison for a subset of the data that is available (Lavelle et al have additional stations in South Sound). Perhaps you did compare with the other stations and could add this to the text?	*We compared with Budd Inlet and Oakland Bay in South Sound because they represent areas furthest from the open boundary where errors would accumulate. Model performance was reasonable, and we did not compare additional stations.*
	6	pg 36 - Minimum near surface layer thickness: (a) this deserves an explanation as to why the model becomes unstable. (b) Given that the model is limited to a 3 to 4 meter vertical resolution at the surface, a careful evaluation needs to be conducted on the suitability of this model configuration for its intended use of predicted water quality. The photic zone can be less than 4 meters, so the ability of the model to predict biological activity becomes questionable with this coarse vertical resolution.	*The thickness of the upper layer was required to be 4 meters for numerical stability. The GEMSS model requires that the upper layer thickness cannot be less than the tidal variation across the model boundaries. The ability of the model to predict biological activity and water quality was demonstrated in the water quality modeling report.*

<table>
<tr><td></td><td>7</td><td>pg 54 and subsequent Salinity plots - Range of the salinity scale is larger than necessary, makes comparison of data to model predictions difficult.</td><td>The range of salinity scale for the new time-depth plots were reduced to between 20 and 35 ppt. However, the time-series plot scale of 20 to 40 ppt was retained since the new plots now included both the surface and bottom layers, RMSE, and bias information. Reducing the upper scale from 40 to 35 ppt will reduce the space available for the additional information included in the plots.</td></tr>
<tr><td></td><td>8</td><td>pg 95 Current Velocities - Report compared model and ADCP velocities primarily to examine tidal flows. It would be useful to present comparisons of the tidally-averaged (or net) flows, particularly since the model will be used for the long-term transport of constituents. Cross-channel transects are a very useful method of presenting this data to compare how the model is capturing the vertical and cross channel structure of the net transport.</td><td rowspan="2">We agree that estimating long term average flows across a transect and comparing to literature values is important in demonstrating how well the model represents circulation. Flows across seven transects (Edmonds, Olalla Point in Colvos passage, Three Tree and Dash Points in East Passage, Tacoma Narrows, Devils Point and Gordon Point) were estimated from model output and compared to literature values.</td></tr>
<tr><td></td><td>9</td><td>pg 95 Current Velocities - Comparison could be made with historical estimates of volume transport in Colvos Passage (Ebbesmeyer et al, 1984) and East Passage (Bretschneider et al, 1985) to provide an indication of how well the model was simulating the flow around Vashon Island. This would be extremely useful to illustrate the model's ability to accurately represent the deep water inflow. This is important because the ocean contribution of nutrients will be an important component to address in the water quality study.</td></tr>
</table>

	10	pg 110 Sensitivity Analysis - Additional explanation of this sensitivity analysis is needed. It is unclear why the changes in the boundary values are not propagated into the model domain. Was the delta added to each month or just one? At what point was the comparison made?	*Because the open boundary data were available on a monthly time scale, we wanted to find out the impacts within the model domain of intra-month variability in salinity and temperature at the open boundary. The addition and subtraction of 2 °C temperature and 1 ppt salinity was done for all months of simulation. Comparisons were made at calibration stations of predicted temperature and salinity time series. The fact that the changes at open boundary did not propagate into the model domain showed that the monthly data at open boundary were sufficient for the purpose of this project and any slight intra-month variability was not significant.*
	11	pg 115 Simulated Dye Releases - The steady state concentrations predicted by the model will be influenced by the amount of net transport across the open boundary, particularly for releases in the Central Sound. Often open boundary conditions underestimate exchange transport across the boundary, so these predictions should be used with caution. Comparison of the steady-state residence times with Ecology's Box Model would provide a useful indication of model performance in this regard.	*Residence times from Ecology's box model would not be directly comparable to residence times computed for grid cell locations in the GEMSS model because of the very large difference in spatial resolution. Comparison of long-term average residual flows is more relevant with regard to net transport across various transects such as the open boundary or the Narrows.*
Heather Trim (People for Puget Sound/ Futurewise)	1	We decided that we will not be sending in comments on this document. Basically it is looking good and since we are not experts in this specific arena (circulation model), we will defer to the experts	*None needed*

Tony Paulson (USGS)	1	The scale on calibration figures (pp. 34-45) should span only the range of reasonable values for salinity and temperature. By having large scales on the y-axis, especially salinity values up to 40, the figures distort the differences between the model and observed results.	*The range of salinity scale for the new time-depth plots for salinity was reduced to between 20 and 35 ppt while that of temperature was retained at 0 to 20 °C. The time-series plot scale of 20 to 40 ppt (for salinity) and 0 to 20 °C (for temperature) was retained since the new plots now included both the surface and bottom layers, RMSE, and bias information. Reducing the upper scale from 40 to 35 ppt (for salinity) or increasing the lower bound from 0 to 5 °C (for temperature) will reduce the space available for the additional information included in the plots.*
	2	For the residence time section between pp 110-114, clarify that the dye injection is a model simulation and a model result, not a calibration between model and field result. The caption on the figure on p. 86 is particularly misleading.	*Agree*
	3	The model temperature in Oakland Bay is greater than the actual temperature in Oakland Bay in August, indicating the model is overestimating the absorption of heat by marine water. Perhaps, the light extinction coefficient used in the model is not accurate. Perhaps, the model results from equations can be compared to the PAR instrument on the CTD, if it has one. For qualitative evaluation, the Secchi dish depth can be examined in Oakland Bay compared to more open waters.	*The discrepancy between prediction and observed temperature may be attributed to two reasons. First the model grid layout was unable to capture the two right turns in Hammersley Inlet next to Oakland Bay and secondly the shallow depths in Oakland Bay were represented by only 2 vertical layers. The first reason caused a slight phase shift in tidal front but not in tidal elevation, the second likely contributes to numerical dispersion. These factors together maybe responsible for the discrepancy. The parameters for light extinction coefficients and Secchi transparency used in the model were derived from observed light attenuation and Secchi transparency data from South Puget Sound and specified as model inputs, and are therefore representative of the actual conditions in South Puget Sound.*

	4	The stratification is a primary control on phytoplankton growth. In general, the agreement of model and actual temperature in main basin of Puget Sound is better than the agreement in South Puget Sound. In the main basin, the temperature profiles are probably driven more by transport than by thermal source term. In the South Puget Sound, there are numerous instances when stratification is not accurately modeled. Perhaps, the vertical resolution needs to be increased in South Puget Sound. The inability of the model to more accurately model the stratification may also be a result of numerical dispersion inherent in the model. If at all possible, advanced techniques in modeling code to minimize numerical dispersion might be employed.	*Agreed, but we are limited by the model. The overall RMSE for temperature and salinity is reasonable with no statistical significant bias. Also the follow-up WQ model predictions are reasonable as well.*
	5	While South Puget Sound is primarily driven by tidal dispersion in the inlets, estuarine circulation may play an important role in water quality modeling. A more sensitive method of evaluating the estuarine circulation is to assess the net advective flow using the ADCP data. In this method, the vector data at each depth (bin) is transformed into the primary components of longitudinal (along shore) and latitudinal (cross channel) flows. The net flow is then simply the mean flow for the duration of the record in the longitudinal direction. I believe analysis of the cross channel flow and it timing prove information about tilting of the surface and bottom layers. The statistical significance of the net flow is more difficult to evaluation as shown by Marlene Noble of USGS in Hood Canal, http://pubs.er.usgs.gov/usgspubs/ofr/ofr20061001 An example of low pass filtering and record mean vertical profiles are shown in figs. 10 and 11 of http://pubs.usgs.gov/sir/2006/5106/ I understand that because of the orientation of the numerical model cells and the horizontal resolution in irregular channels, these analyses of the ADCP record probably would not be useful for model verification. However, a better understanding of the net advection is required to understand the impact of interactions of nutrient loads and circulation patterns on nutrient concentrations.	*Table 8 "Summary of surface-mounted ADCP - measured tidal fluxes versus model results" presents data comparing ADCP measured and model predicted cross-sectional area and average velocity across a transect. Errors remain, as alluded to in the comment, both in ADCP data (data did not capture bottom and nearshore areas due to instrument and navigation limitations) and in model predictions (model grid-cell orientation was somewhat different than the ADCP transect and no attempts were made to correct for this discrepancy). Agree that a better understanding of net advection is necessary to understand interactions of nutrient loads and circulation pattern. Please see response to comment numbers 8 and 9 from Bruce Nairn for additional evaluation of circulation patterns and how it correlates with published values*

Ben Cope (EPA)	1	This is an excellent report. The Ecology team has put together a thorough inventory of information and well-written descriptions of the analytical process used in the construction of the draft circulation model. The graphics work is particularly good, conveying the relevant information in a straightforward fashion. It is clear from this report that care was taken at each stage in the model development process to improve the corroboration between measurements and model predictions. The information presented in the document supports Ecology's conclusion that the model is generally capturing the varied hydrodynamic characteristics within South and Central Puget Sound. This will enable to team to move forward to the water quality components of the model. My detailed comments fall in the category of minor suggestions to improve clarity. I also highlight some of the noteworthy features of this report.	*Thank you*
	2	Pg xi - ES-3. Suggest labeling plot with location (e.g., surface or bottom) of salinity predictions.	*Thanks for your suggestion. Language added to Figure caption to clarify planview maps were for surface predictions.*
	3	Pg xiv - Suggest adding a couple labels to identify areas of interest.	*Thanks for your suggestion. Some labels included.*
	4	Pg xiv - Suggest adding that all rivers/sources released the same concentration of dye.	*Thanks for your suggestion. Language added.*
	5	Pg 13 - "Daily flows were estimated based on the ratios of watershed area and mean precipitation." This was done only for ungaged tribs or areas downstream of gages?	We extrapolated from gaged locations to all ungaged areas, including both downstream from the gaging location and adjacent shoreline areas.

	6	Pg 18 - Suggest a table to summarize weather data location selection (parameter/location), in addition to the existing language	A summary of sources of meteorological data is a good suggestion since it provides clarification on which station the data was obtained from and for which region. A table with the desired information is included in the meteorology section.
	7	pg 26 - Good, thorough description of the painstaking process of grid adjustment as part of calibration.	*Thank you*
	8	Editorial. There are frequent statements that certain parameters are simulated "well". The core of the report can simply report the process and direct the reviewer to figures, which speak for themselves. Ecology's view of the performance of the model could be handled in a short section at the beginning or end of the document. That section can include Ecology's judgment that the circulation model performs adequately to support the pending water quality component.	*A section on error statistics has been included that includes a single plot of all observed and predicted temperatures and salinities at all stations. The plot includes a 1:1 line and overall RMSE to show how well the model predicts observed data. A histogram plot is also included that shows the distribution of errors and the associated bias of the predictions. This section also includes citation of another study that had comparable error statistic.*
	9	Pg 26 - "Model predictions are closer to PSTides-generated water surface elevations, partly because PSTides data were used to force the model and partly because the comparison was conducted with the wind turned off." Why was it turned off for this comparison and was it turned off for others?	*The wind was turned off because PSTide predictions did not have a wind component. However, the wind was turned on when comparing surface elevation predictions by the model to those from NOAA real-time observed surface elevations at Tacoma and Seattle. Wind was turned On for all followup model simulations.*
	10	Pg 35 - Was a uniform friction coefficient used? This should be noted.	*A constant friction factor of 40 was used for South and Central Puget Sound region except for the finger inlets (Budd, Eld, Totten, Henderson, and Oakland) where the friction factor was 20. These coefficients were kept constant throughout the simulation period.*
	11	Pg 36 - What is the layer thickness of the model near the surface as currently setup? It sounds like 3 doesn't work, and 4 doesn't either, and then the language trails off..	*The surface layer thickness used was 4 meters. Language clarified to explain this.*

	12	Pg 37 - The second paragraph is unclear - it appears that a shortcut was used to simplify model/measurement comparisons in order to reduce model output processing.	*The model calibration process was limited to 24 stations that were carefully selected to span the model domain. The number of stations reduced the time for post processing of model output. However, error statistic was based on observed and predicted data at all stations. The error statistic is included in the final report.*
	13	Pg 38 - These are well-crafted plot configurations (here and similar plots elsewhere) to compare measurement/model patterns.	Thank you
	14	Pg 100 - Good figures comparing model cell depth vs ADCP depth, and companion discussion	Thank you
	15	Pg 105 - Suggest adding a plot from Tide Prints to the model plots, since text indicates a comparison was made. Text should note comparison was qualitative only (or semi-quantitative).	*We added the corresponding tide prints as a new Appendix C.*
	16	Pg 111 - Excellent discussion of different ways to evaluate flushing time.	Thank you
Bill Fox (Cosmo-politan Eng)	1	Overall this is an impressive hydrodynamic model development, both the code selected and the data collected for calibration.	*Thank you*
	2	It appears Ecology did a good job of quantifying surface water and WWTP sources.	*Thanks*
	3	The mock tracer runs were very useful and revealing. I concur with the conclusion that central sound sources can not be excluded based on the tracer simulation, and that the water quality component that includes all transformations will be necessary to determine if nutrient loading from central sound sources.	*Thanks*

	4	For future hydrodynamic and water quality simulations, WWTP sources should be entered at their initial dilution trapping level, not at the bottom as was done in the tracer simulations. I'm not sure how significant the difference will be, and it will take some effort to obtain all that information, but it at least needs to be tested. I have performed many of the mixing zone studies in Puget Sound, so please let me know if I can help put that information together.	*Agree. The updated report includes simulated dye studies where the wastewater discharges are aligned at the trapping levels.*
	5	I noticed there was no mention or citation of the transport and refluxing modeling conducted by Ned Cokelet and others in the 1980s. Ned was an oceanographer at NOAA PMEL at Sand Point who developed the first empirical box model for Puget Sound based on conservation of mass and salinity. It may well be that the current box models referenced in the report are based on the same procedures and data that PMEL used, but I don't have those references to know for sure (if the current box models are extensions of the PMEL water and salt budget work, this comment is probably null and void).	*We added information from Cokelet et al., 1990; Ebbesmeyer at al. 1984; and Cannon and Ebbesmeyer, 1978. See also responses to comments received on the corollary water quality report published in March 2014.*

	6	PMEL also used their Puget Sound box model to simulate tracers discharged at several locations in the central basin, much as you did in your reported tracer simulations with your model. I would be VERY interested in how your tracer model results compare to the simulations performed by Ned Cokelet. If the results are similar, as I expect, this would greatly bolster the reputation of your model. Conversely, if there are significant differences, the reasons would need to be investigated. Here are two of the PMEL references, which I expect Skip would have or be familiar with. If not, I can certainly scan the papers and email them to you: Cokelet, E.D., R.J. Stewart and C.C. Ebbesmeyer. The Exchange of Water in Fjords: a Simple Model of Two-Layer Advective Reaches Separated by Mixing Zones. In ASCE 19th Coastal Engineering Conference Proceedings, September, 1984, Houston, TX Cokelet, E.D., R.J. Stewart and C.C. Ebbesmeyer. The Annual Mean Transport and Refluxing in Puget Sound. In Proceedings of the First Annual Meeting on Puget Sound Research, Vol. 1, 108-119, Puget Sound Water Quality Authority, 1988, Seattle, WA.	*We do not have a direct comparison with the Cokelet tracer results based on the box model. We do not evaluate results at the same spatial scale as Cokelet et al. (1988). If we found differences, we would not be able to rule out numerical dispersion from the larger boxes used. See also responses to comments received on the corollary water quality report published in March 2014.*
Phil Crawford (U.S. Army, Fort Lewis)	1	Sorry I can't give you some more useful comments, but I will say this: very impressive work. Although I'm in the environmental biz now, I started my career as an oceanographer, and later skippered the UW research vessels HoH, Onar, and Clifford A Barnes, so I have an idea of the effort that went into this. Can't believe you got that model calibrated so well, in such a complex system. It's a long way from injecting ink into a big wood and plaster model in the basement of the UW Hydrology building. Kudos to the team!	*Thank you*
Lincoln Loehr (Stoel Rives)	1	I don't have any comments.	*Thank you*