September 14, 2012

MEMORANDUM

Subject: EPA and Ecology Response to the Independent Review Report

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To: All Interested Parties

Recognizing the importance, complexity, and interdisciplinary aspects of the scientific questions around estimation of human impacts to dissolved oxygen in Hood Canal, EPA and Ecology requested that the Puget Sound Institute conduct an independent review of our draft report by a panel of experts in early 2012. The panel review was guided by specific charge questions that focused on differences in methodology or interpretation among the researchers. The panel identified a number of important issues in the analyses to date, and we respond to their comments in this memorandum.

Summary

1. We are pleased that the report includes the main characteristics of interest. We concur with the panel's concerns about spatial and temporal scales of analysis in the context of the

regulatory landscape, but EPA and Ecology are responsible for interpretation of the regulations. See below (Question 1) for further discussion.

2. It is not clear from the detailed comments in the review which contributions from the watershed nitrogen loads are underestimated. We assume that the main issue is the presentation of median groundwater nitrogen concentration rather than the mean concentration in the point estimate tables. Based on this concern, we revised the report to include both values in the tables to highlight the potential range of loadings. We note that the Monte Carlo analysis samples the full distribution of concentration, so we do not perceive any underestimation due to methodology used for the final estimate of nitrogen loading and uncertainty.
3. We concur with the panel that the simple salt balance is preferred in the aggregated model analyses and that a re-analysis of marine nitrogen fluxes is warranted. We also note that the ROMS model provides additional information on circulation and fluxes.
4. We concur with the panel that uncertainty is substantial in the nitrogen-algae-oxygen linkage. Quantification of biogeochemical conditions in the environment is inherently complex and uncertain. We have added a quote from the panel on this point in the uncertainty section of the report.
5. We concur with the panel that the ORCA data could be better exploited to analyze variability and estimate uncertainty.
6. We concur with the panel that it is unlikely that human activities are decreasing dissolved oxygen concentrations by >0.2 mg/L in the mainstem of Hood Canal.
7. We concur with the panel that there is not strong evidence that human activities have an impact greater than 0.2 mg/L in Lynch Cove, and we recognize the challenge of analyzing small impacts in highly variable systems. However, the water quality standards compel Ecology to evaluate the potential for impacts at this magnitude. EPA and Ecology have successfully developed models, identified and communicated uncertainties, and made decisions on implementation of standards that require minimization of human impact.

Question 1a

The panel's comments on Question 1a highlight an important issue in any evaluation of water quality impacts: selection of the spatial and temporal scales of analysis. A coarse scale of analysis was used by University of Washington researchers in their Lynch Cove impact assessments. Both simple models and large-scale analyses are useful, particularly in the early stages of an assessment. At the same time, we agree with the panel that implicit in coarse-scale analysis are significant limitations and uncertainties. We agree with the panel that additional analysis of Lynch Cove at smaller temporal and spatial scales would likely reduce uncertainty in the estimates of human impact and questions regarding compliance with water quality standards.

Question 1b

This finding by the panel supports the conclusions in our draft report that human activities are not decreasing dissolved oxygen concentrations by >0.2 mg/L in the mainstem of Hood Canal.

Question 1c

We acknowledge the difficulty in quantifying human impacts in this system and the uncertainty inherent in modeling analysis (particularly coarse-scale analysis). EPA and Ecology are attempting to identify the most plausible estimates from the available body of information. We concur with the panel that some of the available information could be substantially improved through re-analysis (e.g., HCDOP marine flux estimates) and subsequent refinement of the EPA/Ecology Monte Carlo analysis. The Monte Carlo analysis provides decisionmakers and the public with information on the uncertainty of the estimates. We refined the assumptions of the Monte Carlo analysis in response to comments from the independent panel.

Question 1d

We concur that the buoy data should have been better exploited in the available analyses to evaluate variability and uncertainty. While EPA and Ecology are not in a position to conduct a thorough analysis of the buoy observations, we have revised the report to identify an interesting discrepancy between measured current speeds at Twanoh (reported in Devol et al. (2011a)) and the current speeds back-calculated from the salt balance.

We also agree that a differential impact (0.2 mg/L due to humans) cannot be detected because we lack a monitored natural condition state without the influence of human activities. We can account for natural variability by using mathematical models to assess the likelihood that impacts above this level are occurring. We agree with the panel that a refined 3-dimensional model of Lynch Cove may narrow the uncertainties.

Question 1e

We concur that the system is complex and additional analyses could be conducted to reduce uncertainty.

We appreciate the specific recommendations of this panel for improvement of the impact assessment.

Question 2a

The independent review missed several key steps in the analyses completed and summarized in the EPA/Ecology science review. The reviewers offer alternative approaches, but the purpose of the science summary is to summarize the information currently available.

Lines 178-181

The revised report includes dry deposition, which is very small compared to other sources and also very small compared to dry deposition rates in other regions of the U.S. where the reviewer works. We hypothesize that the low population upwind of Hood Canal is the likely reason for low deposition compared to other regions of the country. See the National Atmospheric Deposition Program isopleths for these regional patterns. In addition, the watershed analysis captures deposition to land in the tributary load estimates (see further discussion on this point below).

Lines 182-183

The revised report clarifies the information used for historical and current estimates.

Lines 184-191

We will include both median and mean values in the final report. However, we disagree that high nutrient seeps were ignored. The entire distribution was directly sampled in the Monte Carlo estimations, which is discussed below (Recommendation #5, Line 244).

Lines 192-195

HCDOP reports do not include this level of detailed analysis of land use. "Background" means natural conditions. See responses to comments in lines 229-239 for additional clarification on the role of land cover in the load estimates.

Lines 196-204

We agree that consistency in use of nitrogen parameters is important. However, complete consistency is not possible, because we summarized estimates of researchers who analyzed different parameters. The EPA/Ecology science summary focuses on dissolved inorganic nitrogen (DIN = nitrate + nitrite + ammonium) and/or total dissolved nitrogen (DIN + DON) based on the parameters used in the primary documents. Regional analyses (Mohamedali et al., 2011a; Mohamedali et al., 2011b) also suggest that dissolved organic nitrogen is generally negligible and within the laboratory analytical error, which may be a different pattern than other regions of the country.

Lines 207-209

The revised report describes both general human impact, a term that arises from the water quality standards, as well as specific human activities that may contribute nitrogen.

Lines 210-225

We disagree with several statements in this section. The panel did not understand that the total nitrogen loadings from tributaries to Hood Canal/Lynch Cove are directly calculated using extensive measurements of flow and concentration at the tributary mouths. The statistical correlations only relate to the estimated of source contributions to the loadings observed at the mouths. We updated the report to clarify the fundamental role that monitoring data played in the total load estimates and the subsequent attribution to different sources using statistical analyses. We agree that the uncertainty in these estimates is relatively high (i.e., low R squared values), but we do not agree that the uncertainty is high in the total loadings entering Hood Canal.

We also disagree with the panel view that the use of a statistical model to estimate the proportional source contribution is problematic. Statistical models are an accepted tool for identifying contributing factors to an observed phenomenon. Other methods can be used, such as a mathematical (process) model of the watershed as recommended by the panel, and it is possible that they will provide estimates with lower uncertainty. This type of analysis is not currently available, and the results from the statistical model (Steinberg et al. 2010) should be considered as valid estimates.

<u>List of needed clarifications (lines 226-252)</u>

1. Comment noted. EPA and Ecology have no additional information beyond the Steinberg et al. (2010) analysis.
2. We agree that red alder forest can increase stream nutrient concentrations. The science summary cites other research along with the Steinberg et al. (2010) analysis, which quantified the relative contribution.
3. The panel did not recognize that tributary delivery loadings are derived from direct measurement. There is no assumption that "all fixed N reaches the estuaries" in the Steinberg et al (2010) assessment. The measured loading at the mouth, after loss within the watershed, is the loading that reaches the estuary. The source contribution from red alder to that mouth loading is estimated using statistical correlation.
4. The panel is incorrect in asserting that the analysis does not consider all OSS in the watershed. Upland OSS discharging to tributaries are accounted for in the measured tributary loadings, which are attributed to specific sources, including residential contributions. Therefore, a per capita approach with a buffer width that includes all OSS in the watershed would clearly double-count much of the upland OSS loading to Hood Canal. See Figure 17 in the EPA/Ecology report.
5. The Monte Carlo analysis in Question 3 relies on distributions for all parameters introduced in Question 1 for context. We recognize that the report first introduced "best estimates" without conveying that the final DO impact calculations use the full distribution. We revised the report to introduce the Monte Carlo analysis earlier. The new Table 4 provides lower and upper bound estimates, and the accompanying language points the reader to the Monte Carlo discussion and assumptions.
6. See #3 above.

7. We agree that tributary sampling does not capture all watershed loading, but the tributary analyses included extrapolations of sampled loadings to unsampled portions of the watershed (see Steinberg et al. (2010). These extrapolations included estimates of the contribution of shoreline OSS loadings.

Lines 253-256

The panel's comments have led to improvements in the information presented in tables and better documentation of the upper range in potential loadings from shoreline OSS.

Question 2b

Line 265
This question relates to shoreline OSS, not all OSS in the watershed.

Lines 269-274
We note the panel's general acceptance of the groundwater flow estimate by USGS. The basis for the uncertainty distribution was cited as Sheibley (personal communication) because a report was not available. Since the panel issued its report, we have removed the Sheibley range from the Monte Carlo analysis, because supporting calculations are not available. In addition, we have added a surrogate groundwater estimate by Brett (2011d) that did include supporting calculations. Since we do not have a basis to select between the estimates of Brett (2011d) and Paulson et al. (2006), we have assumed a uniform distribution in the Monte Carlo analysis.

Lines 275-281
The Mason County sampling of seeps included all seeps encountered on shoreline surveys; it was not a random sampling program, as described in the independent panel report.

We considered including brackish samples in the analysis by back-calculating the DIN in the freshwater fraction of the sample using the salinity data. However, the back-calculation requires an assumption about the DIN of marine water infiltrating the groundwater. As noted in the report, the DIN in marine waters of Hood Canal ranges from 0 to 400 µg/L depending on the depth and amount of drawdown from phytoplankton uptake. Arbitrary selection of a DIN value across this range would significantly affect the resulting estimate of DIN in shoreline (fresh) groundwater. We decided that, given that we have 325 non-brackish samples, there was limited benefit to increasing the number of samples in the analysis by adding this layer of complexity and uncertainty.

Lines 286-289
We interpret this comment as a concurrence with our use of seep samples to estimate shoreline OSS loading, because seeps represent the shoreline groundwater concentration and subsequent delivery to Hood Canal surface waters. USGS (Paulson et al., 2006) used well data further from the shoreline to derive some of its groundwater loading estimates prior to the availability of shoreline seep data.

Lines 296-299

We agree that OSS loading to the estuary via groundwater transport likely varies by location and season. This was noted in the draft report (see page 36 of the February 2012 independent review version)., but no quantitative information exists.

Lines 300-304

We use both the measurement-based and the per capita approach to estimate loads, and we will continue these as two separate lines of evidence. We do not agree with the panel's assumption that measurement-based loadings are biased (under-estimates) because all seeps were sampled and the program was not random sampling. We would argue that loading estimates could move either up or down based on inclusion of brackish samples. Similarly, seep measurements may miss low nitrogen plumes, not just high nitrogen plumes. See previous responses on distribution representation in Table 4 in lines 254-257.

Flow volumes associated with failing septic systems are likely low in comparison to regional groundwater. Preferential flow paths due to groundwater hydrogeology likely produce higher flows and lower concentrations indicative of regional groundwater compared with seeps of effluent from failing OSS. Therefore, the mean is not necessarily more indicative of total loading without concomitant spatially varying groundwater flows and would bias load estimates high. Nevertheless, we revised the report to include both mean and median values for nitrogen in groundwater seeps. The Monte Carlo analysis has the effect of weighting the median value over the mean value.

Lines 306-314

The focus of this question is shoreline OSS loadings, since other OSS are included in the tributary loads based on stream measurements. A new watershed model is not guaranteed to improve upon the per capita and measurement approaches described in our report, in part because it would add model error to the tributary estimates.

Lines 315-317 and 323-327

Because large buffers double count the sources already incorporated in the tributary estimates based on monitoring at the mouths, this is not a protective option but a clear overestimate of likely contributions. However, ensuring the population encompasses the entire shoreline fringe area that is outside the tributary catchments would provide an appropriate estimate for the shoreline OSS.

Lines 318-322

This comment notes that the full distribution is used for the Monte Carlo analysis, which is the basis for the DO impact calculations in Question 3. See earlier response to the recommendation to analyze brackish samples (lines 275-281).

Lines 328-333

We agree that changes to input values will change the Monte Carlo results. We provided a discussion of the selection of distributions to clarify this issue.

Lines 336-353

We also added discussion on seasonal variation and time scales of analysis. We agree that, to the extent feasible, it is important to refine/validate all of the estimates that comprise the inputs to the Monte Carlo estimation.

Critical/recommended analyses, Lines 356-385
We have responded to these recommendations above. While we agree with some points, we disagree with the suggestions to include all septic systems in the watershed analysis because the tributary loadings already capture the loading from upland septic systems.

Question 3a

We concur with the panel's recommendation to only consider the salt balance approach to estimate the marine nitrogen loading flux and to consider tidally-driven dispersion in the total flux calculation. We also concur with the specific panel recommendations for more thorough analysis and documentation of pycnocline depth and near-surface salinity extrapolation, as well as more thorough exploitation of available buoy data to analyze variability and estimate uncertainty. We have included a discussion of the panel's findings with respect to marine flux in the body of our report.

We cannot compel the researchers to conduct the reanalysis. Therefore, in our review of existing information, we identified the need for this reanalysis but were unable to generate the results of that reanalysis. These issues also directly affect the level of confidence in the DO impact estimates for Lynch Cove, as described in our response to Question 4a.

We added a recommendations section that includes several analyses identified by the panel. The recommendations will provide HCCC and other options for additional analysis of DO impacts in Lynch Cove in the coming years. This could involve reanalysis of the aggregated model inputs and/or additional 3D modeling using the ROMS model or other available tools.

Question 3b

Lines 445-467
The panel raises several important issues on the marine flux analysis. The panel also provided a figure to support its assertions that some of the methodology used by researchers to date is problematic. All of the points raised should be examined in detail in the future.

The main body of the independent review report does not fully answer a key aspect to question 3b. While a full answer to Question 3B is clouded by the issues and lack of clarity in the marine nitrogen flux methodology, the question also points to an important biogeochemical sub-question raised in the EPA/Ecology report. Assuming the vertical nitrogen flux methodology is corrected with a 2-layer Knudsen analysis as recommended earlier by the reviewers, the next question is this: At what depth in the water column should the vertical flux be estimated for use in the proportional DO impact equation (see Figure 18 and pages 46-47 for differing perspectives and assumptions)? Devol et al (2011)

estimates the flux to the pycnocline depth (which is in the middle of the euphotic zone). This vertical flux is reduced by algae uptake below that depth. Brett (2011) estimates the total flux to the euphotic zone, which extends deeper than the pycnocline. We outline the reasons for this difference in approach on pages 46-47. The question for reviewers is: What choice is preferred for the purpose of estimating the DO impact with the proportional calculation?

We asked the reviewers to provide follow-up recommendations on this issue, and their responses are included in Appendix A of the independent review report. In general, the independent reviewers recommended use of a consistent 2-layer model construct for all calculations related to the marine nitrogen flux rather than the combination of 2- and 3-layer models used by Devol et al. (2011a). However, the panel did not articulate specific concerns about the validity of the oxygen balance assumptions of the 3-layer model. The primary concern of the panel was the proper estimation of the marine nitrogen fluxes into Lynch Cove, which had fundamental limitations flagged as issues of greater importance than questions related to aggregated model formulation.

Lines 464-467

We agree that horizontal advection below the pycnocline is an important component missed in the analyses to date, if the marine flux is calculated at a depth below the pycnocline.

Question 3c

We concur with this response.

Critical analyses, Lines 483-501

We concur that all the recommended analyses are warranted. The revised report documents the concerns of the independent review panel and caveats all human impact estimates accordingly. Because the EPA/Ecology document is a review of available information, the issues raised by the panel will not be resolved in the report.

Question 4a

The main body of the panel report does not provide a clear answer to question 4a, although the information presented is relevant to Question 3B and factors related to human impacts on Lynch Cove DO. Question 4a requests a recommendation on the method to estimate the total DO deficit in Lynch Cove from the buoy data, particularly whether the DO of the full water column at Hoodsport should be used, or only values at depths less than the depth of Lynch Cove (see page 59 of the report for discussion). The human contribution to the DO deficit is a portion of this total DO deficit.

Lines 625-633 accurately describe two approaches proposed by different researchers for evaluating total DO deficit. Lines 631-633 suggest the 2-layer model by Brett (2010) is preferred over the 3-layer model

by Devol et al. (2011a) but does not respond regarding the two ways of calculating total DO deficit using buoy data proposed by Brett (2010) and Devol et al. (2011a).

We asked the reviewers to provide follow-up recommendations on this issue, and their responses are included in Appendix A of the independent review report. In general, the independent reviewers recommended that the deficit calculation focus on oxygen conditions along similar densities (termed "isopycnal lines") between Hoodsport and Twanoh. Devol (pers. comm., 2011d) noted that the isopycnal lines are horizontal between Hoodsport and Twanoh for most of the summer, and therefore common depths between the two locations should be used. This density pattern would call for exclusion of dissolved oxygen data from 30 meters to the bottom in Hoodsport from the comparison to Twanoh concentrations. This general approach was taken by Devol et al. (2011a).

The panel also references findings in Newton et al. (2012). This document was released by HCDOP after our science summary was sent to reviewers, and it was shared with the reviewers by the Puget Sound Institute at their opening workshop. While this information was not previously available to EPA and Ecology, we appreciate the panel's comments regarding how that information fits into the larger science summary.

Lines 518-612

We concur with all of the panel's points regarding uncertainties and data limitations. We have included a quote from panel report (lines 531 to 541) in the introduction to the uncertainty section of our report.

At line 556 and later at line 609, the panel incorrectly attributes an estimate of 13% human contribution to the total nitrogen flux from Devol et al. (2011a), with about half this contribution (6%) originating from shoreline OSS. These values are included in Table 10 of the EPA/Ecology report and we cite Devol et al. (2011a) as support for the marine flux estimates, but the listed human contribution fractions are based on the reduced shoreline OSS estimate from our own analysis. Using higher shoreline OSS estimates from Richey et al. (2010), Devol et al. (2011a) estimated a human contribution of 15-37% and a shoreline OSS contribution of 11-27%. See Table 9 of the EPA/Ecology report for values reported in Devol et al. (2011a). As noted in the EPA/Ecology report, the large buffer used by Richey et al. (2010) double-counts septic systems that are already part of the tributary estimates, so we argue against adding those values to the tributary loads (and human fraction) to estimate the total human nitrogen loading.

The panel also cites Richey et al. (2010) as a source for OSS contributions of nitrogen to the mainstem Hood Canal (line 607). To our knowledge, Richey et al. (2010) did not estimate this endpoint (fractional contribution of OSS to nitrogen loadings in mainstem Hood Canal).

These clarifications are offered not to agree or disagree with the remainder of the panel's comments on the link between nitrogen and hypoxia. We concur with the panel recommendation to explore other data sources. However, given that the EPA/Ecology document is a review of available information, we will be unable to resolve this in the final report.

Lines 611-619

We agree that the signal-to-noise ratio is low, and we concur on the complicated relationships between nitrogen and oxygen. However, even if the relative contribution of humans is low, the stringent water quality standard for dissolved oxygen in Hood Canal/Lynch Cove essentially requires the agencies to insure that the pollution signal remains very low as development proceeds in the watershed. This goal makes sense in an area of natural hypoxia like Hood Canal.

Lines 625-698

We concur with all comments regarding uncertainty and recommended future work.

Lines 643-4

We concur with the assessment of the subsurface seaward outflow and that the evidence that low DO water from Lynch Cove makes a contribution to fish kills at Hoodsport is weak.

Question 5

We agree with the comments and recommendations of the panel. With the available information, it is not possible to objectively assess the relative merit of the aggregated model and ROMS model results.

We also agree with the recommendation to further develop and apply the ROMS 3-dimensional dynamic model, particularly in Lynch Cove. We would note that other models may also offer opportunities for improved 3D modeling. A refined sub-model of Lynch Cove, with boundary inputs from the large scale model simulations of mainstem Hood Canal, would provide a better understanding of impacts in Lynch Cove.

References

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Mohamedali, T., M. Roberts, B. Sackmann, and A. Kolosseus. 2011b. Puget Sound Dissolved Oxygen Model Nutrient Load Summary for 1999-2008. Washington State Department of Ecology Publication No. 11-03-057. fortress.wa.gov/ecy/publications/SummaryPages/1103057.html