EPA and Ecology Responses to Stakeholder Comments

EPA and Ecology, in coordination with the Hood Canal Coordinating Council, released a draft report for public review and comment on September 17, 2012. The comment period extended until November 29. Comments varied in terms of format and level of detail, including general comment letters, detailed technical comments, and mark-up comments in the draft report. Based on comments received, we have made a number of changes to the report.

Comments were received from the following governments, organizations, and researchers:

Port Gamble S'Kallam Tribe
Washington Department of Natural Resources
Mason County Health Department
Kitsap Public Health District
Lower Hood Canal Watershed Coalition
Allan Devol, University of Washington
Michael Brett, University of Washington
Jan Newton, University of Washington
John Mickett, University of Washington

The responses to comments are organized into the following topic areas:

General Comments
Peer Review Process
Shoreline OSS and Groundwater
Marine Nitrogen Flux Estimates and Influence of Dissolved Oxygen
Sediment Core Information
Subsurface Seaward Outflow
Additional Analysis Needed
Policy Implications
Miscellaneous Topics
Other Information Received During the Comment Period

General Comments

Comment from Washington Department of Natural Resources (DNR):
DNR considers the protection and restoration of Puget Sound to be a fundamental part of its responsibility as manager and Steward of state-owned aquatic lands. To achieve protection and restoration, scientific information is needed describing the condition of aquatic lands and the key stressors affecting habitat health. We appreciate the contribution that the draft report makes toward better understanding of ecosystem condition, and also provide recommendations for its improvement.

Response:
Comment noted.

Comment from Washington Department of Natural Resources (DNR):

The draft effectively synthesizes a substantial amount of scientific research. The scope of the report is limited to assessing the impact of human-derived nitrogen on Hood Canal dissolved oxygen, including the events that lead to widespread fish kills. Our main overarching concern is that findings of "no effect'[1] regarding human-derived nitrogen will be interpreted to apply beyond the scope of the report. This would be detrimental to the fundamental ecosystem management goals in Hood Canal. We strongly recommend that the report and memorandum clearly acknowledge the limited scope of the report. Specifically, the report should caution that the findings related to dissolved oxygen should not be considered a comprehensive assessment of the impacts of human-derived nitrogen on the Hood Canal marine ecosystem and the importance of nutrient management policies in the management of Hood Canal. We were very concerned when we heard from colleagues involved in Puget Sound issues that the report demonstrates that nitrogen is not an important Hood Canal management issue. Elevated nutrient inputs can have fundamental impacts to marine food webs and individual taxa that are not within the scope of the report. For example, the recent Blue Ribbon panel report on Ocean Acidification indentifies reduction of nutrients and organic carbon as important aspects of the state's response to ocean acidification.

[1]"…human-caused nitrogen discharges have almost no effect on dissolved oxygen in central Hood Canal" (p. 83). While the potential for elevated nutrient impacts in Lynch Cove is clearly presented, the final conclusion is that "available studies are inconclusive on compliance with water quality standards in this area due to substantial uncertainty in the methods, nitrogen sources, and averaging over time and space" (p. 82). The overall tenor of the findings is to downplay the risk of human-derived nutrient inputs to the Hood Canal marine system.

In summary, we request that language is added to both the report and the accompanying memorandum that summarizes the limitations in the scope of the findings, especially related to nearshore environments. We also request that the in the memorandum be expanded beyond its current focus on shellfish as the resource of concern in shallow water habitats. Nearshore habitats provide essential fish habitat and are known to be sensitive to human-caused pollution. Aquatic vegetation is one key indicator of nearshore habitat condition.

We strongly agree with EPA and Ecology that additional protections are needed to ensure that nitrogen and bacteria loadings are minimized to protect ecosystem health.

Response:

We acknowledge that there is a potential for misinterpretation of the findings of our report, and we have emphasized the narrow scope of our report in all public settings. We agree that nutrients can affect the marine ecosystem beyond impacts to dissolved oxygen, but the dissolved oxygen concern has been the primary topic of Hood Canal research to date. We took the scientific information on its face and sought a tone in the report that neither exaggerated nor downplayed the risks and uncertainties in impacts from nutrient inputs.

As noted, the state Blue Ribbon panel report on ocean acidification identifies local discharges of nutrients and organic carbon as an additional stressor on top of the global carbon dioxide inputs to the ocean. We support additional investigation to assess the relative effects of local versus global drivers of acidification.

Comment from Kitsap County Public Health:
While it is good to know that human activities are not causing the fish kills near Hoodsport, it is evident that Hood Canal is a sensitive waterbody and that humans need to be aware of impacts our activities may have on Hood Canal. Appropriate and effective oversight and management of these human activities is vital to protect the many beneficial uses and resources of Hood Canal. We understand that effective wastewater management is one of these critical human activities. Kitsap Public Health has been, and will continue to be, fully committed to protect Hood Canal and to ensure that septic systems are sited, designed, installed, maintained, and used properly in accordance with Washington State Department of Health regulations, including the timely identification and correction of failing septic systems.

Response:
Comment noted. We agree that septic system management is extremely important in this watershed.

Peer Review Process

Comment from Kitsap Public Health District:
As you outline in your September 14, 2012, Memorandum, peer review of scientific reports generally leads to better scientific products. The Kitsap Public Health District vigorously agrees with this simple, straight-forward concept, especially when public money and public agency time and effort are involved (as they have been with the HCDOP for over six years now). But scientific peer review is even more important, critical, reasonable, and logical when government sponsored scientific products may impact management decisions that in turn may affect private citizens by way of new rules, regulations, and/or financial investments. In such instances it is critical that the science products are valid and transparent --- and this can really only happen through rigorous peer review. Thank you for your leadership with the Hood Canal Coordinating Council to ensure that this peer review process occurred for the Hood Canal dissolved oxygen studies.

Response:
We agree on the importance and value of peer review.

Comment from Lower Hood Canal Watershed Coalition:
The Lower Hood Canal Watershed Coalition (LHCWC) is disappointed that the long-awaited peer review does not bring more confidence and clarity to the HCDOP reports and, in particular, the anthropogenic contributions to reduced dissolved oxygen levels in the lower arm of Hood Canal.

Response:
Comment noted. We acknowledge that this review has clarified some aspects of the human impact, but it has also illuminated important uncertainties as well such as marine nitrogen flux that could not be resolved further with the information available.

Comment from Lower Hood Canal Watershed Coalition:
For over 20 years, LHCWC and its predecessor organizations have promoted good stewardship and best practices for residents, businesses and visitors to Hood Canal. Now, after asking citizens to wait patiently for this peer review, the results are inconclusive and there remains disagreement among scientists and researchers. Citizens are increasingly cynical about the money and time spent on

researching Hood Canal's dissolved oxygen issues, however, a scientific consensus has the potential to turn public opinion around.

Response:
As noted in the introduction to the review, EPA and Ecology have sought consensus among researchers for over 18 months, with several rounds of meetings, reviews of drafts, and independent review. There is consensus around most of the findings in the review document. Nevertheless, some disagreement remains due to the complexity of the system, limits of available information, and differing professional opinions among researchers over assumptions that cannot be fully supported or refuted by available information. EPA and Ecology responded to these challenges with an independent peer review, conducted by the Puget Sound Institute, to identify issues and uncertainties that affect planning and implementation of pollution control programs. Despite these uncertainties, the final report provides valuable technical information for pollution control and watershed planning.

Comment from Lower Hood Canal Watershed Coalition:
A consensus must be reached. Every HCDOP scientist and all EPA and Ecology reviewers need to meet face-to-face in a facilitated discussion, share and exchange all information and hammer out a consensus.

Response:
The EPA/Ecology review is not a consensus-based document, but we explored areas of consensus and disagreement with the Hood Canal researchers. We met with every lead scientist (including HCDOP lead researchers) on multiple occasions, including workshop-type meetings to attempt to "hammer out a consensus". Even after input from the independent review panel, consensus was not achieved. Consensus, by definition, cannot be willed upon the participants.

Comment from Port Gamble S'Kallam Tribe:
The report suggests that there is no significant human contribution to low dissolved oxygen in the mainstem of Hood Canal. This, we find patently absurd and would like to request that a full scale follow-up technical meeting be conducted as was recommended at the September 12 meeting in Belfair which would include most, if not all HCDOP scientists.

We would like to insure that the above mentioned science meeting takes place and prefer it to be professionally facilitated, documented, and has a formal follow up process defined before we finalize our comments.

Response:
As noted above, EPA and Ecology have met with every lead scientist (including HCDOP lead researchers) on multiple occasions, including workshop-type meetings, to discuss several drafts of the report. We have also received comments from HCDOP scientists on the latest draft of the report and have responded in this document.

Except for ongoing debate about the subsurface seaward outflow and potential connections between Lynch Cove and Hoodsport conditions (see the section in this report on that topic), the finding of no significant human impact due to nitrogen discharges in the mainstem of Hood Canal is a consensus finding among Hood Canal researchers and the independent review panel.

Shoreline OSS and Groundwater

Comment from Kitsap County Public Health:
We agree with and support your refinement of shoreline OSS nitrogen contributions. It is critical, for the reasons mentioned above, to eliminate incorrect and non-documented data. We also agree that a measurement approach based on real spatial and time-relevant seep and groundwater data is superior to per capita "guesstimations". Looking into the future, we believe this is a data area that we (as a region) can/should improve on. Kitsap Health will include salinity measurements for any future shoreline nitrogen sampling that we are able to conduct.

Response:
Comments noted. We support inclusion of salinity measurements in future shoreline nitrogen sampling.

Comment from Kitsap County Public Health:
Although not really relevant to the Lynch Cove area of Hood Canal, future studies of "shoreline OSS impacts" should address high-bank areas where OSS are substantially elevated (10 – 40 feet in elevation or greater) above the shoreline. High-bank areas are prevalent in central and north Hood Canal. These high-bank areas do not experience salt water intrusion of the drainfield as some shoreline OSS may experience.

Response:
This comment is consistent with the recommendations for future analysis by Brett (2011f) that we cited in the report. We agree that future refinement of shoreline OSS analysis should consider a sub-category of high-bank areas.

Comment from Kitsap County Public Health:
Identifying/employing a process to determine background shallow-groundwater DIN levels would seem to be another data gap that could be better addressed in the future.

Response:
We agree that this gap should be addressed.

Comment from Kitsap County Public Health:
Another data gap that we feel needs to be addressed is differentiating OSS-N seep inputs from pre-1995 OSS versus post-1995 OSS. State OSS regulations changed significantly in 1995. On a number of management fronts, it would be good to know if the higher-level OSS regulations adopted in 1995 are more protective of water quality than standards used prior to that.

Response:
Our review was focused on estimated current impacts, but we agree that all available information should be considered in evaluating effectiveness of regulations over time. None of the available estimates considered age of wastewater infrastructure so we had nothing to review.

Comment from Kitsap County Public Health:
On this same note, many failing septic systems have been identified and repaired/replaced since HCDOP started in 2005. This is an ongoing process. Is there a way to determine if the elimination of these failing OSS have resulted in water quality improvements with respect to nitrogen? We know that fecal coliform

bacteria levels have been improved in many areas of Hood Canal as a result of OSS repairs. If would be beneficial to know (if possible) if nitrogen levels have also decreased as a result of these corrective actions.

Response:
Due to the significant natural variability in oxygen conditions, we can only estimate water quality improvements using models. Since the algae in Hood Canal are nitrogen-limited, OSS improvements will improve water quality in proportion to the level of reduction from current loadings. Over time, the current baseline of seep data can be updated with new sampling information and re-analyzed to determine the overall improvement in median and average nitrogen concentrations.

Comment from Mason County Public Health:
In general, it appears there may be some misunderstandings regarding where and when MCPH performed nutrient monitoring. MCPH performed sampling for nutrients under two circumstances:

1. The samples were taken from within the segments that were selected for nutrient analysis, called the 'Nutrient Segments' or
2. During resampling of sites where the original fecal coliform sample was ≥900 FC/100mL.

MCPH collected samples from any flowing water accessible on the beach; this includes seeps, bulkhead and other drains, tight-lined flows, creeks/streams, stormwater discharges, etc. MCPH believes that the analysis of our data by PSI was focused on sites where the word seep was used in the description.

Response:
We appreciate the clarifications. Note that the subset of groundwater data we used for our primary data source was obtained from USGS (PSI conducted a separate analysis). MCPH is correct that data was sorted based on a search of sample locations identified as seeps.

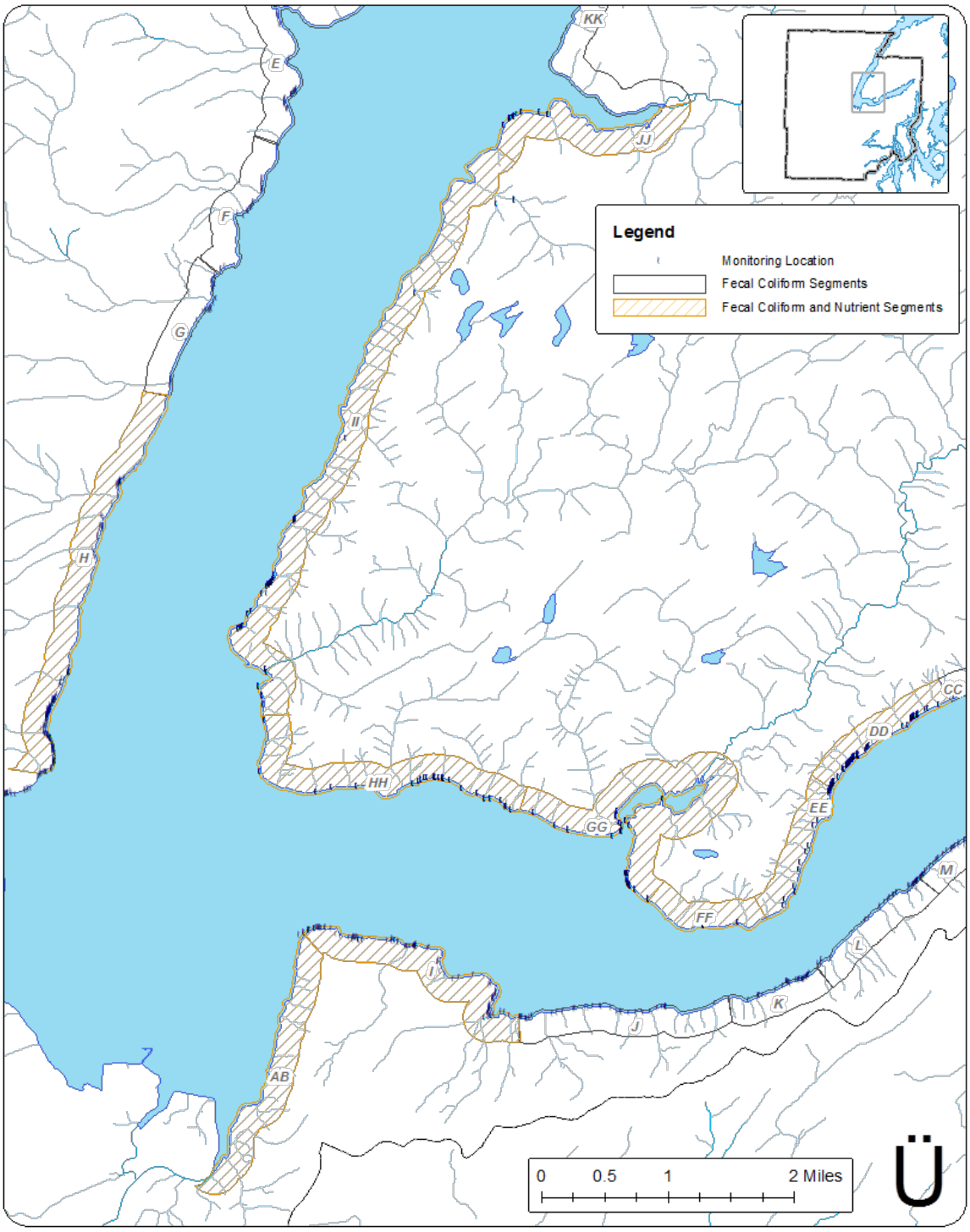
Comment from Mason County Public Health:
MCPH collected *fecal coliform* samples from all accessible waterways/discharges that were flowing into Hood Canal; however, MCPH only collected *nutrien*t samples from ~6 sub-sections of shoreline. In general, MCPH monitored along the western and eastern shorelines in areas located from Hoodsport south to the Skokomish Tribal Property and from the southeastern shoreline of Annas Bay, north and then east to the Alderbrook Resort. Along the northern shoreline, samples were analyzed from nutrients from approximately the eastern side of Sister's Point west around the corner and then north to the Dewatto River. MCPH is unsure if the data collected along the northern/eastern shorelines was utilized for the data analysis.

Response:
Despite the fact that nitrogen samples have not been collected across the entire shoreline, the available data for shoreline seeps remains the best available information for shallow groundwater quality. The USGS-analyzed data we relied upon included approximately 16 seep samples from the northern/eastern area identified in the County maps, but the data period extends to November 2010 whereas the County indicates in its comments (below) that sampling in this area continued into 2011.

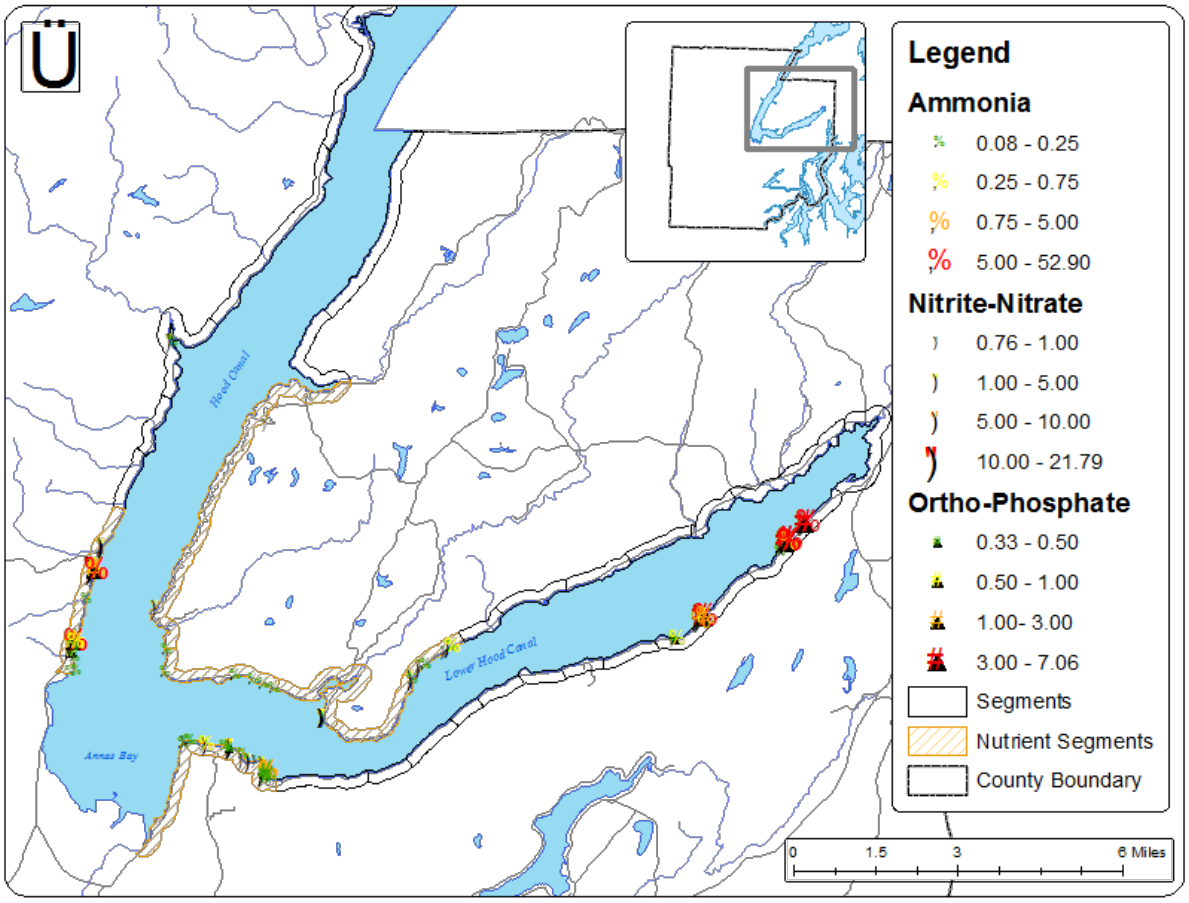
Comment from Mason County Public Health:
MCPH has included two maps in these comments. The first map shows all monitoring locations and the nutrients segments. The second map shows nutrient results that MCPH identified as being above the

"Level of Concern." MCPH defined the "Level of Concern," as the 90th percentile for each analyte. In addition, the segments with orange diagonal lines represent those segments that MCPH had nutrient analysis in addition to fecal coliform analysis completed at each discharge point. In addition, there are several nutrient results that are located outside of the nutrient segments. Those samples were taken in response to the initial fecal coliform sample result being ≥900 FC/100mL.

MCPH Nutrient Segments and Monitoring Locations



Map Created September 24, 2012 byA. Georgeson



Response:
We appreciate the inclusion of maps in the comments. We have replaced the map in the draft report with the second map above, since it shows both nutrient segments and results of targeted sampling in Lynch Cove. The County provided us with updated maps with clearer graphics for our report.

Comment from Mason County Public Health:
In regards to comments about data definitions and limitations of data; I understand that many datasets are limited in one way or another. I am not advocating for not using the data, I just think that there should be clear definitions and discussions about the known limitations of the data.

Response:
Comment noted.

Comment from Mason County Public Health:
Page 30, 4th paragraph, last sentence: "The median DIN concentration was also 250 µg/L in Lynch Cove, where there are significantly fewer seeps due to the mudflat terrain (25 samples, with none along the eastern shore at Belfair)."

MCPH did not monitor along the Belfair shoreline located between approximately just east of Deveraux Creek to the just west of the Union River. The main reason that samples (fecal coliform or other) were

not collected from this shoreline was not that 'there were significantly fewer seeps', but that MCPH could not safely access the tidal flats. MCPH did not make any assumptions about how many waterways were missed in this area. MCPH also did not have any nutrient segments in this area. The only sample data that has nutrient results in this area are associated with elevated levels of fecal coliform (≥900 FC/100mL) and most (all?) of which are associated with on-site septic system that were found to be failing.

Response:
We appreciate this correction and revised the report language accordingly.

Comment from Mason County Public Health:
Page 31, Map: The map on this page shows the locations of all MCPH *fecal coliform* monitoring locations, a sub-set of these sites were analyzed for nutrients. See maps 1 and 2 included within this comment document for better maps that show the nutrient monitoring locations. On request, MCPH will provide other maps, if there is something particular that would be useful to this review.

Response:
We appreciate this correction and have replaced the map in question with the maps provided by the County.

Comment from Mason County Public Health:
Page 34, 1st paragraph, last sentence: "For 21 Lynch Cove samples, the median was 190 µg/L and the mean was over 2,700 µg/L due to a small number of high sample concentrations."

Clarification: The only samples that were analyzed for nutrients on the south shore in the area of Lynch Cove were associated with an initial fecal coliform result ≥900 FC/100mL. See map below, blue sites are fecal coliform monitoring locations, red sites are confirmation monitoring locations where the initial FC result was ≥900 FC/100mL. There were 23 monitoring locations in this area where the initial FC result was ≥900 FC/100mL. MCPH has confirmed OSS failures associated with 7 monitoring locations, associated with 3 parcels, which were repaired. MCPH suspected OSS failures at 14 monitoring locations, associated with 4 parcels, but no follow-up work was performed, either because no contact could be made with the homeowner or MCPH was denied access. Two monitoring locations had low fecal coliform and nutrients during the confirmation monitoring event. MCPH hypothesizes that these two results may be associated with structures with seasonal occupancy, where the site was occupied during the initial monitoring event, but was not occupied during the confirmation monitoring event.

Response:
We appreciate this information and included the OSS failure counts in the final report.

Comment from Mason County Public Health:
Page 34, Table 4, notes: "Sheibley and Paulson (2011) analysis of data from Georgeson et al. (2008). Dataset reduced to one value per seep by EPA/Ecology. Lower and upper bound are median and mean sample values, respectively."

How did USGS/EPA/Ecology reduce the dataset? Was only the most recent monitoring event used? At least one segment was used for this analysis (and 5 additional segments along the north/east shores) were monitored both during wet and dry weather. MCPH believes it would be inappropriate to reduce

to one value per seep in these circumstances. MCPH can also provide data in reference to known sites with OSS failures, if the researchers want to reduce the data to one value for each of those sites.

Response:
As the draft report indicated, to reduce the bias from multiple sampling of seeps, we reviewed the Sheibley and Paulson (2011) dataset and reduced the distribution to a single value per seep with preference given to the most recent summer sample value. We disagree that this is inappropriate, because inclusion of multiple sample values for a single seep will skew the distribution.

Comment from Mason County Public Health:
MCPH did not remove any data for the following summary statistics, which were compiled as part of the Northshore-HCPIC final report:

http://www.co.mason.wa.us/health/environmental/water_quality/reports/northshore/2011_Final_G1000122_NSPIC.pdf

The data was separated between the Northshore HCPIC project (project area north and east shores of Hood Canal) and the HCPIC project (project area west and south shores of Hood Canal) and then all data was combined. MCPH also has this data for those sites within the HCPIC study where OSS failures were identified.

Figure 1. NS-HCPIC Summary Statistics of Nutrient Monitoring Results (2009-2011)

	Ammonia-Nitrogen (N) (NH_3-N) (mg/L)	**Nitrite-Nitrate-N (NO_3 + NO_2 – N) (mg/L)**	**DIN (NH_3 + NO_3 + NO_2 – N) (mg/L)**	**Ortho-Phosphate (PO_4-P) (mg/L)**	**Salinity (ppm)**
Average	0.02	0.21	0.23	0.02	1
Median	0.01	0.07	0.09	0.01	0
Mode	0.01	0.01	0.02	0.01	0
Max	0.60	4.50	4.51	0.25	20
Min	0.01	0.01	0.02	0.01	0
90th Percentile	0.04	0.49	0.52	0.05	3
St. Dev.	0.05	0.43	0.43	0.03	3
Count	347	347	347	347	347

Figure 2. HCPIC Summary Statistics of Nutrient Monitoring Results (2007-2008)

	Ammonia-Nitrogen (N) (NH_3-N) (mg/L)	**Nitrite-Nitrate-N (NO_3 + NO_2 – N) (mg/L)**	**DIN (NH_3 + NO_3 + NO_2 – N) (mg/L)**	**Ortho-Phosphate (PO_4-P) (mg/L)**	**Salinity (ppm)**	**FECAL COLIFORM (colonies/100mL of H_2O)**
Average	0.36	0.49	0.85	0.23	6	448
Median	0.02	0.21	0.27	0.06	2	7
Mode	0.01	0.06	0.11	0.02	0	2
Max	52.90	21.80	65.30	7.06	30	160000

Min	0.01	0.01	0.02	0.01	0	2
90th percentile	0.12	0.89	1.14	0.65	20	240
STDEV	3.15	1.31	3.75	0.61	8	6690
Count	593	593	593	593	432	593

Figure 1. HCPIC and NS-HCPIC Summary Statistics of Nutrient Monitoring Results (2007-2011)

	Ammonia-Nitrogen (N) (NH_3-N) (mg/L)	**Nitrite-Nitrate-N ($NO_3 + NO_2 – N$) (mg/L)**	**DIN ($NH_3 + NO_3 + NO_2 – N$) (mg/L)**	**Ortho-Phosphate (PO_4-P) (mg/L)**	**FECAL COLIFORM (FC/100mL of H_2O)**	**Salinity (ppm)**
Average	0.24	0.39	0.62	0.16	312	4
Median	0.01	0.15	0.19	0.04	4	0
Mode	0.01	0.06	0.03	0.01	2	0
Max	52.90	21.80	65.30	7.06	160000	30
Min	0.01	0.01	0.02	0.01	2	0
90th Percentile	0.09	0.74	0.87	0.33	216	15
St. Dev.	2.51	1.08	3.00	0.50	5317	7
Count	940	940	940	940	940	779

Response:
We appreciate this information from the impressive database developed by Mason County. These summaries are for all samples, not only seeps. We maintain that seep samples are most representative of groundwater, which is the focus of our inquiry into the PIC data. Nevertheless, we note that the median and mean DIN values from the full database (above) are quite similar to those for the seep-only database developed by USGS and used in our analysis.

The dataset summarized by the County above includes new data collected since the EPA/Ecology report was submitted to independent review. The new data has not been analyzed by USGS to distinguish seeps, and we continue to rely on the previous work by USGS as the best available dataset for estimation of shallow groundwater quality.

Finally, we have added the North Shore report cited in the comment to the reference list in the report.

<u>Comment from Mason County Public Health:</u>
Page 35, paragraph 3, 1st sentence: References 10% denitrification rate used by Paulson et al.

> Is it known if they gave a reference for why they used this amount? It is known that denitrification rates are highly variable and although this amount does not seem totally out of sync with common amounts out there, it would be great to have a reference as to why they chose that particular value. Kitsap County determined that:

"Many uncertainty factors are apparent regarding fate and transport of nutrients in Hood Canal and nationwide. The nitrogen removal efficiency is highly variable dependent on many factors including: OSS treatment type, design, installation, location, operation and maintenance, waste strength, water use, source water chemistry, disposal soil characteristics, surrounding vegetation (or lack thereof); and groundwater and surface water conditions including proximity to these waters.

Previously reported studies of OSS nutrient transport have been performed in the Pacific Northwest. These studies found nitrogen concentrations in down gradient groundwater samples similar, or reduced, and highly effective nitrogen removal rates by OSS. They stress that attenuation processes occurring during groundwater transport within the unsaturated soil horizon are responsible for high nitrogen removal rates.

A study by Patmont, Pelletier, Welch, Banton, and Ebbesmeyer in Lake Chelan showed two of seven test sites had nitrogen removal rates of 100 +/- 3% and two had 99 +/- 3% removal. The overall removal rates of nitrogen by OSS averaged 89 +/- 7%. Nitrogen removal was not correlated with depth to water, but appeared to be somewhat correlated with local hydraulic conductivity. The study notes that less permeable soils appear to better remove nitrogen, possibly due to greater residence time within the shallow vegetative root zone and associated plant uptake (Patmont, 1989). Hart Crowser cited nitrogen uptake by plants in shallow groundwater systems in Black Diamond, Washington, "owing to the general deficiency of this plant nutrient in regional soil systems" (Gessel, et.al., 1969; Harper-Owes, 1985). At the study sites on Lake Sawyer overall average nitrogen attenuation was 78.6 +/- 10.7%. Some sample sites showed 100% removal (Hart Crowser, 1990)."

http://www.kitsappublichealth.org/environment/files/reports/shoreline_discharge_technical_report.pdf

Patmont, Clayton R.; Pelletier, Gregory J.; Welch, ,Eugene B.; Banton, David; and Ebbesmeyer, Curtis C. Lake Chelan Water Quality Assessment Final Report, State of Washington Department of Ecology Contract No. C0087072, January, 1989.

Hart Crowser. Lake Sawyer Hydrogeologic Study, Black Diamond, Washington, Prepared for Washington State Department of Ecology Contract No. J.2482, October 5, 1990.

Response:

Paulson, et al. (2006) assumed "a loss of 10 percent of the total dissolved nitrogen through denitrification to nitrogen gas (N2) or volatilization of ammonia (Dudley and Stephenson, 1973; and Walker and others, 1973a, 1973b)". The report did not provide additional explanatory information. It should be noted that Paulson, et al. (2006) assumed a total nitrogen loss of 35%, via denitrification (10% as noted above) and removal of organic nitrogen in soils (25%).

We appreciate this information on the potential range of de-nitrification of OSS effluent in soils based on studies in other parts of Washington state. The draft report noted the importance of the processes that lead to removal of nitrogen in soils, and the range of estimated removal in a Hood Canal study by Atieh et al. (2008). We have added language to this discussion, noting the range of removals from the studies referenced in the Kitsap County report, and we have included the report to our reference list.

Comment from Mason County Public Health:

Page 37, paragraph 3, 1st sentence:

"A more recent analysis (Horowitz and Peterson, pers. Comm.., 2011) identified 979 shoreline OSS within a 30-meter buffer."

MCPH feels that additional clarifying information should be provided about this dataset. MCPH believes this dataset was created based on the centroid of the parcel, which is arbitrary compared to the location of the OSS. Some parcels include the tidelands waterward of the OHWM, pushing the centroid of a parcel into the marine water. It is not uncommon to find that an OSS may be on an adjacent upland parcel and not located on the developed waterfront parcel, however, with the data used by Horowitz and Peterson, there is no way to identify these sites. MCPH has validated and verified data for the parcels located within 1000' of the shoreline. Part of the validation/verification includes determining if the site has an as-built record and whether the drainfield is located within 100' of surface water. This data is available and has been provided to Gretchen (Peterson).

Response:
Clarification noted. Since we adopted the measurement-based approach to estimating OSS nitrogen loadings, we have elected not to request refined information in support of existing per capita estimates.

Marine Nitrogen Flux Estimates and Influence on Dissolved Oxygen

Comment from Port Gamble S'Kallam Tribe:
While the science regarding the sources and causes of hypoxia and associated fish kills and unknown chronic and subsurface acute impacts are not clear, there is no question that human based nitrogen is a major source. It is clear to us, that significant additive amounts of human nitrogen inputs to the main stem (lower and central) Hood Canal are part of the problem. When the stage is set for deeper hypoxic waters to spread up into shallower surface waters and broaden out spatially lifting hypoxic waters up to the surface, the impacts from nitrogen to phytoplankton, to the benthos, and ultimately to hypoxic deeper waters have already taken place and the human sources contributed to that problem. We believe that the modeling is not accurately addressing this phased process in which by the time the winds and weather patterns trigger the upwelling process for deeper hypoxic waters to surface, the dead phytoplankton in the benthos has been degenerating for many months and the source of the surface water nitrogen when the phytoplankton are blooming is local, mostly human sources. The nitrogen laden marine waters, when they do move in, are held below the halocline and as such are not what actually enhances the phytoplankton.

Response:
We agree that many of the processes described in this comment are relevant to dissolved oxygen conditions, but we disagree with the conclusion that "there is no question that human based nitrogen is a major source" of oxygen depletion.

The statement that "nitrogen laden marine waters, when they do move in, are held below the halocline and as such are not what actually enhances the phytoplankton" is incorrect. All published and informal findings concurs that marine waters mix with the surface layer, as evidenced by the substantial salinity of the surface layer. Secondly, marine water moving landward below the surface layer directly fuels phytoplankton growth, because the euphotic zone extends below the pycnocline (surface layer boundary) due to availability of light at these depths.

We agree that seasonal water column and sediment enrichment by which human impacts could theoretically change the conditions prior to a weather-triggered fish kill. However, the available

information from multiple researchers indicates that marine nitrogen inputs, not human inputs, dominate the conditions in the main stem. As noted earlier, this is a consensus in the research groups involved in this review.

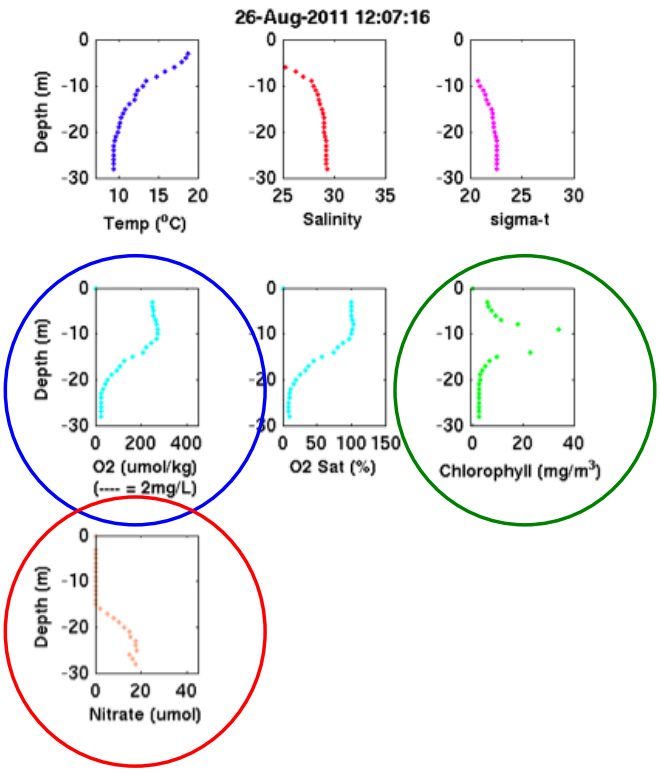
Comment from Allan Devol (UW):
With respect to the oceanographic aspects, it seems that the issue of the depth at which the marine loading calculation is made is critical. Pretty much no matter who's data you look at, it is clear that the euphotic zone extends below the mixed layer. As you point out, whether you use the base of the mixed layer or the bottom of the euphotic zone makes a big difference in the marine loading calculation, and thus to the importance of OSS. I continue to maintain that calculation of the marine loading from the bottom of the euphotic zone is not appropriate when trying to scale N-loadings (marine and anthropogenic) to oxygen drawdown. Assuming all OSS is input into the mixed layer, all OSS potentially contributes to deep oxygen draw down. However, some of the total marine loading to the lower euphotic zone below the mixed layer is used for photosynthesis and thus results in production of oxygen in the deep layer. This O2 production below the mixed layer is not included in the oxygen drawdown calculation. In other words, assessing the effect on oxygen from the ratio of OSS/Nmarine assumes that both OSS and Nmarine only result in oxygen consumption, but in reality some Nmarine actually results in oxygen production.

Response:
We agree that the assumptions used in the aggregated (box) model have a direct and significant effect on the human impact estimates. We also agree that oxygen demand in the lower layer is offset, to some extent, by production in the lower euphotic zone (which is fueled by the marine nitrogen flux). For these reasons, we adopted Allan Devol's assumptions into our impact estimates. However, we did not adopt Allan Devol's assumptions alone. We also accepted the alternative conceptual model of Michael Brett (UW), whose assumptions diverge from those of Allan Devol. As noted in our report, we believe both sets of assumptions are reasonable in the context of the uncertainties in the oxygen dynamics of this system, and we adopted a range of marine nitrogen fluxes that captures both sets of assumptions.

As noted in our report, the independent review panel identified several fundamental limitations of each of the available box model analyses. The primary issues was how to analyze the monitoring data to calculate fluxes. No re-analysis has been conducted to address the technical points raised by the panel. Our report continues to rely on the available information, but the aggregated models should be re-analyzed as recommended by the independent review panel before adopting the results in a regulatory decision.

Comment from Michael Brett (UW):
I still think you gave the HCDOP folks too much leeway on the 3-box model quandary. I think the external committee made it very clear that the HCDOP 3-box construct made no sense. Phytoplankton production driven by marine inputs below the pycnocline but within the photic zone will produce organic matter that settles out and contributes to hypoxia below the photic zone. I believe pretty much every ORCA buoy profile collected during the low DO months shows that (crystal) clearly [see attached ppt file].

(Note from EPA/Ecology: We have copied below the first of 35 slides provided with the comment.)



Furthermore, the HCDOP hypothesis that 1/3 of the marine N gets used above the pycnocline and 2/3 gets used below (and doesn't count) ISN'T supported by the field data. This is just a mathematical artifact created by averaging lots of field data. As the attached plot shows, in all cases the NO3 was used up immediately below the Chl peak. In about 1/3 of these cases the Chl peak was above the pycnocline and in about 2/3rds it was below.

IF the HCDOP hypothesis were true then we would see systematically higher DO below the photic zone when the NO3 gets all used up below the pycnocline. I don't see that pattern in these data.

Response:
Our report adopts a range of potential marine nitrogen loadings for comparison to watershed loadings. The range spans the low and high end estimates of the different methods used (aggregated model in Devol et al, 2011a; ROMS model in Kawase and Bahng, 2011). The method advocated by Michael Brett has merit and leads to a loading value in the middle of this range. This comment, then, is directed at the low end estimates in Devol et al. (2011a). Note that we have added a figure in our report that summarizes the range of nitrogen loading estimates by different researchers.

While the independent review panel expressed a general preference for the 2-layer aggregated model instead of a 3-layer model as in Devol et al., 2011a, the panel's comments were not detailed enough to warrant a rejection of the 3-layer construct in our opinion. We could not outright reject the assumption of Devol et al. (2011a) that some fraction of the oxygen production below the pycnocline mixes into the lower water column, mitigating a fraction of the marine nitrogen contribution. Since the assumptions of Devol et al. (2011a) process are environmentally conservative and lead to higher estimates of human

impact, we believe the resulting loading estimates are a reasonable lower bound for the marine nitrogen loading.

Finally, as noted above and in our report, the independent review panel identified several fundamental limitations of each of the available box model analyses. No re-analysis has been conducted to address the technical points raised by the panel. Our report continues to rely on the available information, but the aggregated models should be re-analyzed as recommended by the independent panel before adopting the results in a regulatory decision.

Dissolved Oxygen Impact Estimates

Comment from Allan Devol (UW):
I find interesting that the Monte Carlo gives a median of 0.06, which is lower than any of the estimates in Table 11.

Response:
The values in Table 11 derive from a variety of assumptions about oxygen deficit and nitrogen loading. The primary reason the Monte Carlo median estimate is lower than values in Table 11 is that the onsite sewage system (OSS) loading values used by researchers prior to the EPA/Ecology report were higher than the values we estimate using shoreline seep data.

Comment from Allan Devol (UW):
Also, on page 71 you present Mitsuhiro's (Kawase) model simulated changes. If you interpolate to 300 mg/l, which you say is representative of L.C. (Lynch Cove) you would get a drawdown of about 0.14 mg/l again higher than the Monte Carlo. As you point out this is mainly driven by the Marine input value, which relates back to the euphotic zone/mixed layer discussion above.

Response:
The interpolation offered carries a similar risk of over-estimation as the 500 ug/L scenario from the model (note that the comment has a typo - "300 mg/L" should be "300 ug/L"). That is, the interpolated scenario implies that central Hood Canal tributaries, including the nearby Skokomish River, are discharging nitrogen at 300 µg/L. This is three times higher than current concentrations (about 100 µg/L), and this loading may cause an overestimation of dissolved oxygen depletion in Central Hood Canal and Lynch Cove. In any case, the estimate of 0.14 mg/L would fall within the estimated range of uncertainty in the draft report (0.03 to 0.3 mg/L).

Comment from Allan Devol (UW):
While we are on the Monte Carlo, looking at first term on the right hand side of the equation on the bottom of page 63 it is the fraction of loading attributable to humans. This is multiplied by the drawdown, D. Since the oxygen draw down (1.6 mg/l) is derived from the difference between Hoodsport and Twanoh, isn't the implicit assumption that the drawdown occurs over the residence time of the water between Hoodsport and Twanoh, which is related to its travel time? This gets to the question about lagged versus unlagged differences. You assume no lag for the estimation of D. Doesn't no lag assume one of two things; 1) there is no travel time between Hoodsport and Twanoh, 2) oxygen concentrations don't change at Hoodsport.

Response:
We do not believe that the time lag assumptions are adequately assessed in the research to date, and we also reject the notion that use of unlagged data is representative of the two assumptions offered in the comment. The choice to use unlagged differences was explained in the draft report, which notes that "It is not clear whether it is appropriate to incorporate a fixed time lag into the analysis, because the travel time likely varies significantly with the onset of the September intrusion. It is also difficult to discern a clear lag in time series comparisons (see Figure 8). Furthermore, it appears that the 25-day travel time calculated by Devol et al. (2011a) was based on circulation conditions during the September intrusion of saline ocean water, when current speeds are unusually high compared to mid-summer conditions. We back-calculated the velocity from the advective flow from Devol et al. (2011a) using the salt balance method. The resulting value (0.0005 m/s) is an order of magnitude lower than the velocity used in the travel time calculation by Devol et al. (2011a)." We would only re-emphasize that a lag effect is not apparent in the time series comparisons. Also, average current speeds at depth in this area are very low, and diffusive flux/mixing may be an important factor, as noted in the comments of the independent review.

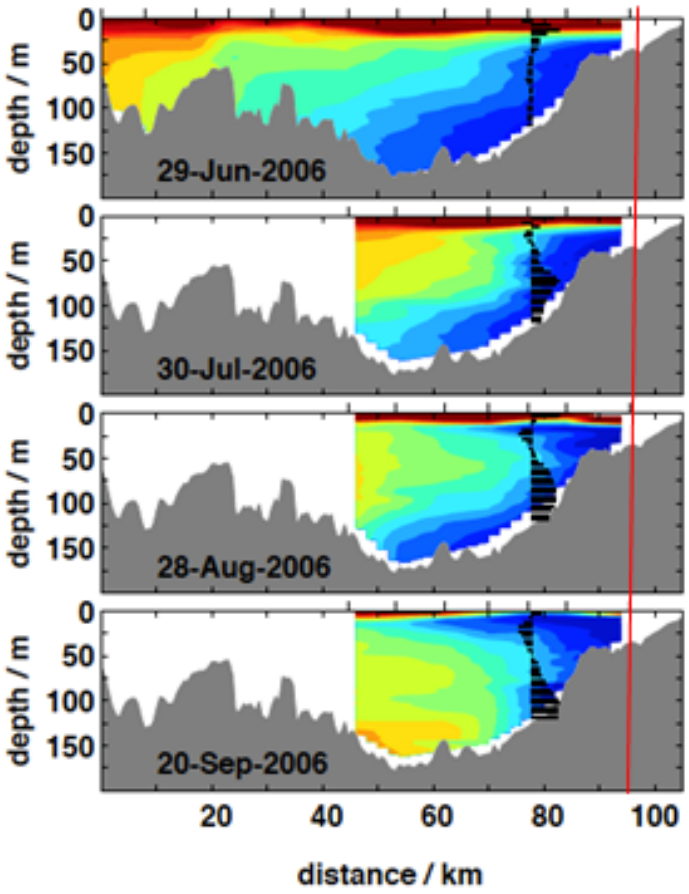
Finally, this issue is another challenge related to the over-simplifications of the aggregated (box) model. As noted above, the independent review panel raised concerns about the uncertainties inherent in the existing box models. A spatially-explicit, time-varying model (e.g., ROMS) can account for simultaneous transport and drawdown, whereas a box model requires highly simplified system characterization.

<u>Comment from Michael Brett (UW):</u>
I think Al (Devol)'s way of calculating the delta DO between the Lynch Cove and Hoodsport buoys does not make physical sense. In fact, his approach directly contracts some of the most compelling field data we have.

To recap, I calculated delta DO as the difference in DO for the entire water column below the pycnocline. Al calculated the delta DO by comparing the 10-20 m strata at both locations. Al's approach gives twice as large of a delta DO as does mine.

The problem with Al's approach, as I see it, is what happens to all of the water below 20 m at Hoodsport if it does not get advected downstream and through Lynch Cove? Does this water just sit therefor infinity? This cannot be the case. So this deep water either refluxes below the pycnocline before ever getting to Lynch Cove (in which case the 10-20m strata would be moving AWAY from Lynch Cove), or alternatively it is ultimately advected to and out of Lynch Cove as I hypothesize.

BUT we know from the data presented in the Micket analysis that in the summer of 2006 the water that refluxed from Hoodsport to Lynch Cove and back out again actually originated on the BOTTOM of the area near Hoodsport.



This directly contradicts Al's hypothesis. So during the time when DO was the lowest in recent history, we know for sure Al's hypothesis was 100% wrong.

Response:
The independent review panel recommended that the deficit calculation focus on oxygen conditions along similar densities (termed "isopycnal lines") between Hoodsport and Twanoh. This supported the method of Devol et al. (2011a).

As noted in the draft report, the aggregated modeling approach rests on an assumption that circulation conditions are constant over the period of analysis from June through September. For most of this period, it is reasonable to estimate circulation flows and mixing based on the assumption that mixing is constrained within isopycnal zones (method of Devol et al. (2011a). However, the intrusion of denser, low oxygen marine water at depth in September is a significant external forcing mechanism that alters the mid-summer balance. As Devol (2011d) notes, energy is required to overcome isopycnal mixing, and the September intrusion provides an influx of mixing energy. For this reason, we find merit in Brett's criticism of the isopycnal method for this fall intrusion period. It is plausible that the early-summer dissolved oxygen deficit is best evaluated using the Devol et al. (2011a) approach, while the September deficit may be better characterized by the Brett (2011i) approach.

Sediment Core Information

Comment from Kitsap County Public Health:
Thank you for utilizing a historical perspective and the readily available, relevant historical data (i.e., sediment core data, etc.) to evaluate the dissolved oxygen status of Hood Canal. This has been a glaring data gap that had been under-utilized prior to this report.

Response:
Comment noted.

Subsurface Seaward Outflow

Comment from Lower Hood Canal Watershed Coalition:
There is an apparent absence of discussion of the "squirt" of low dissolved oxygen from lower Hood Canal into the main stem of the Canal as a result of the intrusion of ocean water. The "squirt" is an important finding in the HCDOP report and may affect the conclusions.

Response:
The draft review does discuss this phenomenon, but we refer to it as "subsurface seaward outflow" rather than a "squirt" as terminology agreed upon with John Mickett.

Comment from John Mickett (UW):
Excerpt from draft report:

> *Second, water-column respiration depletes oxygen levels immediately below the euphotic zone. As described in Devol et al. (2011a), primary productivity in the euphotic zone produces organic matter that decomposes lower in the water column. Mickett et al. (2011, and personal communication) ruled out respiration as causing the dissolved oxygen minimum at 20 meters depth based on bounding calculations that have not been published. However, dissolved oxygen time series indicate that concentrations decline by 2 mg/L over 4 months (Devol et al., 2011a). Until additional respiration-based calculations are made available, water-column respiration, particularly during the late summer months, cannot be ruled out as a controlling factor for the dissolved oxygen minima observed at Hoodsport.*

Comment on this excerpt: *Mickett et. al 2001* made the point and showed with cross-channel transects at Hoodsport from Oct 2008 (fig. 4) that the lowest DO water at HP (then <2.5 mg/l) was *highly* correlated with the seaward shallow jet. The likelihood that this minimum was, then, caused by "local" respiration, or decrease in DO as water is upwelled locally at HP seems low. Hoodsport moored time series data show the same advective feature year after year---lowest DO always correlated with seaward flow. Granted, some of the north-flowing water could have originated in the main stem south of HP (and at <50m depth for the initial stages of the outflow) and decreased in DO levels as it was advected seaward, but given the proximity of HP to the southern end of the main stem (~7 km) the time for respiration to act on this water between the advent of upwelling and the flow seaward past Hoodsport would be relatively short. Given flow rates in the jet of order 5 cm/s, 7 km would be covered in under two days. These evidence points to the lowest DO water in the early stages of the outflow (first 1/3 or so) to have originated from advection of an along-channel DO gradient, with lower-DO water south of

Hoodsport. T-S plot comparisons between the Hoodsport and Twanoh ORCA moorings indicate that the water upwelled at Hoodsport is seen shortly after at Twanoh---sometimes on the order of several days or less, implying a robust connection of the water masses of the two locations. By conservation of volume, water landward at Twanoh must be balanced by total water out minus river flow into Lynch Cove.

Response:
We appreciate this elaboration on factors reducing the influence of respiration on the subsurface minimum DO at Hoodsport, and we have edited the report to capture points raised in this comment. . We agree that the October 2008 transect data in Mickett, et al. (2011) show a pattern of lowest DO in the subsurface outflow, though the sampling period for that figure is approximately a month later than the fish kill period. The short travel time for the southern end of the main stem (7 km segment) is relevant, but we note that the distance from Sisters Point to Hoodsport is a more appropriate segment when discussing links between Lynch Cove conditions to Hoodsport conditions, rather than the length of the southern end of the main stem. This would approximately double the distance and travel time.

Comment from John Mickett (UW):
Excerpts from draft report:

> *Brett (pers. comm., 2011i) also noted that the subsurface seaward outflow mainly occurred mid-thalweg and not in nearshore areas where fish kills have been observed.*
> *AND Conclusion:*
> *The subsurface seaward outflow affects the thalweg, or center, of the channel, and not the nearshore regions where fish kills occur.*

Comment on these excerpts: It has been observed that the fish in HC will move to the highest DO levels available during low-DO events, which would be at the edges of the seaward jet and not in the middle. So, observations of the lowest DO water being in the center of the jet and the highest being on the edges is consistent with where fish kills were observed. Additionally, dead fish concentrate on shore--so there may be a sampling problem here. This reason for rejecting the low DO jet as related to the fish kill should be removed as it is unsubstantiated.

Response:
We continue to concur with the independent review panel that "the evidence that low DO water from LC (Lynch Cove) makes a contribution to fish kills at Hoodsport is weak." At the same time, we acknowledge that the language in the excerpts does not capture the uncertainty in precisely "where the fish kills occur" with respect to the jet location (i.e., the "sampling problem" identified in the comment). We have edited the report to address this point.

Comment from John Mickett (UW):
Excerpt from draft report:

> *The ROMS model found a maximum 0.04 mg/L human impact on dissolved oxygen where fish kills occur, well below the level that would make any difference to the biota.*

Comment on this excerpt: We all agree that the ROMS model failed to capture the details of this jet. When there was some indication of a seaward shallow outflow in the model, it extended over a greater depth range. Thus, the ROMS model should not be relied upon to draw conclusions about the influence of the seaward jet on fish kills.

Response:
We acknowledge the limitations of the ROMS model simulations, and we note those limitations in the report. However, there is a clear indication of the seaward outflow in the ROMS model. Kawase and Bahng (2010) emphasize the point in noting that "the outflow that overlies this intrusion between 10 and 60 meters is also seen both in the observations and in the model."

We disagree that the model results should be rejected as a line of evidence in the analysis of human contributions to fish kills. Model estimates of human impact to dissolved oxygen (both aggregated models and the ROMS model) are directly relevant to the fish kill analysis. We elaborate on this point in our response to the next comment (below).

Comment from John Mickett (UW):
Excerpt from draft report:

> *While Lynch Cove may have contributed to the Hoodsport dissolved oxygen minimum in August, by September density signatures were consistent with upwelled near-bottom water and not Lynch Cove water. This indicates that Lynch Cove water is not influencing dissolved oxygen conditions at Hoodsport in the time frame (September) of fish kill events. The independent review panel concluded that "the evidence that low DO water from LC (Lynch Cove) makes a contribution to fish kills at Hoodsport is weak. Therefore, available information indicates that human nitrogen releases to Lynch Cove do not significantly contribute to fish kills at Hoodsport.*

Comment on this excerpt: My independent analysis confirmed that the volume of water landward of the Twanoh ORCA mooring (3.9x10^8 cubic meters) is *similar* to the volume of water associated with the low-DO outflow at Hoodsport (10 m thick x 2000 m wide x 20000 m long, or 4x10^8 m^3). More conservatively, assuming a 20 m thickness---though HP ORCA observations show the core of the low DO water typically on the order of 10 m thick---gives a volume that is about twice that as that landward of Twanoh. Viewing this a simple mixing problem of two water masses---one mass upwelled south of HP and one water mass advected from landward of Twanoh---which hides complexities such as entrainment during advection, differing flow rates, respiration, etc., low-DO water from Twanoh could reduce DO at Hoodsport by as much as ONE mg/l during the first stages of the outflow.

This calculation is based on the difference in DO of the two locations (from 0.5 to as much as 2 mg/l) and assumes no changes of the "LC" water until it makes it to HP (no mixing or no further drops in DO levels). Again, this pertains to the first stages of the outflow when water originates from depths of 50 m or less. The anthropogenic part of this influence is obviously smaller---how much is hard to guess. This estimate does not account for the export of nitrate from LC to HP that could cause local draw-down of DO at HP if greater than upwelling nitrate levels. It also does not account for water that does not mix completely with the upwelling water, but instead simply displaces existing water at Hoodsport over the density range of the jet---which would obviously result in a greater influence of LC water on the low-DO signal at Hoodsport.

Finally, all of these estimates neglect the influence of the low-DO water between Twanoh and Hoodsport, though PRISM transects show that during the summer and fall upwelling period this region typically has lower DO than within the main stem---and there is potentially an anthropogenic contribution to these low-DO levels given the population density (Union and Tahuya).

Response:

This comment begins with a general estimate of the potential drop in DO at Hoodsport due to the seaward subsurface flow, and then delves into the key question: the "anthropogenic part of this influence". We appreciate the discussion of the overall influence of the jet on DO conditions at Hoodsport and referenced this input in the report.

Regarding the anthropogenic influence on conditions within the jet, the commenter's answer ("this influence is obviously smaller - how much is hard to guess") does not adequately characterize the substantial effort by multiple researchers to estimate this influence. These efforts include analysis of watershed loading, shoreline loading, marine circulation of nitrogen, and models to integrate these sources and derive human impact estimates. As noted in the comment above, the DO impact estimate from the ROMS model at the fish kill location was a maximum of 0.04 mg/L. Furthermore, based on both aggregated and ROMS models, the human impact on DO in Lynch Cove ranges from 0.03 to 0.3 mg/L. This general scale of impact at the fish kill location and in the potential source water for the subsurface seaward outflow suggests that DO conditions at Hoodsport, while potentially influenced by the seaward subsurface flow, are not significantly influenced by human releases of nitrogen in the watershed. This finding is consistent with our analysis as well as the conclusions of the independent review panel (PSI, 2012) and the modeling analysis of Kawase and Bahng (2012). We have added some discussion on this point in the final report.

Additional Analysis Needed

Comment from Kitsap County Public Health:
As recommended in your report, we support full re-analysis of marine nitrogen flux as this contribution is critical to determine the significance of the human/watershed N-impacts. A peer review/consultation process should be used to agree on the parameters of this re-analysis before it is initiated.

Response:
Additional analysis of marine nitrogen flux in Hood Canal/Lynch Cove will require new funding. If new funding is secured for such an effort, we would support development of a detailed Quality Assurance Project Plan (QAPP) that is peer reviewed as suggested in this comment.

Comment from Lower Hood Canal Watershed Coalition:
We support refinement of the existing marine analytical model for lower Hood Canal with existing data. Further, we support continuing the appropriate marine water measurements to support and continue validating the model.

Response:
Refinement of the model(s) and continuation of specialized marine water measurements such as ORCA buoys would require new funding. If new funding is secured for such an effort, we would support development of a detailed Quality Assurance Project Plan (QAPP) that is peer reviewed as noted above.

Comment from Lower Hood Canal Watershed Coalition:
Strong scientific consensus combined with additional results of a refined analysis model that includes the "squirt" data would reduce the range of uncertainty in the anthropogenic sources contributing to low dissolved oxygen levels in lower Hood Canal. We feel this is important so that LHCWC and other

groups and policy makers can develop clear, concise and factual messages for citizen stewards of lower Hood Canal and which can support potential corrective actions if needed.

Response:
See above (Peer Review Process) regarding the effort to achieve consensus. The subsurface seaward outflow ("squirt") was evaluated. Refinement of the model(s) would require new funding. If new funding is secured for such an effort, we would support development of a detailed Quality Assurance Project Plan (QAPP) that is peer reviewed as noted above.

Policy Implications

Comment from Port Gamble S'Kallam Tribe:
We respectfully yet strongly disagree with the Ecology and EPA report and determination that the findings of this science report do not support development of a state water quality improvement plan at this time. While the report described the sensitivity of Hood Canal and the importance of protecting water quality overall and minimizing human impacts, it fell far short of applying anything close to use of a scientifically defensible precautionary principle that we feel is not only warranted, but critical to the long term sustainable productivity of Hood Canal.

Response:
EPA and Ecology are committed to both scientific defensibility and long-term protection of Hood Canal. We respect the concerns of the Port Gamble S'Kallam Tribe and appreciate its commitment to water quality protection. The determination that a water quality improvement plan is not supported at this time, included in the cover memorandum for the draft report, does not represent a retreat from our commitment to support programs to protect Hood Canal. There are a number of regulatory mechanisms available for implementation of protective measures in the watershed. Hood Canal is a sensitive and vitally important waterway, and EPA and Ecology will continue to collaborate with the Hood Canal Coordinating Council, and the governments and stakeholders they represent, to identify the most effective path forward on water quality protection.

Miscellaneous Topics

Comment from Washington Department of Natural Resources (DNR):
We have particular concern about the nearshore environment and the effects of human-derived nutrients on seagrasses, as well as other nearshore biota. We think it fair to say that the report reflects a focus on the broad marine properties of Hood Canal. There is an emphasis on vertical structure of deeper main channel and the integrated effects of point and non-point nitrogen sources on the water body. This perspective does not consider that biotic and abiotic properties in the nearshore can strongly diverge from the main channel. The nearshore often mediates terrestrial nitrogen inputs to marine waters through local uptake and cycling. Terrestrial nitrogen inputs can potentially have strong localized impacts in the nearshore that are not observed further offshore. Not only have the detrimental impacts of human-derived widely demonstrated in the literature, nutrient management has proven to be a very challenging issue that will require the resolve of natural resource managers and policymakers. It is

counterproductive if readers of the report conclude that control of nutrient inputs to Hood Canal is not a management interest.

A substantial body of scientific research provides worldwide evidence that human-derived nitrogen creates eutrophic conditions (N:P>16; Kaldy 2006), reduces available light, and increases interspecific competition causing major stress on seagrasses, which are widely used as indicators of estuarine health. Seagrass loss related to nutrient loading has been documented in: Chesapeake Bay (Dennison et al 1993), Waquoit Bay, Massachusetts (Short and Burdick 1996, Valiela et al. 1997, Fox et al. 1996), Ninigret Pond (Short et al. 1996, Harlin et al. 1981), Great Bay, New Hampshire (Beem and Short 2009, Trowbridge 2012), Venice Lagoon, Italy (Coffaro 1997), Placencia Village, Belize (www.SeagrassNet.org) , Palau, Koror (www.SeagrassNet.org), Seto Inland Sea, Japan (Tamaki et al. 2002), Mumford Cove, Connecticut (Vaudrey et al. 2010), Tampa Bay, Florida (Johnasen et al. 1992, Greening 2000, Tampa Bay NEP 2009), Boston Harbor, MA (Taylor 2010, Maciolek et al. 2009). References for these publications can be provided.

Response:
We acknowledge that our report focuses on the large-scale properties of Hood Canal and not potential localized, nearshore effects of human-derived nitrogen loadings, including potential effects on seagrasses. None of the research to date focused on nearshore regions, and this is a review of current research. Our report reflects the focus and scale of analysis of the Hood Canal research community to date. One of our recommendations is to develop a refined 3D model of Lynch Cove. This model would have a finer scale grid and would include shallow waters excluded from the recent 3D model, which would allow for better assessment of both nearshore and offshore processes.

We would caution against over-generalizing effects on seagrasses from studies in other areas of the world. Some of the areas referenced clearly have far higher development near the aquatic resource compared to the rural areas of Hood Canal, where marine nitrogen sources dominate all loading calculations by all researchers to date. Nevertheless, we support site-specific assessment of impacts on seagrasses, including potential impacts from nutrient releases.

Comment from Jan Newton (UW):

Excerpt:
Figure 7: Average dissolved oxygen concentrations in Central Hood Canal (aggregate of all stations, depth > 20 meters)

Comment on excerpt: Not "all" stations, just the 6 shown on web.

Response:
We have edited the title of the figure to note that it is an aggregate of 6 stations.

Comment from Jan Newton (UW):

Excerpt:
Tidal mixing may be significant in Lynch Cove, where the mean tidal range (2-3 meters) is a significant fraction of the mean depth (18 meters) (Brett, personal communication, 2012).

Comment on excerpt:
There should be a NOAA tide ref for this.

Response:
We are referencing a personal communication from a Hood Canal researcher in this instance, not our own investigations.

Comment from Jan Newton (UW):

> Excerpt: *While low dissolved oxygen is a concern in many areas of Puget Sound, Hood Canal is a primary location of interest because of multiple fish kill events that were attributed to extremely low dissolved oxygen levels. Fish kills have been associated with wind events that align with the axis of the Canal and lead to upwelling of hypoxic bottom waters into shallower depths where biota are not accustomed to such low oxygen levels (Newton et al., 2011c; Kawase and Bahng 2011; Kawase 2007). The fish kills have generally occurred in September on the southwestern shore of Central Hood Canal (from Lilliwaup to Potlatch), which is substantially deeper than Lynch Cove.*
>
> Comment on excerpt:
> Should say in second sentence of first paragraph that there are two kinds for biota kills.
> Should include the biota kills observed by Skok Tribe and UPS with camera showing mats and dead crab in the Lynch Cove area.

Response:
We added language as noted that diver video surveys by the Skokomish Tribe have observed bacterial mats and dead crab in the Lynch Cove area (Newton et al., 2011a). We have also referred to episodic fish kills throughout the report to distinguish from any impacts due to chronically low dissolved oxygen.

Comment from Lower Hood Canal Watershed Coalition:
Change and/or use consistent language to describe lower Hood Canal referred to as "Lynch Cove" in your report. For residents of Hood Canal, Lynch Cove is a relatively small area of the lower Hood Canal, which extends from Belfair State Park down to Belfair, approximately 1.5 miles. In this report, Lynch Cove is used to describe the lower "arm" of the Canal, which is closer to 18 miles long.

Response:
We recognize that different researchers have used different definitions in the past, and that local residents have their own accepted location definitions. We used consistent language for Lynch Cove to refer to the area east of Sister's Point for purposes of this report. We regret that this conflicts with the definition accepted by the Lower Hood Canal Watershed Coalition.

Comment from Allan Devol (UW):
Figure 8, Page 16. The y axis says "DO < (less than) 11 m (mg/l)". I think you mean > (greater than).

Response:
This figure was obtained from Michael Brett (UW). It is actually not a typo, and we have added this clarification to the figure title: "Y axis is monthly mean DO (mg/L) from 11 meters depth to the bottom".

Comment from Allan Devol (UW):
In the first paragraph on page 63, you say "Based on a synthesis of available estimates and the median estimate from a Monte Carlo analysis, we estimate that the mean human impact in the summer in Lynch Cove is approximately 0.06 mg/L (range of 0.3 to 0.23 mg/L). I am pretty sure you mean range of 0.03.

Response:
This was a typographical error as noted. The range is 0.03 to 0.23 mg/L.

Comment from Allan Devol (UW):
Finally, the HCDOP references are available on the HCDOP website. It would be nice if all the Brett references (mostly e-mails) were available on some website also.

Response:
All of the references have been and will continue to be available upon request to EPA and Ecology.

Comment from Mason County Public Health:
Also, in regards to potential nutrient sources in Hood Canal, it seems that fertilizers (both residential and commercial) seem to be lacking from the sources. It is unclear as to if that is because they are considered negligible sources.

Response:
Total watershed loadings are based on measured flow and concentration at tributary mouths, but identification of the sources contributing to this total loading has required the use of statistical models. The statistical analysis of Steinberg, et al. (2010) identified two variables, population density and mixed deciduous forest (red alder), as the best estimators of nitrogen concentrations in tributaries. Population density is a "catch all" variable that encompasses loadings from all human activities, including both OSS and fertilizer use. Available research has tended to focus on OSS sources as the main contributor, but fertilizer use may also contribute nitrogen loadings to Hood Canal. In general, we would join the researchers in focusing primary attention on OSS, but this comment points to an important omission in the language of our report. We have revised the tributary loading section to recognize non-OSS sources such as fertilizer application.

Comment from Mason County Public Health:
Page 39, Table 5: Below the table is a reference "[3] Summer contributions…". However, from what I can find, no [3] is located in the table.

Response:
This footnote was included erroneously and has been removed. We appreciate the thorough review by Mason County.

Comment from Mason County Public Health:
Page 40, Table 6:
There is a column that references the number of homes and in other places in the document developed properties are referenced. It would be great to include information about how each study determined if there was a home (not in the table). There are other locations within the review that reference "developed" parcels. Since each county assessor's data is different, it seems like an important part of this would be to include information somewhere on how each study determined structures/development on a property.

Response:
The primary focus of our report was estimation of human impacts, including impacts from onsite sewage systems. We report estimates from researchers based on population-based (per capita) calculations, but our best estimates of human impacts are not based on this method of analysis; rather, they are

based on the alternative method of sampling/analyzing shoreline seep data. We do not disagree that more could be written about details of the population estimates, we have placed more priority on adding detail on shoreline seep data in response to information from Mason County.

Comment from Mason County Public Health:
Page 64, Paragraph 1 (at the bottom of the page): "In this case, we selected a typical range of uncertainty in laboratory analysis for nutrients (+/-20%), because this uncertainty was higher than the uncertainty in the flow and model correlation." Please cite the source of the 'typical range' of uncertainty?

Response:
The source for this analytical range is an Ecology report on analytical precision in TMDL studies in Washington (Mathieu, 2006) that we have cited and added to the reference list. The range used (+/-20%) is actually conservative (most studies achieve lower standard deviations in replicates), and we have substituted "conservative" for "typical" in the language cited.

Comment from Mason County Public Health:
Page 65: Table goes off the page

Response:
This typographical error has been corrected.

Other Information Received During the Comment Period

EPA and Ecology received other information during the comment period that was relevant to the Hood Canal report.

New Published Study on Effects of Hood Canal Bridge
A new study by researchers at the Pacific Northwest National Laboratory (PNNL) was published in the journal Applied Ocean Research (Vol. 39, 2013) titled "Potential Alteration of Fjordal Circulation due to a Large Floating Structure – Numerical Investigation with Application to Hood Canal Basin in Puget Sound" (Authors: Khangaonkar, T., and Wang, T.). This study analyzed the potential effects on circulation and residence time of Hood Canal waters by the Hood Canal Bridge. Using the FVCOM hydrodynamic model, the researchers present initial estimates that the bridge could increase residence time of Hood Canal waters by 8-13%.

Response:
Our draft report noted the dearth of information about potential effects of the bridge on circulation, so this new paper provides valuable insights. The estimated magnitude of the change to residence time is large enough to warrant inclusion of the bridge circulation effect in future studies of cumulative dissolved oxygen impacts. At the same time, the analysis is a preliminary step in analyzing bridge effects, and it does not include analysis of oxygen effects. We have updated the uncertainty section of the report to cite the new paper.

Comments published in the Seattle Times
The following comments were published in the Seattle Times on November 28, 2012.

Op-ed: Hood Canal still needs a pollution fix

Although septic waste was not linked to fish death in Hood Canal, we should still work on reducing pollution from septic tanks, writes guest columnist Michael Brett.

By Michael Brett
Special to The Times

AFTER four years of intense scientific debate, a recent report found weak evidence linking septic-system waste to fish kills in Hood Canal.

As an environmental scientist involved in key studies on human impact and nitrogen input to the Hood Canal, I'm now wrestling with new questions. Does the new finding mean we should abandon efforts to control discharges from septic tanks to the canal?

While a costly program to replace septic systems in upland areas of the watershed no longer seems warranted, there are prudent measures that we can take short of building large-scale sewage-treatment plants, and good reasons why we must do more to control pollution in the canal.

For years, studies focused on the low concentration of oxygen in the canal, which caused periodic fish kills in the Hoodsport area.

A report released in September by the Environmental Protection Agency and the Washington Department of Ecology concluded that roughly 98 percent of the missing oxygen in Hood Canal water is a consequence of natural factors — in particular, poor circulation and large inputs of marine nutrients.

Reducing septic-system nitrogen discharges into Hood Canal is therefore unlikely to lessen the frequency or severity of fish kills. Furthermore, historical patterns in Hood Canal sediments documented by a NOAA research team show low oxygen concentrations are characteristic of this estuary going back at least 400 years.

Is that the end of the discussion? Should we ignore water quality in Hood Canal?

Not in my opinion. As an aquatic scientist, I see this as an opportunity to review our strategy. As environmental stewards, we have a professional responsibility to support the most cost-effective programs to reduce pollution.

Failing septic tanks near the shore pose a considerable risk to people swimming in the canal and consuming seafood caught there. The EPA and Department of Ecology also noted that cleaning up high-risk septic systems is still very important for protecting the health of Hood Canal.

Research from my lab has shown that some of the septic systems near shore have tidal water reaching all the way into their drain-fields, where the wastewater infiltrates the soil. This puts human waste and any associated pathogens directly into the marine environment.

During peak high tides, this can also cause untreated sewage to back up into low-lying homes. The households that have tidally influenced drain-fields are the most at risk of having overflowing toilets and contaminated shellfish on their beaches. Septic systems a quarter-mile or more from the shore are less of a risk.

Effluents released into the soil from septic systems in the uplands take several weeks, or more, to reach Puget Sound. This longer travel time means pathogens are much more likely to die before they reach the Sound.

But for homes near shore, we can reduce risk by building cluster sewage-treatment systems that can serve a dozen homes using innovative designs that remove both nutrients and pathogens. Researchers at the University of Washington and Washington Department of Health are already collaboratively developing effective, low-cost, nonproprietary septic-system designs.

Hood Canal and Puget Sound management activities should now focus on failing septic systems near the shore. These systems pose by far the greatest risk to surface-water quality and environmental and human health.

The Hood Canal regional agencies — Mason, Kitsap and Jefferson counties — have made great strides in identifying the highest-risk septic systems, and these efforts should be supported and intensified in the future.

Michael Brett is a professor of civil and environmental engineering at the University of Washington.

Response:
These comments raise reasonable questions in follow-up to our science review. We support the work of Michael Brett and others to identify the highest risk sources of pollution to Hood Canal. Since the review is focused on scientific analysis and not restoration strategy, we would defer on Michael Brett's policy recommendations on future management actions to the Hood Canal Coordinating Council.

We offer a few clarifications on statements in this Op-Ed:

- Michael Brett writes that our report "concludes that roughly 98 percent of the missing oxygen in Hood Canal water is a consequence of natural factors — in particular, poor circulation and large inputs of marine nutrients." While we did conclude that the vast majority of nitrogen in the surface waters of main stem Hood Canal is marine-origin, we do not report a "98 percent…missing oxygen" value. Michael Brett has apparently calculated this value from information in our report.

- It was technically not a "NOAA research team" that analyzed historical patterns in Hood Canal sediment cores. Rather, Pacific Northwest National Laboratory (PNNL) led a research team in these studies under a grant from NOAA.