North Totten Inlet Mussel Farm Proposal

Review of water column aspects of Taylor Resources consultants' reports

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Contents

Summary

I reviewed several reports regarding water column and environmental effects of proposed mussel rafting in Totten Inlet. I also examined all the reports for physical oceanographic topics as it is impossible to separate these physics from biology in such circumstances and I do have extensive experience in physical studies for aquaculture siting and impact assessment.

The information provided is mostly of high quality and answers many of the basic questions of near and far field effects that are needed to produce a useful EIS. Most of my comments are detail-oriented, asking for clarification and correcting errors or omissions that are not highly important. However, there remain two additional sets of calculations to be made to complete the needed estimates. I recommend these calculations be added to the NewFields Northwest report as follows:

The first is the calculation of summer and early fall mussel nitrogen excretion and the resultant biomass of phytoplankton and associated dissolved oxygen demand from decomposition or respiration of the same phytoplankton. Annual estimates of nitrogen production are presently included in the NewFields Northwest report, but there is no breakout by season including the critical summer and early fall period only. The existing data should be parsed into the appropriate time periods of late fall/winter, spring, and summer+early fall with emphasis on the summer and early fall and worse case conditions at that time. In each time period it is most important to highlight the differences between removal of nitrogen into mussel tissue and denitrification losses in the sediments versus addition of excreted, dissolved nitrogen and its possible fate as mentioned above. The effects of vertical stratification, although less pronounced at the proposed site than further south inside the inlet, should be considered in this analysis relative to the vertical location of the mussels.

The questions to be answered may be stated as follows: will the proposed mussel raft project significantly increase the population of dinoflagellates or other types of phytoplankton during the summer/early fall period? Or will the beneficial effect of nitrogen removal and denitrification outweigh the excretion effect? The currency to be used in the analysis is both nitrogen and oxygen. What will be the extent of these effects temporally and spatially (in general), and what will be the effect on subsurface dissolved oxygen levels?

The other calculation not performed is overlapping with the above and involves the biological oxygen demand of waste solids and how that might affect subsurface oxygen concentrations during summer. This estimate can be made from existing bioenergetic information for mussels, available from the literature. The estimate can be made from carbon and oxygen stoichiometry and mass balance estimates of the amount of oxygen needed by benthic invertebrates and bacteria to assimilate (into biomass) and respire the mussel-origin waste carbon while assuming loss to burial in sediments is nil. The oxygen demand can be quantitatively compared with the flux of water over the affected area to illustrate the potential reduction of dissolved oxygen to provide the range of effects from normal to low oxygen periods.

During the critical summer periods when the inlet's waters are most quiescent and vertically stratified, most of the phytoplankton species biomass is composed of dinoflagellates that are

capable of vertical migration. Given the degree and depth of the nutricline (probably matching the thermocline), these species are not fully "nutrient limited" in terms of population growth as they migrate to the deep layer to obtain nitrogen at night. There is a bioenergetic cost to this type of vertical migration and by adding N to the upper water column there will be faster growth of the population.

It may not be possible to model or project the answers to each of these questions precisely, but I believe it is possible to use existing data to show the general results; i.e., no effect, adverse effect, or positive effect of mussel rafting.

From other locations and studies, I believe the results of the above will show that there is a beneficial effect of mussel feeding to remove nitrogen. This affect may couple with denitrification in the sediments to outweigh the potentially adverse effect of dissolved nitrogen excretion. But there could be a localized drawdown of subsurface or sediment interstitial dissolved oxygen in the immediate vicinity of the proposed farm. The degree of that effect can be minimized and mitigated by sizing the operation properly to maintain the surficial sediment layers as aerobic, thus promoting denitrification and other beneficial effects such as maintenance of the invertebrate infauna and demersal fishes.

Introduction

I believe there are many useful observations and analyses in the following reports that will help in preparation of the mussel raft EIS. Our fiduciary responsibility as independent technical reviewers (ITRs) was to provide guidance on scope of needed field work and to scrutinize draft reports for any errors and interpretation of results. I emphasize that many of the corrections and suggested amendments to the consultants' reports are to help the EIS writer construct the most accurate assessment of existing condition and possible changes resulting from the proposed project. The large number of overlapping reports presented some difficulty in assessing the probable effects of the proposed project. As my colleagues have pointed out, it would have been better to integrate the reports, particularly by topic.

I reviewed these reports as if they were to be published in the scientific literature, challenging statements or interpretation I felt inaccurate or misleading. In some cases I have suggested alternative interpretations or method of analysis. All of the reports I have reviewed are considered draft reports and should be amended and corrected for errors or suggestions that the ITRs have identified.

I do recommend that the consultants provide a summary list of reports with major topics and studies covered in each for use by the EIS preparation team. Presently, the topics are scattered around in different reports and the EIS process would benefit from a coherent guide to what was done and where the details could be found. The summary provided does not accomplish this.

I believe the literature shows that Totten Inlet is nutrient-sensitive during a portion of the algal growing season, but it is composed of a gradient from highly sensitive in the shallow, distal end to less sensitive in the northern, more vertically mixed northern end near the mouth. By

nutrient-sensitive I mean that any additional nutrient added to the mixed (surface) layer during those portions of the growing season when nutrient concentration and flux is low may result in rapid uptake and growth by phytoplankton and possibly other forms of algae. Some level of algal production is desirable to fuel the food web, but excessive amounts are of course undesirable as that may involve excessive turbidity, light interferences with submerged aquatic vegetation (e.g., eelgrass), dissolved oxygen depression related to bloom senescence or even exacerbation of harmful algal blooms.

The shorelines of Totten Inlet are now nearly completely urbanized; therefore, the chances of anthropogenic enrichment of the macronutrient load are much increased versus what they were several decades ago. Mussel farm could serve as a means to reduce the nutrient load in total, which may be a valuable environmental service, but I wanted to see more focus on how that would occur during the nutrient-sensitive summer and early fall periods, as discussed below. All of the consultant reports dealing with this concept would do well to cite west coast examples by Cloern (USGS) and colleagues that demonstrates how bivalves can control phytoplankton populations (Cloern et al. 1985),[1] and how changes in predator abundance driven by climate cycle and reduction of shellfish led to increasing phytoplankton blooms (Cloern and Jassby 2007)[2].

Below I review two reports relating to water column ecology and in particular nutrients and algae. By necessity, I first include comments regarding physical oceanography, as this field is intimately tied to water column ecology and I am experienced in these pursuits. The physical oceanography topic is reported in several of the consultant documents.

Physical Oceanography and Flushing Rate (Several reports)

No single report deals with all the physical oceanographic aspects of the proposed project, but here I provide comments related to our prior recommendations and what was performed.

Detailed physical oceanographic study protocols were prepared by the ITRs and sent as formal correspondence to Taylor Resource Inc. several years ago[3]. These protocols were designed to a) more accurately determine the flushing rate of Totten Inlet both in general and b) provide a refined view of water transport near the proposed mussel raft site. These data would then be used to more accurately predict effects of the proposed project on dissolved oxygen, nitrogen and phytoplankton dynamics in the inlet and perturbations from the proposed project. In preparing these protocols, we set forth methods that would determine the average "tidal excursion" of waters passing through the site as well as the actual volume of water exchanged through the mouth of the inlet. We believed that our recommendations represented an

[1] Cloern, J. E., Cole, B. E., Wong, R. L. J., Alpine, A. A. (1985).Temporal dynamics of estuarine phytoplankton: a case study of San Francisco Bay. Hydrobiol. 129: 153-176

[2] Cloern, J., A. Jassby, J. Thompson and K. Hieb. 2007 . A cold phase of the East Pacific triggers new phytoplankton blooms in San Francisco Bay. *Proc. National Academy of Sciences, 104*(47), 18561-18565.

[3] Response of the Independent Technical Review Committee (ITRC) to Thurston County and Taylor Resources Regarding Letters of Comment Received in March 2002 During the Process to "Refresh" the Scope of EIS, dated August 20, 2002 attached as appendix 1.

optimum scope and benefit for the anticipated costs. After completing the 8/20/08 memo we were left with the impression that the work was being planned.

Consultants conducted current meter recording with bottom-mounted ADCP at the proposed site as well as some cross-channel profile using over-the-side (OTS) ADCP. These are useful data; however, they are not described in total in one report, but rather at several locations in different reports by different authors (EHI and Brooks). Also drogues of different depths were tracked from the proposed site through a single tide exchange. These studies provide useful information that is adequate for site characterization but our suggested protocols were not followed in every case.

We had suggested installing four or five fixed ADCP profilers at the mouth for an extended period, but OTS meter use for a series of tidal periods would have also potentially been adequate if tidal elevation was monitored. Instead, monitoring of cross-channel flows was conducted for nearly 12 hours across four different transects. This work was done for most of a relatively large flood tide and the first half of a much smaller ebb tide. Unfortunately, these data cannot be used to estimate tidal flushing as the tides were not near average, were unequal and the ebb tide was not monitored in total. Instead, the data provide some general information about how flow rates and directions vary across channel at the four locations.

Drogue studies were done by releasing drogues from near the proposed site, but we had suggested that this work be done during periods of mean tidal exchange so as to assess average tidal excursion range and paths. Drogue surveys are only useful when analyzed in relation to the tidal exchange during sampling, which of course is still possible. However, drogue surveys for estimating reflux of ebb tide water leaving the inlet and returning on the following ebb tide were not conducted that would have allowed a more accurate estimate of inlet flushing.

As a result of the above, the actual flushing characteristics of the site of the proposed action in Totten Inlet remain somewhat undefined despite the studies that were conducted. It is useful to revisit the purpose of the physical oceanographic studies to evaluate if sufficient information is now available for this topic. We know that flushing rate studies are conducted with regard to projects of any kind that may have significant or measureable far field effects. Flushing rate studies are often of importance to dispersion and dilution of some contaminate that is conservative or refractive in the ecosystem. This is not the case with mussel culture where nutrients, notably nitrogen, can be sequestered into the flesh of the mussels, buried in sediments, cycled through the food web or denitrified and vented to the atmosphere as nitrogen gas. And most importantly, far field water column effects (greater than 30 to 70 m distant from a mussel raft) are typically not measurable unless there are a series of mussel raft farms in relatively close proximity. This would not be the case for Totten Inlet for the proposed action. As pointed out by Roger Newell, two other separate assessments of flushing rates of the inlet in total were prepared and the results are somewhat comparable.

In the case of nitrogen or phytoplankton (chlorophyll *a*) concerns, if the content in the source water (outside Totten Inlet in South Puget Sound [SPS]) was significantly different from the inlet, there could be a necessity to more accurately determine the flushing rates. But there is no evidence of this. Other inlets and channels in SPS are likely similarly replete or depleted of nitrogen based on the literature and our experience. One of the reviewed studies by Kenn Brooks showed no significant difference in chlorophyll *a* content on flood versus ebb tide

sampling just inside the mouth of the inlet. Although there were only 3 or 4 sets of measurements during the nutrient-sensitive algal growing period, it helped to have winter period measurements for comparison.

Another consideration is that all of SPS (south and west of the Tacoma Narrows) is generally considered relatively poorly flushed. The best data is still from Cokelet et al. 1990,[4] and these data show very minimal runoff or transport in SPS (at the Devils Head cross channel transect). Accordingly, prior studies of nutrient response of phytoplankton in SPS (Kiefer and Atkinson 1989[5], see WDF 1991[6]) used these data to model South Puget Sound conservatively as an entirely closed system that did not exchange with the main basin or the ocean. The physical oceanographic work, nutrient studies and impact assessment of the NewFields Northwest report indicate that the degree of effect is limited to near field distances and since there is a net removal of nitrogen by such a proposed project, the lack of highly accurate flushing rate information is regrettable, but not a major impediment. I recommend that the report deal with this issue (lack of very accurate flushing rate estimates by subarea of the inlet) by noting that approximate estimates are available and the system can be dealt with as a closed system with no real significant flushing with the central basin or the ocean to the first approximation. If this is done, the issues of estimated reflux rates, flood versus ebb tide water quality differences in north Totten Inlet and related matters are less important from a practical and regulatory (but not academic) point of view.

Report No. 2: An assessment of potential water column impacts of mussel raft culture in Totten Inlet. Prepared by NewFields Northwest.

A. Overview

The Independent Technical Reviewers (ITRs) previously reviewed a report by these consultants regarding the same topic and provided extensive suggestions. This version of the report looks quite different than the prior report and overall I think it is quite an improvement. The report provides a detailed discussion of the anticipated changes and effects of the proposed mussel raft operation. These authors synthesized information from others and did an admirable job. They were not responsible for field work or other types of work such as the physical oceanographic or chemical and biological assessments, but worked with what was available.

Overall, the report will provide a good basis for the water column portions of the environmental assessment after a few more calculations and resulting tables are provided. Also there are a number of typographical errors, incomplete sentences and missing references. All of these must be corrected, and I recommend attention to the major points made below. Not included here

[4] Cokelet, E.D., R.J. Stewart and C.C. Ebbesmeyer. 1990. The annual mean transport in Puget Sound. NOAA Technical Memorandum ERL PMEL-92. Seattle.

[5] D.A. Kiefer and C.A. Atkinson. 1989. The calculated response of phytoplankton in South Puget Sound to nutrient loading by the Swecker Salmon Farm. Prepared for Swecker Sea Farms, Inc. Tumwater WA. and presented to the Washington State Shoreline Hearings board.

[6] Washington Department of Fisheries. 1991. Final programmatic environmental impact statement fish culture in floating net-pens. Prepared by Parametrix Inc, Rensel Associates and Aquametrix Inc. for Washington State Department of Fisheries, 115 General Administration Building, Olympia, WA. 161 p.

are several editorial comments and corrections included as Adobe Acrobat "sticky notes" on the original PDF text. Below I address the content and important remaining issues in categories recommended by EIS writer, Vicki Morris.

B. Technical Observations and Applicability to Environmental Review of the Proposed Action

As stated above, the report includes a number of important calculations and summaries of data that are important to reviewing the proposed action. The report provides a basis for comparison of the flux of nitrogen, phosphorus and carbon budgets in the inlet and the impact of the proposed mussel farm on these fluxes as well as the phytoplankton dynamics and food web effects. The new draft report dispenses with the Ecopath model previously used and rather uses a simple spreadsheet accounting method to deal with tropic level (food web) flux. In our opinion, this simplification results in a more understandable and useful method for the type of analyses needed in this proposed action.

C. Possible Technical Errors, Deficiencies, and/or Issues of Concern

I do not consider any of the following comments to represent a fatal error or something that cannot be addressed in the final version of the report. But none are trivial and should be corrected or at least addressed.

C.1. Assessment of summertime Nitrogen flux. An annual nitrogen budget (section 4.3.3.3) and the resulting phytoplankton response (section 4.3.3.6) are represented in the report and these are important, but I believe the budget and phytoplankton response should be separated into late fall/winter, spring and summer plus early fall to capture the algal growing season (approx. April through October), and nutrient sensitive periods (summer and early fall) separately. This is the crux of the nutrient sensitivity issue, and is not presently included in the report. It should not be difficult to do.

Nitrogen removal, section 4.3.3.5. This discussion neglects other important sinks of nitrogen from remineralization including denitrification of nitrate to nitrogen gas and burial. You may wish to make these points that significantly reduce the total available total and dissolved N pools from sediments even if stated elsewhere that you couldn't estimate it.

Phytoplankton response 4.3.3.6. This section should address a key point..... it is inescapable that DIN added to the surface layer during most of the summer will result in additional phytoplankton biomass and possible D.O. depression when those cells become senescent or are converted to fecal pellets by grazers. It is also relatively easy to calculate that response, and that should be done and weighed against the reduction of N from tissue assimilation by the mussels as well as losses of N to nitrogen gas (denitrification) from sediment-deposited phytoplankton. We consider this a major oversight. The fact that you can't measure an increase in N downstream by 70 m means little, it merely means that the N has been diluted or the measuring technique is not representative, not that it is gone.

C.2. Tidal exchange on field study days. Throughout all the reports relating to the proposed action there are various physical studies of water motion but no data is provided explaining what kind of tidal exchange occurred during these studies. For example, what was the tidal exchange related to the mean and diurnal ranges known for at least one of the two locations

available in Totten Inlet? This was not the charge of these authors but rather other consultants; nevertheless, this should be included in this document.

C.3. Downstream wake of rafts. The discussion on page 13 indicates that the shape and extent of the downstream plume does not vary with changes in current velocity. The accompanying figures do not indicate that and it is not intuitive. Please amend or explain the statements in the second paragraph. See notes associated with confusion on the last paragraph of that page too.

C.4. Dissolved oxygen records. The first paragraph of the DO section appears to be in error regarding the available annual records from the Dept. of Ecology which are more extensive than reported. It would be useful to explain why the lowest DO records were observed in August and October. I note that the monitoring of Brooks showed peak chlorophyll a levels in August, so these data could be from algal (probably dinoflagellate) respiration. This is also a potential sign of eutrophication; i.e., large enough blooms that crash and cause DO depression. Why would October be the other low-value month? Could it be due to turnover of the inlet due to wind-induced loss of vertical stratification?

C.5. Field measurements during flood tide only. Page 21. If the field measurements of PSI were made on an ebb tide, would it be any different? I am not convinced that measurements on the flood only was a good thing, and know that was PSI's decision, but what can you say to address or mitigate that? If it isn't an issue, please say so. I can see it varying both ways, higher incoming DO during blooms in the inner inlet but perhaps lower incoming DO during the short period when the inlet undergoes fall turnover (full vertical mixing due to winds, weather and tides). To possibly address these issues, the report should reference work by Brooks that compared conditions at the mouth of the inlet to outside the inlet in Squaxin Passage.

C.6. Downstream DO measurements and standing stock biomass. DO concentrations return to nearly normal by 30 m downstream of very large salmon net pens under worst-case conditions (late summer, maximum biomass of several million pounds, low ambient DO, Rensel 1991). Based on probable biomass and respiration comparisons, it would seem that DO would rebound much before 70 m downstream of these mussel cages, why was this not the case? What was the mussel biomass when DO was measured by PSI in 2002 and 2003, and how would it compare to the proposed mussel raft site in NTI? The DO reduction data are relatively meaningless without the biomass loading data.

C.7. Nearfield effects Throughout the report the distinction is blurred between the observed immediate downstream effects and the missing calculations of phytoplankton production that would occur due to N addition during N limited periods. This section label is unintentionally misleading.

C.8. Fouling community, section 6.3.3. The statement is made that anemones are non-selective filter feeders, which is a concept that probably originates in one of the Brooks reports. Unfortunately, this is incorrect. Anemones are not non-selective feeders according to one of the world experts on the group, Jennifer Purcel. I also found this in work at fish farms..... "*Surprisingly there was no effect for anemones on floats. This suggests they are utilizing other, non-fish farm sources of food and because of their long-lived nature, these other sources are likely persistent and substantial. Waste pellet analyses indicated that Metridium senile feeds mainly on diet of copepods, polychaete larvae, bivalve and gastropod veligers, copepod nauplii,*

and barnacle nauplii and cyprids (Purcell 1977 and others) and many of these plankton are the product of recruitment over a large space and time that would not be affected by a local source of nutrients like a fish farm. All of the anemones were from floats and hence are slightly further away from the farmed fish but this should not have been a major effect. [7] "

The analysis in this section should be recalculated or amended as a result of the selective nature of anemone feeding. [8]

C.9. Summary, seasonal growth rates section 6.4 This doesn't make sense, Figure 51 shows dramatically lower growth rates in winter and early spring. In a non-food-density-dependent habitat, one would expect lower growth rates when temps are lower and specific growth rates naturally decrease with all organisms with increasing size and age (Wenberg growth fundamentals). Moreover this seems awkward. Do you mean.... *Because growth of mussels persists in the winter when primary production is dramatically less the relative importance of POM for mussel consumption is likely to increase. Because phytoplankton biomass is halved in the winter it is reasonable to assume that* ..... ? But again, I don't think it is reasonable. Why not calculate required phytoplankton to sustain the observed growth rates and leave it at that?

C.10. Summary, impact and significance of rafts on primary productivity Section 6.4 I believe this is mislabeled; it is not the effect on primary productivity (which is a rate function), but rather the effect on standing stock or biomass of phytoplankton. This is a fundamental difference that is not trivial.

Missing entirely from this section is the estimate of NET shift in standing stock or production of phytoplankton as all you have been discussing here is simple removal. You need to assume that 100% of excreted N and some proportion of denitrified N from the benthos will rapidly become phytoplankton biomass via the nitrogen discharge and uptake within the nutrient stressed system.

C.11. Reference Documents Not Provided, and/or Not Peer-Reviewed

Many references are not included. Unpublished citations should be annotated adequately for others to be able to access them.

D. Editorial Observations and Comments

What is the "PSEP model" referred to on page 8? Is this a confused reference to Rensel Associates and PTI Environmental Services 1991? No new model was reported in that reference, just a general formula for a tidal prism model.

The third and fourth paragraphs of page 9 are quite esoteric and should be decoded to more understandable English. You may want to say something to the effect that this amounts to a

[7] Rensel, J.E. and J.R.M. Forster. 2007. Beneficial environmental effects of marine net pen aquaculture. Rensel Associates Aquatic Sciences Technical Report prepared for NOAA Office of Atmospheric and Oceanic Research. 57 pp. http://www.wfga.net/documents/marine_finfish_finalreport.pdf

[8] Purcell, J.E. 1977. The diet of large and small individuals of the sea anemone *Metridium senile*. Bulletin of the Southern California Academy of Sciences 76: 168–172.

simplification and the grids do not fit the bathymetry or topography exactly as a result, but it is good enough for the simulation.

Page 23. Amino-N Are you referring to free amino acids or something else? If excretion is dominated by FAA (and the references I checked by Bayne indicate the same) then why wasn't this measured by Cheney et al. or at least total nitrogen in lieu of it? You may wish to note that urea was not measured but based on the same reference is not expected to be a significant contributor to the waste dissolved nitrogen pool.

Page 24. The Rensel 1991 citation is actually Rensel Associates and PTI Environmental Services. 1991. Nutrients and Phytoplankton in Puget Sound. Prepared for U.S. Environmental Protection Agency, Region 10, Seattle. EPA Report 910/9-91-002. 130 pp. Please cite it correctly. All references you have not seen (that are cited by somebody else) should be noted as such in the references cited. A number of other edits are offered for the nutrient sensitivity section of this page in the marked up PDF I can provide.

Page 24 N:P ratios. The average was and is 16:1, you cited a RANGE of N:P ratios for different species on a MOLAR basis (not by weight). It is always important to note the units. There is no Newton 2005 in the literature cited but Jan has not revised the Redfield ratio; rather, this is an attempt to cite the Molar (16:1) versus weigh (~7:1) ratios. In any event, the range cited 15:1 to 5:1 makes no sense. It is best to cite the original literature and recent discussion of it.[9]

Report No. 3. Supplemental study of dissolved nutrients and particulate organic matter in the waters near the proposed mussel farm in North Totten Inlet, Washington State, USA by Kenneth M. Brooks

A. Overview

For the purposes of the EIS that will be prepared, this report provides several important sets of information regarding dissolved (inorganic) nutrients and particulate organic matter. Anyone familiar with Totten Inlet knows that it is like other SPS inlets in having a gradient of warmer, more vertically-stratified waters further toward the shallow, distal end during the summer and early fall. This report provides quantitative evidence of that condition which is important because the proposed project and its measurable effects will be in the northern end nearer the mouth of the inlet that is more suited to raft culture than the southern end in my opinion. Historical and prior sampling of water quality was more centrally located which tends to overstate the degree of vertical stratification and nutrient sensitivity. The report also provides reinforcing evidence that the inlet is rich with phytoplankton production but may not be significantly different from adjacent water bodies.

[9] Arrigo, K.R. 2005. Marine microorganisms and global nutrient cycles , Nature, Vol 437, pp. 349-355, 2005 Redfield A.C. 1934. On the proportions of organic derivations in sea water and their relation to the composition of plankton. In James Johnson Memorial Volume. (ed. R.J. Daniel). University Press of Liverpool, pp. 177-192 .

I believe much of this information is useful for the EIS, but there are errors or omissions as well as faulty interpretation of several key aspects. Some of the figures are hard to understand, particularly the legends representing the source, timing and location of the data represented. I believe the report should be revised to deal with these issues.

B. Technical Observations and Applicability to Environmental Review of the Proposed Action
C. Possible Technical Errors, Deficiencies, and/or Issues of Concern

I combine these topics as it is more expeditious to deal with them sequentially by subtopic as follows:

Dissolved Nitrogen Measurement and Interpretation

Totten Inlet dissolved inorganic nitrogen (DIN) concentrations were slightly lower at the mouth of the inlet on ebb tide than during the flood tide during sampling from August to November 2005. The author asserts that this suggests that Totten Inlet was a "net consumer" of dissolved inorganic nitrogen. This may be true for dissolved nitrogen, but it is important to consider nitrogen in total. Similar statements were made about phosphorus which are also not valid for the similar reasons (i.e., Total P was not measured).

Total nitrogen (all dissolved and particulate forms) was not measured (from which dissolved can be subtracted to estimate particulate N + urea or other non DIN forms), so it is not conclusive to assume that the inlet was a net consumer of nitrogen overall. Nitrogen may flux rapidly from dissolved to particulate form in the food web during the nitrogen cycle. I also suggest emphasizing (as it already discusses in other sections) losses of particulate or dissolved N could be due to a combination of phytoplankton production, grazing, sedimentation and loss to the atmosphere of nitrogen gas.

Furthermore, I believe it is important for the report to note that such a flood versus ebb tide analysis conducted on the same day suffers from inevitably re-sampling of the same water later when it returned during the flood tide. The ITRs believed it was important to determine reflux rates and provided an easy method to establish the rate in our 2002 memo, but no estimates of reflux (i.e., return rates) were conducted, so the matter remains uncertain. My guess is that reflux would have been relatively low given the strength of tidal flow and mixing in Pickering and Squaxin Passage, but I have no means to be certain.

No information was given on tidal amplitude for the sampling periods in this report. I may have missed it in some other document. This may not be essential in this case, but it could be if the tides selected were the lesser of the exchanges in this mixed semidiurnal tidal area.

Phytoplankton Abundance or Density

Chlorophyll *a* pigment, an approximate measure of phytoplankton density, was not significantly different during the flood versus ebb tide at the mouth of the inlet. Given the sampling strategy discussed above, this is not too surprising as the water being sampled includes some unknown amount of refluxed inlet water.

In the section "Food for cultured mussels in Totten Inlet" (page 23), the statement is made that "*The dry weight of phytoplankton is generally equal to 50X its chlorophyll a content*". The report then goes on to use that factor to estimate phytoplankton concentration in dry weight. No basis for the statement is provided, and it is not correct that the dry weight of phytoplankton is ever equal to 50 times its chlorophyll a content. The currency typically used to estimate phytoplankton standing stock or production is carbon and an appropriate carbon (not dry weight) to chlorophyll a ratio could be at least 50:1 (but varies highly with species and physiological state). The carbon content of the dry weight of phytoplankton varies, but would be only about 1/3 of the total dry weight, so the estimate shown in Figure 21 of the subject report is inaccurate by a factor of three.

I recommend that this section include more embellishment of the important point found much later in the recommended monitoring section… *" As long as TVS and Chl a are not significantly (α = 0.05) less on the ebb tide in comparison with the flood tide, the estuary is not near its carrying capacity".* This is a cogent point that needs amplification, and is more convincing than the models and spreadsheets cited later that are subject to large unknowns and possible calibration errors.

Station location representation

A simple matter, but it is difficult to interpret parts of this report as many of the figures or tables refer to "North Totten Inlet" yet there were two locations sampled within that area, the site of the proposed farm and near the mouth of the inlet. Table 1 shows the universe of locations, dates and depths, but some are as diversely located as Skookum Inlet (toward the south end of the inlet). The final report should also be amended within each parameter results and discussion to clarify this. The labeling should be consistent, for example, the area map (Figure 1) does not include the term "reference location" but some subsequent plots do. Although not shown in Figure 1, apparently this means Deepwater Point. Perhaps a different label is appropriate since there is a mussel raft at Deepwater Point and the term "reference" is usually reserved for an unaffected area like a "control" in an experiment.

Some of the figures or tables may represent pooled data, it is not intuitively obvious from the legends or the accompanying text; e.g., for TVS. In other cases the text clearly states that statistical tests were done to partition the data, which is fine. If the data from the two North Totten Inlet locations was pooled, was it examined for differences first? As an example of the confusion, the Figure 2 legend refers to some undisclosed location in the same figure. Figure 3 repeats the same notation and leaves the reader wondering what it actually represents. No units are given in the x or y axis of Figure 3.

Salinity and temperature measurement and interpretation.

The study involved the use of a YSI 33 meter for measuring salinity as well as historical data from Ecology (Seabird CTD). In our experience, these meters are notoriously inaccurate even if calibrated before and after user because it is an analogue meter of relatively low cost and quality (not sold by YSI for several years now). They require frequent testing, probe recoating (platination) and other measures that render them very untrustworthy. I bought one of these new in the late 1980s but found it to be unreliable and changed to another instrument and had to discard the unit despite several manufacturers' service attempts.

The method of salinity standard preparation was not accurate for this study. Oceanographers define salinity in the Practical Salinity Scale (PSS) or PSU as the conductivity ratio of a sea water sample to a standard KCl solution. Ratios have no units, so it is not the case that a salinity of 30 exactly equals 30 grams of salt per liter of solution[10] as the methods indicate. Typically, standard solutions of seawater that have been tested with a laboratory salinometer (that has been calibrated with true seawater standards or a solution of KCl) are used to calibrate field salinity probes.

This problem is important if one is attempting to accurately detect small differences (e.g., 0.1 PSU or less). In the present report, the differences are large (compared to oceanic variation where salinity is used as a conservative tracer), so the accuracy issue is less important. The report goes on to compare salinity and temperature differences in time and space as a measure of water column stability.

A more expedient means is to combine salinity and temperature in sigma T density units and then partition out the contributory differences of each. It is not necessary for this report as the point is made that the inlet is episodically stratified and I do not think anyone disagrees with that point. However, if the report were to compare to other analysis in Puget Sound, conversion to sigma T density units would be necessary. The entire analysis would have been better conducted throughout the whole summer, the period of more likely vertical stratification and nutrient sensitivity, but the August data does show the sort of expected trend.

There was no indication that the temperature probe of the YSI unit was checked or calibrated at any point. While it may have been, or the unit may have been accurate, this model of YSI is not known for its good performance compared to typical CTDs manufactured by Seabird, YSI or Hydrolab in recent years.

Nutrient Limitation Misinterpretation

It is stated on page 14 that "*Phytoplankton production is light limited in the Pacific Northwest from November to the beginning of March*". This is true only for some terminal inlets and bays as the majority of the marine waters of Puget Sound main basin, North Puget Sound and the Strait are light limiting (to algal growth) year round. This is an important point that should not be glossed over. Also there are profound differences between the limiting levels for phytoplankton (in general) versus seaweeds, the latter being much higher.

The report goes on to say: "*It should be noted that at no time were the DIN concentrations < 1.0 μM where the nutrient becomes limiting*". This is without doubt incorrect, the range of ½ saturation constants for phytoplankton uptake or growth limitation (the ambient value at which uptake or growth of algae is restricted to one half the saturated nutrient level) span a very wide range from < 1 μM to > 7 μM with tremendous variation among species[11]. This should be

[10] UNESCO (1985). The International System of Units (SI) in Oceanography. *Tech. Pap. Mar. Sci.*, 45: 124 pp.

[11] Bowie, G.L., Mills, W.B., Porcella, D.B., Campbell, C.L., Pagenkopf, J.R., Rupp, G.L., Johnson, K.M., Chan, P.W.H., Gherini, S.A., and Chamberlin, C.E. 1985. Rates, constants, and kinetics formulations in surface water quality modeling. U.S. Environmental Protection Agency, Environmental Research Laboratory,

corrected if this is to be an accurate report regarding nutrient dynamics in the subject area. The waters of Totten Inlet are "nutrient sensitive" based on many authoritative studies and reviews, and the incorrect assessment provided here does not alter that fact. Certainly they would be less nutrient-sensitive nearer the north end, given the cooler temperatures, higher DIN levels in the mixed layer, and less vertical stratification compared to the central or southern end. While this is well known for this inlet and all the other inlets of SPS, it is important in the present context to demonstrate the more suitable siting of the mussel rafts that are proposed.

Redfield Ratio Misunderstanding

The Nitrogen to Phosphorus ratio cited by the author on page 16 relates to the average growth requirements of phytoplankton and should be stated using molar units, not by weight as the author has done. On a weight basis, the Redfield ratio is closer to 7:1, not 16:1 as stated. Therefore, this section of the report is incorrect and should be redone. Also, the report interprets the findings to indicate that there is "excess nitrogen" most of the time simply because the ratio is high. But N:P ratios are not properly used for determining if this could be, rather they are only used for determining which nutrient may be relatively more scarce in terms of algal bioenergetics needs. It is entirely possible to have a high N:P ratio and have both N and P be limiting to growth as phytoplankton populations are typically composed of a variety of species that have highly different rate constants for nutrient kinetics. If the author wishes to interpret the N:P data in this manner, each data point should be categorized if it is in a potentially N limiting range which varies by source but often has been selected to be < 7 μM N (0.1 mg/L N). If the field work had included a description of the dominant species involved, it may have been possible to select a more specific ½ saturation constant applicable to the entire population, but this was not done, so it is not possible.

The section also states for dissolved phosphorus (I assume this means dissolved inorganic phosphorus) "….the lowest mean value reported for North Totten Inlet (1.92 mg/L) is equivalent to 62 μM P". Examination of Figure 15 and associated text shows results of 0.02 to 0.09 mg/L, the former value equivalent to 62 μM P. Perhaps this referred to the N:P ratio instead. Regardless, the report is correct that P is likely never limiting to phytoplankton growth, but the analysis should be changed. I note that some of the figures are in molar units and other are in weight units. The report should pick one and be consistent.

Overall, if Figure 16 is correct, it appears that nitrogen is potentially limiting on all occasions but one (August 2, 2002), a radically different interpretation than what is given in the report.

Analysis and Interpretation of nutrient data

Approximately one half of the sampling conducted for this study in 2005 did not occur during the main algal growing season. Summary tables such as Table 12 lump growing and non-growing season data in the same presentation. This tends to bias the results toward different values than would have been obtained for the growing season, particularly for dissolved

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Office of Research and Development, Athens, Ga. EPA/600/3-85/040.. Still the most often cited and used information source by EPA and others. See also Thomann, R.V. and J.A. Mueller. 1987. Principles of surface water quality modeling and control. Harper Collins, New York, NY.

inorganic nutrient results. I recommend that these data be analyzed and presented separately to address the time period of principal concern, the growing season.

Drifter study 2005

I do not understand why the drifter study was conducted from the North Totten Inlet water sampling station that was neither at the mouth of the inlet nor at the proposed NTI site as recommended in our suggested protocols. The purpose for placing the drifters where and when (2 hours prior to slack high tide) they were initially placed escapes us. I advise that the author should provide that information to help the EIS writer understand the purpose of this work. And although small scale tidal diagrams are provided to show the period of the study, no mention is made of how the total range of the selected tides correspond to the mean or diurnal range of tides in the area (although some mention that part of one tide was greater than average).

The rest of this work provides some interesting but not really essential aspects of circulation, but due to the limited number of drogues, the selected release point and times that include partial tidal cycle coverage, I cannot see the utility of this work. Finally, this section states… "*These few drifter studies support the hypothesis that the flushing characteristics of Inner and North Totten Inlet's are different*". I fail to see how this was showed, although certainly agree that there is little doubt that flushing rates are inversely related to distance further into the inlet. That is a fundamental of inlet circulation, not a hypothesis.

Phytoplankton production spreadsheet

On page 38 it is stated **…"***A spreadsheet model was developed based on these algorithms to predict the increase in phytoplankton biomass as a proportion of the standing biomass*". This is unclear. Is the author referring to an increase of biomass due to the excretion of nitrogen from the mussels of the proposed farm, the existing farm or what?

Later in the same section it is stated …." *the negative May value* (of phytoplankton biomass turnover) *is important because Totten Inlet is a highly productive shellfish growing area and these results suggest that bivalves were cropping excess phytoplankton and helping (together with reduced DIN concentrations) to prevent what might have developed into a nuisance bloom (>20 μg Chla/L (WDOE 2002)). The increased ammonium concentrations observed following the spring bloom in 2002 (Figure 13) suggests that much of the organic carbon removed from the phytoplankton community was sedimented with subsequent nutrient regeneration"*.

In lieu of any macrozooplankton data, I find these comments unduly speculative that bivalves were THE cause of phytoplankton cropping. The section should note that possibility. It is also speculative that increased ammonium was a result of organic carbon cropping and sedimentation followed by release into the water column as bloom senescence could have caused the same effects.

Estimated carrying capacity

This is a very useful exercise, but I would prefer to see more detail and explanation. This section refers to an Appendix 1 that was not included, and to a prior unpublished report by Brooks. This report should stand alone and present how key values were selected for using the model;

e.g., what was the zooplankton grazing estimate and how was it developed? It would also be useful to explain how the value of existing cultured bivalves was estimated at 253 tons, rather than citing an unpublished reference.

Summary

The summary should be altered to deal with the issues raised above.

Other Comments

Introduction: background literature section: good points but suggest citing Cloern et al. for west coast perspective

Introduction final section discussing experimental design: Indicates that data from 2005 to be used but subsequent discussions use other data, too.

page 10. The report appears to compare canister (trap) TVS in British Columbia versus water column samples in Totten Inlet.

Citations of unpublished reports (Brooks 2000 and Gardiner et al. 2004 are outdated now and should be updated to cite the 2006−2007 reports).

The abstract says that phytoplankton measurements were higher on the ebb than the flood, but not statistically significant. If it was not statistically significant, then it wasn't "higher" at all, just nominally different. For the sake of public reviewers, I suggest that this be amended. I also note that the sample size was small. If additional measurements would have been conducted and the results were within the recorded range, would the differences have been statistically significant?

Report No. 4. Totten Inlet baseline studies completed by Aquatic Environmental Sciences in 2002 and 2003 by Kenneth Brooks

This report summarizes studies by Dr. Brooks. I was not responsible for review of all the topics involved, but a quick reading of the non-water column sections indicate to me that these sections are generally well written and should be very useful for the EIS writer and the EIS process. In terms of extent of the work, they do indeed far exceed that done for any aquaculture facility anywhere in the world. I note that the title of the report does not seem to be consistent with the years of work actually reported (as subsequent years included field work or writing).

The water column sections of this summary need to be altered in accordance with my previous comments; in particular, the case that there is a difference in ebb vs. flood tide standing stock of phytoplankton is very weak. This version includes reference to a single set of measurements on one day in 2002 that tested only marginally different, and then not substantiated by further measurements in 2005. Yet the summary reaches the conclusion that Totten Inlet is a net producer of phytoplankton compared to the rest of SPS. In my opinion, again, the sampling was entirely too limited in space and time and biased because it was only in the extreme north end

of the inlet and received some unknown level of reflux water. Neither of the assumptions, that Totten Inlet is "a significant consumer of nutrients," or as a net consumer of nitrogen (as stated in the underlying report) is substantiated as previously explained. I recommend that this entire line of reasoning be dropped as the experimental design was incomplete and these misstatements detract from the more important point that the inlet is rich in phytoplankton and seston production, and that carrying capacity is not being approached. If anything, I believe the data shows that north Totten Inlet is not unlike Squaxin Passage and therefore not unlike other deep SPS passages. The insistence of stating non-significant differences as important is very distracting.

As pointed out by Dr. Kawase, the report of a maximum current velocity of 84.3 cm/s seems unlikely given the mean current velocity. He also makes some other comments regarding the physical oceanographic sections of this report that I do not reiterate here.

As a general comment relating to all the consultant reports, there is a need to summarize all the studies still, particularly with regard to major disciplines such as physical oceanography, water column ecology and benthic effects. Otherwise it is left to the EIS writer to attempt to distill down the important points. Dr. Brooks states that the large number of individual reports were necessary because they were formatted that way for publication. That may work for the convenience of the author, but our priority is to see that the EIS writer is provided with the best, most clearly written summaries and presently I feel that the information is scattered about several reports in a sometimes confusing fashion.

Appendix 1. Previous Correspondence sent by ITRs to Taylor Resources

North Totten Inlet Mussel Culture Proposal

Response of the
Independent Technical Review Committee (ITRC)
to Thurston County and Taylor Resources

Regarding Letters of Comment Received in March 2002
During the Process to "Refresh" the Scope of EIS

August 20, 2002

On February 28, 2002, Thurston County issued a second Scoping notice for the Taylor Resources North Totten Inlet Mussel Culture EIS. Agencies, Tribes, and members of the public were invited to participate in "refreshing" the Scoping process for this EIS by commenting on the revised proposal and alternatives to be addressed in the EIS, and on the proposed scope of work for elements of the aquatic environment to be studied. Thurston County invited additional comments on the EIS scope of work at this time due to the:

- Amount of time that has passed since the Scoping process was originally conducted (September 14–October 5, 1998)
- Change in the description of the proposal (i.e., elimination of the proposal to expand mussel rafts at the Gallagher Cove site)
- Identification of an alternative to the North Totten Inlet proposal to be analyzed in the EIS
- Availability of additional detailed information describing the proposed scope of work and protocols for field studies to be performed to evaluate potential impacts to elements of the aquatic environment.

Thurston County received 28 letters of comment in response to this second EIS Scoping process conducted in March 2002. All of these letters were received from individuals, none from Tribes or regulatory agencies. The majority of the letters expressed opposition to the project, but did not address the EIS scope of work or the scope of the aquatic environment technical studies. Eight letters raised questions or made suggestions regarding the Aquatic Environmental Sciences (AES) or Pacific Shellfish Institute (PSI) proposed methods or protocols. A few other letters raised some of the same issues identified in these eight letters that were selected as representative of the comments on the technical scope:

A.P.H.E.T.I. (March 31, 2002)	Walker (March 25, 2002)
Acheson (March 22, 2002)	Wimberger (April 1, 2002)
Gentle (March 25, 2002)	Woodnutt (March 14, 2002)
Stein (March 27, 2002) + Attachment XIII	Ziemke (March 27, 2002)

Thurston County requested the opinion of the Independent Technical Review Committee: Should any changes be made to the work proposed for aquatic environment technical studies – beyond those identified in prior ITRC recommendations dated November 1, 2001 and February 13, 2002 – as a result of the Scoping comments received? This memo constitutes the consolidated response of the four ITRC members.

Members of the Independent Technical Review Committee and their respective areas of expertise are as follows:

Independent Technical Reviewer	*Area of Expertise*
J.E. Jack Rensel, Ph.D. Rensel Associates Aquatic Science Consultants	Phytoplankton, algal blooms, effects on benthic organisms, benthic conditions, beaches and finfish.
Mitsuhiro Kawase, Ph.D. University of Washington School of Oceanography	Physical oceanography: Flushing characteristics (circulation) and water quality (eutrophication).
Jan Newton, Ph.D. University of Washington, School of Oceanography	Biological oceanography: Water quality (nutrients, oxygen) and phytoplankton productivity.
Ralph Elston, Ph.D. AquaTechnics, Inc.	Mussel genetics: methods for species identification, and potential escapement and competition issues.

Re: Harmful algal blooms, effects on benthic organisms and conditions, effects on beaches and finfish.

1. The EIS should address potential impacts on nearby (and remote) beaches, including the fate of psuedofeces in terms of its density, sinking rate, transport and assimilation into the sediments and food web. If the Taylor Resources' consultant uses existing literature on this subject, coupled with known transport vector and site-specific circulation information, no special studies are needed. There is no evidence to suggest a significant impact of rotting mussels accumulating on the beaches that originate from mussel rafts.

2. If there has been any credible study or professional documentation of conditions in Penn Cove before and after mussel rafting was established, the EIS should examine it.

3. The EIS will undoubtedly discuss the nutrient and eutrophication status of Totten Inlet and its sensitivity to nutrient inputs or removal (or change from dissolved to solid form).

4. The water quality section of the EIS will undoubtedly deal with the fact that ammonium is produced by mussels, as well as its probable and rapid transformation to nitrate.

5. If dredging the bottom is proposed or commonly practiced beneath the mussel rafts, this should be discussed in the EIS. (*Note: Personal communication between Vicki Morris, ITRC Coordinator, and Diane Cooper, Taylor Resources 5/23/02: no dredging is proposed.*)

6. Taylor Resources' consultant is capable of analyzing and interpreting the acquired data, and utilizing best professional judgment for presenting this information in the EIS in the manner requested in the APHETI letter of comment. There have been a number of changes to the protocols based on the ITRC recommendations. The ITRC also expects that the technical report will deal with the issues outlined by the ITRC as possible problems.

7. Potential impacts to resident demersal and pelagic fishes should be addressed in the EIS in addition to potential impacts to Puget Sound chinook salmon. (*Note: The AES Scope of Services document dated August 29, 2001 at page 2 acknowledges that the results of the AES*

and PSI studies will also be used by a fisheries biologist – a member of the EIS team – to address potential impacts to salmonids. A fisheries biologist has been retained to review the scopes of work and protocols for the aquatic environment technical studies being performed by AES and PSI, and the recommendations of the ITRC that modify the original proposals for these studies. A subsequent memo will be prepared by the fisheries biologist, addressed to Thurston County, to comment on whether any additional information may be needed from the aquatic environment technical studies for use in preparing the EIS analysis of potential impacts to demersal and pelagic fishes, and potential affects on threatened and endangered species.)

8. The EIS should review available information on threatened or endangered species in the area that may be affected by the proposed mussel culture operation. It will need to address potential impacts to the prey organisms of Puget Sound chinook salmon, but no special studies are needed given the wealth of information about food habits, distribution and habitat preferences of salmon in southern Puget Sound. There are no naturally-occurring or hatchery populations of chinook salmon in the Totten Inlet drainage. However, colleagues of one ITRC member at NMFS have advised that because of the *possibility* of bona fide listed chinook salmon (e.g., White River spring or Northern Puget Sound stocks) rearing or residing as blackmouth (resident chinook salmon) in South Puget Sound marine areas, the NMFS Habitat Conservation Division is requiring sponsors of activities that may affect fish in those areas to consult with NMFS concerning potential ESA impacts. NMFS is also applying listed chinook species protection review/consultation requirements in some South Sound freshwater habitats (including Deschutes River stock) where chinook salmon have been observed spawning recently or in the past.

9. Analysis of the effects of mussel rafts on local currents, effects of waste accumulation on the benthic environment, and physical effects of raft operations on the benthic environment is proposed in the scope of work for aquatic environment technical studies.

Re: Water column impacts – flushing characteristics and eutrophication.

Clarification is offered here concerning the purpose of the water column impact study to address flushing and circulation in Totten Inlet. The first objective of the study is to establish the overall flushing time of Totten Inlet as a whole. The second objective is to establish the pattern of circulation within the Inlet itself, pathways of water that arrives at and departs from the proposed mussel raft site, and flushing rates of different parts of the Inlet.

The upper and lower bounds of the overall Inlet flushing time should be established by determining a) the net through-flow only, and b) for a complete tidal prism replacement on each tidal cycle, respectively. The upper bound is dependent on the amount of freshwater input to the Inlet. An exchange circulation will enhance flushing and would reduce the flushing time. A previous estimate of 10 days based on the net transport of 240 cubic meters per second (URS 1986) was reported in the *Visual Impact and Ecological Concerns Assessment for the Totten Inlet Mussel Rafts Project* (EDAW, Inc., January 1998). However, the net transport is likely to be highly variable, since the Inlet appears to lack a steady, significant source of freshwater. Especially during Summer months, when the freshwater input is at a minimum and the net through-flow and exchange circulation is weakest, Inlet flushing by these mechanisms could be highly inefficient. The correlation between seasonal flushing variability and the period of peak mussel growth needs to be examined.

Using the tidal volume estimates of McLellan (1954) – also cited in EDAW, Inc. (January 1998), and a simple tidal prism model, one can estimate a 10% dilution (90% replacement) time of 1.37 days for the lower bound of overall flushing time. It is highly unlikely that the Inlet would be flushed over such a short time frame due to refluxing (while unknown in percentage). During Summer months, tidal exchange could become the dominant mechanism for flushing. Available studies have not addressed this issue.

Tidal exchange and circulation will redistribute waterborne materials within the Inlet. The pattern of this redistribution could be complex, with some regions of the Inlet experiencing poor flushing relative to others. While the proposed mussel raft site is likely well-flushed by tidal currents, the materials that leave the site could end up in regions of poor flushing depending on the circulation pattern. Thus, circulation mapping within the Inlet is important to determine the potential impacts of the proposed raft site on the remainder of the Inlet.

10. The ITRC reiterates a previous recommendation that a reliable assessment of the flushing time of Totten Inlet be made principally through combined use of the existing data set and available numerical models. Additional measurements must be taken where data coverage is deficient. The analysis should endeavor to establish the upper and lower limits of residence time.

11. A reasonable point was made in one letter received during the March 2002 Scoping process that Totten Inlet does not discharge into an open body of water, but rather into a network of channels and other inlets for which ventilation characteristics may be quite complex. The water column impacts assessment should include estimates of the dispersion and transit time of water originating in Totten Inlet and in the neighboring bodies of water. The important neighboring bodies of water are the waterways around Squaxin Island, Dana Passage, and Pickering Passage.

To help clarify an appropriate level of effort for the measurements recommended in the comments above, a suggested experimental setup is provided as an attachment to this memorandum.

Re: Water quality, phytoplankton, and nutrients.

12. Previous recommendations of the ITRC (November 1, 2001 and February 13, 2002) identified the need for a study plan to assess potential project-specific impacts on water quality, phytoplankton, and nutrients from the proposed mussel rafts. The PSI study, as some of the Scoping letters recap, approaches the water column work from a very different perspective. Taylor Resources has not yet provided the document requested to "bridge the gap" between the PSI work and the project-specific impact analysis. Therefore, the ITRC has not yet received a response to this recommendation to facilitate review of these elements of the proposed scope of work. Public comments received will be taken into consideration when the ITRC recommendations are formulated regarding the water column studies. Taylor Resources is encouraged to also consider these comments as the study plan is being prepared.

13. The study of water quality, phytoplankton and nutrients should be well-coordinated with (i.e., conducted to complement and take advantage of results from) the studies of flushing and circulation in order to evaluate dynamics of the system. The water quality, phytoplankton, nutrient study should be conducted to assess conditions throughout an annual cycle, due to

seasonal variation in primary productivity. The study may be less intensive in Winter, but Spring, Summer and Fall conditions should be characterized.

14. Cumulative environmental effects should be addressed in the water column study plan requested.

15. Use of any computer models needs to be outlined in the study plan requested above, including model definition and intended application for the EIS impact analysis.

Re: Mussel species identification, escapement and propagation.

16. The genetic marker methodology to be used for mussel species identification is well-covered in the ITRC November 1, 2001 Recommendations, based on input solicited from a leading mussel genetics expert. Comments submitted by Peter Wimberger (Associate Professor of Biology, University of Puget Sound) can be conveyed to the independent geneticist who will run the assays if the County so desires.

17. Sampling aimed at detecting 10% increments in relative populations (as proposed in the AES scope of work) is adequate. The ITRC does not consider it necessary for any additional samples be collected, nor for additional sampling sites (beyond the 3 proposed), larger sampling sites, or longer sampling periods to be established.

Re: Gallagher Cove as a Study Site Instead of or In Addition To Deepwater Point

Strong sentiment was expressed among members of the Association for Protection or Hammersly, Eld & Totten Inlets (APHETI) that the Taylor Resources mussel farm location in Gallagher Cove be an aquatic environment study site instead of or in addition to the Deepwater Point mussel farm site. In a memorandum dated July 1, 2002, Taylor Resources described their rationale for selecting Deepwater Point as the "treatment site" for study of the effects of mussel faming in Totten Inlet. Reasons given describe more similarity in the physical characteristics of the Deepwater Point and North Totten Inlet sites, and in the size of the existing and proposed mussel farms.

The ITRC is in general agreement that the Deepwater Point site is suitable for the aquatic environment technical studies, and that it is not necessary to completely duplicate sampling at the Gallagher Cove site. The Committee does, however, recommend that the cumulative impact analysis address whether or not nutrient fluxes are likely to occur in Gallagher Cove as a result of the proposed mussel farm installation at the North Totten Inlet site. The flushing and circulation study should include drogue deployment to determine the relative transport of nutrients between the North Totten Inlet site and Gallagher Cove. The Committee also requests a description of sediment grain size in Gallagher Cove in comparison to the Deepwater Point site and the North Totten Inlet site. A relatively simple task, such characterization will tend to indicate if bottom water currents are similar and will help describe the relative depositional or erosional nature of the sea bottom at both locations.

Suggested Experimental Setup
to Establish the Residence/Ventilation Time of Water in Totten Inlet

A focused study of Totten Inlet flushing and circulation during a few key months would address the scope of work questions identified for the North Totten Inlet Mussel Culture Proposal EIS. The purpose of the study should be to quantify the range of flushing time within the Inlet. In particular, the study should examine the upper bound ("worst-case") situation, when flushing is at a minimum and flushing time is longest. This is likely to occur in the Summer, when surface water runoff to the Inlet is at a minimum and prevailing northerly winds tend to oppose generation of the density-driven exchange circulation. Another study period would be during the maximum anticipated impact from the mussel culture operation. (It is possible that this would be the same as the Summer study period previously described. Taylor Resources should clarify the period of maximum mussel growth for use in designing the water column study.) Lastly, an "average" period of flushing and residence time should be examined. Suggested methods for conducting this study (which may be refined by the consultant retained to perform the work) include:

1. Place an array of 4 to 5 acoustic Doppler current profilers (ADCP) or similar instruments across the mouth of Totten Inlet to measure the influx/outflux and structure of tidal currents and exchange circulation at the mouth. The measurement should have sufficient horizontal (4–5) resolution and duration (over two fortnightly tidal cycles) to resolve horizontal and vertical structure of the tidal and time-mean (exchange circulation).

2. Monitor relative tidal elevation during one of the reflux surveys and relate this information to profiler information. Tidal level measurements will help refine the estimate of the tidal prism, and will also augment the ADCP measurements. Alternative methods for tidal elevation monitoring include: a) Deploy internally-recording pressure sensors on pilings, and calibrate with stadia rods at the beginning and end of drogue deployments. b) Attach a stadia rod to a piling and record the tidal elevations periodically during the survey. If possible, relate the stadia rod elevations to benchmarks or other objects of known elevation nearby. A further refinement would be to use a rotating laser beacon on shore to record the elevations in the survey boat using a stadia rod with laser detector. The beacon could be set up near a benchmark or other exact known elevation.

3. Deploy 10 or more drogues at a near-surface level in Totten Inlet to ascertain the pattern of tidal currents within the Inlet, as well as to measure their escape from the inlet. Ten drogues would be sufficient if deployment occurs over multiple tidal periods. If short-duration deployments (like one tidal cycle) are performed, it is recommended that 20 drogues be deployed. Include Gallagher Cove as one study location. Two depths are recommended: one relatively shallow (e.g., 1 m) and another deeper, approximately half way to the average bottom depth in the area being surveyed.

4. To accomplish an empirical measurement of tidal excursion, deploy drogues (mini-windowshade style recommended) in upper Totten Inlet from high water through low water and back to high water on a true mean tidal exchange day. This usually happens once or twice per year, typically in September or October.

5. Deploy 10 or more drogues in Dana Passage, Pickering Passage, and the waterways around Squaxin Island to measure the efficacy of the "conveyer belt" circulation around Hartstene Island to flush out outflow from Totten Inlet. Make serial releases from the mouth of the inlet at exact intervals throughout average tides (or minimal and extreme range tides, too, if

desired). Estimate reflux by counting the number of drogues (if any) that return, and calculating the concurrent water exchange during the corresponding tidal cycle phase (non-linear volumes of water passing out and back in). It is recommended that this task be performed during the summer to record worst-case conditions (i.e., the nutrient-sensitive period with no south wind to help move surface water to the north as would occur in the winter and Summer/early Fall).

Note: A drogue and current meter study performed in Pickering and Squaxin Passage for the Squaxin Tribe may be available upon request to augment the measurements recommended above.

6. Spot-monitor temperature and salinity (T&S) at different locations in Totten Inlet. Three or four continuous monitoring stations at different locations would be ideal. Relatively inexpensive instruments that continuously record temperature, salinity, and pressure (such as Hydrolab DataSondes) are available that could be used to record the data requested in both Tasks 1 and 5 of the Totten Inlet residence/ventilation time experiment. The T&S devices can be bolted to existing pilings just below the mean lower low water (MLLW) level. The instruments should be calibrated by direct measurement at the time of deployment and retrieval.

   In order to monitor the evolution of stratification, direct measurements of temperature and salinity at the surface, mid-depth and bottom should be made whenever there is operation out in the Inlet. In addition, it is strongly recommended that a continuous monitoring of temperature and salinity be made with a device mounted on the cage of the central ADCP deployed at the mouth of the Inlet, as recommended in Item 1 above. A high-precision instrument is strongly recommended for this deployment.

7. Compare data with numerical model of circulation.

The residence/ventilation time experiment should cover two full spring-neap tidal cycles (approximately one month). It should take place during the relevant season (i.e, during the period of maximum mussel growth or mussel fecal matter discharge).