Scientific review of the technical reports, with final comments, submitted in support of the application by Taylor Resources for placement of mussel rafts in North Totten Inlet.

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Totten Inlet Circulation Study by Evans Hamilton Group.

1: Technical Observations and Applicability to Environmental Review of the Proposed Action (Your observations, concurrence and/or disagreement, based on the information provided)

The study clearly indicates that there are strong water currents in the region. This confirms earlier observations at the same site by Brooks (see page 60 to 64 in his report "Baseline Information Describing Sediment Physiochemistry of Totten Inlet and the macrobenthos of the proposed North Totten Inlet mussel farm"). This information on water currents is fully discussed in the NewField report. These currents are beneficial for mussel aquaculture rafts as water flow will serve to distribute the mussel biodeposits over a wider bottom area. These currents will also mix the water to ameliorate the effects of seston depletion by the mussel rafts.

Evans Hamilton report low dissolved oxygen levels of less than 5 mg/L in the bottom water for the Totten Inlet mussel raft site. They speculate that these readings may be partially due to the housing of their oxygen meter reducing water flow over the sensor. I wonder why they did not modify their design for their subsequent deployments? In the NewFields Northwest report (last paragraph P 16), they report that Brooks (2003) also found low oxygen concentrations in bottom water at the Deepwater Point mussel raft site. In the NewFields report summary they gloss over these low bottom DO values by saying in their summary (last paragraph page V) that DO was 5.9 to 13 mg/L at the maximum depths sampled. That is not quite correct and needs to be clarified because at the maximum depth sampled- which was 5 cm above the sediment- DO was often less than 5 mg/L.

After reviewing all available data and discussing this with Jack Rensel and Carol A. Coomes at Evans-Hamilton, it seems that the bottom waters at NTI are well oxygenated and that an inadequate oxygen sensor was the reason for the measured low values. This point is discussed in more detail in review comments submitted November 2008 by Dr. Jack Rensel.

2: Technical Errors, Deficiencies, and/or Issues of Concern

I am not trained in Physical Oceanography and so I am not able to comment on the methods used in their study. I did find that the discussion and interpretation of the data to be less thorough than I would have expected. This deficiency is largely offset by the NewFields Northwest report that discusses and uses these circulation data.

One point that concerns me is that their data on tidal flow was collected solely on 9/26/2005; additional sampling of water quality parameters took place for a month in October/November. The September circulation data is before the onset of the main rainy season which is October through March according to the NewField report (page 5). I wonder how changes in salinity associated with increased river flow may alter any water flow patterns or water quality. Often when estuaries become stratified oxygenation of bottom water is reduced.

The subject of bottom water oxygen concentration and possible development of anoxic sediments is a crucial one that needs to be thoroughly considered (see section 5 below for my suggestion for addressing this concern). I address this point in greater detail in my review of some of Dr. Brooks report "Baseline Information describing Sediment Physiochemistry of Totten Inlet and the macrobenthos of the proposed North Totten Inlet mussel farm"

3: Reference Documents Not Provided, and/or Not Peer-Reviewed: None

4: Editorial Observations and Comments: Covered in sections above.

5: Additional information/analyses required

They use the Washington Department of Ecology data for September (see p 4) as a comparison for their October/November water quality data. It would be useful if they synthesized and presented in their report the Dept of Environmental Quality water quality data for the Windy Point site from other months and years to provide a more complete picture of salinity and dissolved oxygen in Totten Inlet. As I discuss below these data have already been obtained by Dr. Brooks so they only need to include that information in their report to make it complete. By including this information that shows stratification has rarely been observed it will allay concerns about this matter that others may have when reading this report.

In Dr. Brooks report "Executive Summary....Totten Inlet baseline studies completed by Aquatic Environmental Sciences in 2002 and 2003" he reviews Washington Dept. of Ecology data (page 6 second paragraph) and concludes from this that there is no water column stratification. But in contradiction to this in Dr. Brook's report "Supplemental Study of Dissolved nutrient and particulate organic matter in the waters near the proposed mussel farm in North Totten Inlet" figure 2 and 3 he does present monthly temperature and salinity depth profiles that show

evidence of some small temperature and salinity stratification in August and November. These differences in density may be sufficient to reduce mixing of oxygenated surface water to the bottom. Dr. Brooks (See page 41 last paragraph in Literature Review Second edition prepared Feb. 16, 2006) does state that Washington Dept of Ecology classifies Totten Inlet as an "Episodically Stratified Inlet". Later in that same review (page 43 bottom of page) he states that "Totten Inlets low freshwater input and thorough mixing reduce, but do not eliminate the potential for stratification." I suggest that Evans Hamilton Group (or the NewFields Northwest) simply review the same long-term Washington Department of Ecology monthly data used by Dr. Brooks. This will allow them to include in their report some statements for a longer time span than their own measurements were made about whether stratification of the water column is likely to be a potential impediment to reoxygenation of bottom water. No additional analysis has been added to address my point about water column stratification. But after further discussion with Jack Rensel I believe that the low freshwater inflows into Totten Inlet mean that differences in density between surface and bottom water are unlikely to be very large. This means that the water column will not remain stratified for long periods, thus allowing oxygen to be mixed down to the bottom. This mixing of oxygenated surface water to the bottom is the mechanism where oxygen is supplied to the sediments and prevents benthic respiration from depleting all of the oxygen in the interstitial waters hence causing the sediments to become anoxic.

An assessment of potential water column impacts of mussel raft culture in Totten Inlet Report by NewFields Northwest.

1: Technical Observations and Applicability to Environmental Review of the Proposed Action (Your observations, concurrence and/or disagreement, based on the information provided)

Overall, I think that they have done an effective job of assessing the possible effects of mussel aquaculture rafts at Totten Inlet on water column processes. I think that it is reasonably clear, based on the calculations and models presented in this report, that the additional mussel aquaculture rafts in Totten Inlet will have negligible influence on water column processes. It was interesting that their models predicated that <1% of the estimated carbon production throughout the year in Totten Inlet would be consumed by these extra mussels. Given the area of Totten Inlet and the relative small size of the aquaculture rafts I think that these estimates are realistic. These predictions are also in line with estimates made by Brooks using a different model and a different set of starting assumptions (see his report Supplemental Study of Dissolved nutrient and particulate organic matter in the waters near the proposed mussel farm in North Totten Inlet.)

They estimate that the removal of nitrogen contained in the harvested flesh of mussels and associated biofouling organisms from the additional rafts at NTI would remove between 17 to 40% of anthropogenic N inputs to Totten inlet (see second paragraph on page VI and the main calculations on page 32 and at top page 41.) Their use of the phase "17 to 40% of anthropogenic nitrogen introduced to Totten Inlet"" is misleading as I think that they only mean "N from Septic system." Total anthropogenic inputs are likely to be larger than this due to riverine inputs from the watershed and airshed inputs. Unfortunately, I was not able to check their calculations because they do not provide a correct reference to the work of Alberston et al (2002) who estimated watershed nutrient inputs to NTI. But their estimate seems quite a high percentage removal and suggests that with the existing two aquaculture sites that mussel aquaculture is capable of removing more than 100% of anthropogenic nutrient inputs. This does seem to be a prime example of extractive aquaculture being able to contribute substantially to improving water quality. They seem to have responded to these comments in their revised report. I have marked on their final report a couple of places where they need to do a little editing and rewriting to make it clear that they are referring ONLY to 17 to 40% of sewage related inputs from houses in the Totten inlet watershed. If this distinction is not made, people may think that they are including all the anthropogenic nitrogen that is imported into the inlet with tidal prism water entering from Puget Sound. That N importation is likely a huge number given the overall nutrient enrichment in South Puget Sound.

Jack Rensel and I suggest that they also add a final paragraph in which they use the same type of calculation to contrast how much less phytoplankton may be produced using their estimates of N removal via harvest of mussels and fouling animals. This can then be contrasted to their existing estimates of how much more phytoplankton may be supported by N recycled through mussel N excretion. This will illustrate the point that N removal via harvest is an important means of reducing phytoplankton biomass.

They should also make the point here that even though mussels may appear to enhance N concentration around the raft that must mean less N is being remineralized elsewhere in the ecosystem; i.e, the overall amount of N being regenerated is not increased in NTI by mussel aquaculture but only the location where that regeneration is located in NTI. This point has been somewhat confused in the European literature (Asmus, Smaal etc). I review the literature on this topic on page 10 in the following paper. This can be downloaded from the web site<http://hpl.umces.edu/faculty/newellcv.html>
Newell, R.I.E., T.R. Fisher, R.R. Holyoke, and J.C. Cornwell. 2005. Influence of eastern oysters on Nitrogen and Phosphorus regeneration in Chesapeake Bay, USA. Pages 93-120 In: The comparative Roles of Suspension Feeders in Ecosystems. R Dame and S. Olenin (Eds.) Vol 47 NATO Science Series: IV - Earth and Environmental Sciences. Springer, Netherlands. Download from <HYPERLINK "<http://hpl.umces.edu/faculty/newellcv.html>" http://hpl.umces.edu/faculty/newellcv.html>

2: Technical Errors, Deficiencies, and/or Issues of Concern

Some of the major conclusions of this report are based on the predictions from a model of nitrogen flow through mussel growing on the proposed raft at NTI (see p 37). They based their model on one developed by Rodhouse and coworkers for mussel culture in Ireland. As far as I can ascertain, they simply used the model predictions from Rodhouse et al. and adjusted each predicted output downward because the predicted mussel production at NTI will be only 40% of that reported by Rodhouse in Ireland. On page 75 line 3 they state that phytoplankton consumption was based on the Rodhouse model but again as far as I can see this was just a straight transfer from the Rodhouse model and did not really take in account seasonal changes in Totten Inlet phytoplankton stocks. If I am correct in my interpretation of what they did then this is an odd way of adapting the Rodhouse model to NTI. I would have thought they should have started with the monthly data for NTI of phytoplankton abundance (Chlorophyll *a*) and detrital carbon (estimated by subtracting Phytoplankton Carbon from total carbon). These data could then presumably have been used in the Rodhouse model to actually estimate the amount of material partitioned into the various components, including feces, excreta etc. This approach would also have avoided them having to roughly estimate how much of the filtered seston was rejected in pseudofeces (see section 6.3.1) as the mass could be predicted from the model. Currently, they simply assume that the pseudofeces production will be the same as fecal production during summer months. I do not think that their final conclusions will be altered that much if they had parameterized the mussel model with the real data from NTI. Where it becomes of concern is if others try and adapt this model to predict the effects of additional mussel farms in NTI. Then it would be more realistic to have a simulation model properly parameterized on actual NTI seston data to understand these cumulative effects of suspension feeding mussels on seston dynamics. In their revised report I inserted the following comment about how they should add a paragraph to clarify how they adapted the Rodhouse model.:

My understanding of how they applied the Rodhouse model was that they used it to first estimate the percent allocation to the various N components of growing mussels. These percentages were then used to back-estimate from the projected mussel harvest value what amounts of N would be partitioned into the various components (excretion, feces etc). If that is not how the model was

applied then they should clarify their explanation in their report. If my interpretation is correct they should note in their report that this is only approximate as this approach is not based on using the actual phytoplankton/Total suspended solids/Particulate organic carbon etc concentrations in North Totten Inlet. Thus, in reality the particulate N (feces and pseudofeces) deposited to the sediment surface and dissolved nitrogen excreted in urine at NTI will be different from their estimates of the magnitude of these N losses.

I have some concerns about the Rodhouse model diagram included as Figure 31 page 39. What do they mean in parenthetical statement that this is based on data collected? Surely this figure is just copied out of Rodhouse et al and the values annotated on the C and N flows are for his mussel site in Ireland? Need to make this legend much clearer to avoid the reader trying to think that these numbers apply to the NTI site. This legend also incorrectly states that this simulation is for 14 months but the grow-out period for Ireland mentioned by Rodhouse was 18 months (table 3). It is 14 - 18 months for NTI and they use an average 16 month in Table 3.

I am confused by the definition of "Assimilation Efficiency" on page 73 which they define as the proportion of consumed carbon that is assimilated into tissue or used in metabolic activities. They then state that they use a value for this of 15% for mussels and 10% for all other bivalves. There must be something wrong with their definition because that would mean that 85% of the carbon digested and absorbed is then being excreted as DOC. This is not true. So I think that they must mean that they are using a "Growth Efficiency" which is the amount of carbon available for tissue or germinal production.
They did not alter this part of the text. After reading this again, I think it is a terminological error that can be altered quite simply. On page 68 of the September 2007 report (i.e I have used page numbering from original report in order not to confuse page number which are now different in the Sept 2008) they state in the first line under Fig 49 that 13.7% of the carbon consumed was used in production. I think that this is this what they are subsequently incorrectly referring to on p 73 as "Efficiency or assimilation efficiency is the proportion of consumed ration that is assimilated in tissue or used in metabolic activities" Based on the following definitions they should in section 6.3.5 page 73 redefine "efficiency" to be Gross Growth Efficiency. In this definition the carbon that is used for metabolism is not included. I have edited the text in the report PDF file

Food Utilization equations from Crisp, D.J. 1984 Energy flow measurement. pp 284-372 In: N. A Holme and A.D McIntyre (eds) Methods for the Study of Marine Benthos. IBP handbook 16 Blackwell Scientific.

$$\text{Absorption Efficiency} = \frac{\text{Consumption-Feces}}{\text{Consumption}} \times 100$$

$$\text{Assimilation Efficiency} = \frac{\text{Consumption-Feces-Urine}}{\text{Consumption}} \times 100$$

Scope for Growth = Consumption-(Respiration+Feces+Urine)

Gross Growth Efficiency K_1 =Scope for Growth X 100
Consumption

Net Growth Efficiency K_2 =Scope for Growth X 100
Scope for Growth + Respiration

They state that the anemone *Metridium senile* is a non-selective filter feeder that consumes phytoplankton and POM in the seston. (See page 78 penultimate paragraph, bottom page 70). They then estimate how much phytoplankton carbon is being utilized by these anemones. To the best of my knowledge this sea anemone is a carnivore, primarily feeding on small zooplankton. For example, examination of feces of *Metridium senile* in Monterey Bay, California (Purcell, 1977) revealed a diet of copepods, polychaete larvae, bivalve and gastropod veligers, copepod nauplii, and barnacle nauplii and cyprids. Sebens (1985) reported that barnacle cyprids, ascidian larvae and gammarid amphipods were the preferred food of *Metridium senile* over invertebrate eggs, foramaniferans, copepods, and ostracods.

Purcell, J.E. 1977. Diet of large and small individuals of the sea anemone Metridium senile (Coelenterata, Actiniaria). Bull. S. Cal. Acad. Sci. 76:168 172.

Sebens, K.P., 1985. Community ecology of vertical rock walls in the Gulf of Maine: small-scale processes and alternative community states. In The Ecology of Rocky Coasts: essays presented to J.R. Lewis, D.Sc., (ed. P.G. Moore & R. Seed), pp. 346-371. London: Hodder & Stoughton Ltd.

They have corrected this in most instances but I highlight a place where this still needs to be corrected in my marked –up copy of the PDF file of their report.

On page 23 last sentence of third paragraph they have “and pseudofeces” as part of a discussion of gamete production. I am not sure what they meant to write but this is wrong. Also, in the first sentence of last paragraph on page 23 they state that mussel particulate wastes include gametes. This is not really correct. Gametes are considered a loss of carbon and nitrogen to the individual mussel but are not part of the particulate waste. Maybe the should just delete “and gametes” from the end of the first sentence Still needs to be edited out.

On page 62 (top sentence) and again on page 64 (section 6.2.3) they indicate that dissolved organic material forms a small but still important component of the bivalves diet. This is not correct. Although bivalves have been shown to have the ability to take up DOM (e.g, work by Donal Manahan) their uptake kinetics do not approach that of bacteria. So in the natural world there is insufficient DOM in the water column for bivalves to be able to compete with bacteria (Langdon and Newell 1996). So delete this point from their report or provide the citations that support this assertion**.** They still include this incorrect point in their report. Leaving these errors in reduces the overall credibility of their work.

Langdon, C.J., and R.I.E. Newell. 1996. Digestion and Nutrition of Larvae and Adults. Pages 231-270, Chapter 6 In: V.S. Kennedy, R.I.E. Newell, and A. Eble (eds.) The Eastern Oyster, *Crassostrea virginica*. Maryland Sea Grant Publication

3: Reference Documents Not Provided, and/or Not Peer-Reviewed

The reference section is incomplete with many papers in the text not cited in the reference section (e.g., see p 69 where missing citations for Ruckleshaus et al. 1998, Navarro and Widdows 1997 etc.) I did not take the time to check all refs but that needs to be done. Within the references section there also errors See the Alberston et al. ref 2001 at the top of page 84. This is the same citation as they give as the third citation on that page for Alberston et al (2002) They cite Alberston et al (2002) in the first paragraph on page 32 for data on nutrient budget but when I looked at the paper cited on page 84 it had no information on nutrients.

On page 62 (second paragraph) they refer to information in Appendix D. The copy of their report I printed did not contain any appendices so I am not sure if appendices were not included or I simply overlooked them on the CD disk

In their report (see p 42, 48) they rely heavily on several unpublished studies undertaken by the Pacific Shellfish Institute (PSI) on the existing mussel aquaculture sites in Totten Inlet. They clearly acknowledge these data sources on p 4. I should imagine that these are high quality data sets but I have not had access to them. **Kenn Brooks responded to this point (see p 12 of his October 6 2008 report)** One reason I am concerned is that in their discussion of the PSI assessment of phytoplankton species composition (p 42 penultimate paragraph) is that they only refer to samples collected on 11/2/02 and 4/28/03. Both of these samples were taken in what can be considered "winter" which is a time of low mussel feeding. Did PSI not take samples for phytoplankton species composition in summer? In the final two sentences of this same paragraph it is not clear whether they are discussing Totten or Budd inlet data.

4: Editorial Observations and Comments

In section 2.1.4 dealing with "Flow in the immediate vicinity of mussel rafts" I found it confusing to know when they were presenting real data measured from existing mussel rafts at Deepwater Point versus model predictions of what the flow may be like at the proposed new raft site in North Totten Inlet. This section needs to be clarified so that the reader can better understand the differences between real data and model predictions.

This report contains some minor typographical errors and incomplete sentences. I only provide here a few examples of places in the text that need rewriting to clarify the meaning but I do have many more errors marked on my paper copy of their report.

Page 29 4 lines from bottom..mean "urine" and not "urea"
page 63 last paragraph in Section 6.2.2.1 on Zooplankton. The second sentence starting "As a group" needs to be clarified.
Page 64 line 8 "Production for wild and cultured clams, geoduck, oysters and mussels combined is driven by food availability for oysters and mussels" What does this mean I wonder?

Page 80 start of third paragraph.
Last sentence of fifth paragraph on page 82. "Consumption associated to the proposed"

Page 44 comment at top of Fig 34 regarding labels

5: Additional information/analyses required

Clarify their estimates about how much of the nitrogen inputs into Totten Inlet might be removed through harvest of mussels and associated fouling organisms.

Ensure that the unpublished reports that they cite area available for scientific review. These reports are cited but Dr. Ken Brooks states in his October 2008 "Response to review comments" the PSI study was never completed nor published.

Executive Summary....Totten Inlet baseline studies completed by Aquatic Environmental Sciences in 2002 and 2003
K.R. Brooks

1: Technical Observations and Applicability to Environmental Review of the Proposed Action (Your observations, concurrence and/or disagreement, based on the information provided)

This report is a useful summary and should be read first to understand what has been included in these other reports

I find the number of separate reports authored by K.R. Brooks slightly confusing. There is a considerable degree of overlap in the information among reports. Useful bits of information pertaining to one topic are separated among all reports. This makes it VERY difficult to find the most useful new information being presented in support of the EIS.

2: Technical Errors, Deficiencies, and/or Issues of Concern

3: Reference Documents Not Provided, and/or Not Peer-Reviewed

4: Editorial Observations and Comments

Personally I think that the information provide by Dr Brooks would be easier to access, if a single report had been prepared. It would have been different if each document were peer reviewed journal articles that are difficult to repackage but his various reports seem to have been written for this EIS review. But I do understand how this permit application has been going on for such a long period, with extra studies being requested, that it was perhaps inevitable that the final reports would be turned out as a series of reports. I do not recommend that a single synthesized report be prepared now!

5: Additional information/analyses required

The frequency of *M. edulis galloprovincialis* alleles in Washington State

Ken Brooks

1: Technical Observations and Applicability to Environmental Review of the Proposed Action (Your observations, concurrence and/or disagreement, based on the information provided)

This report addresses an often voiced concern that hatchery based cultivation of the commonly called 'gallo" mussel may result in increased interbreeding with *M. trossulus*, which is the mussel species commonly found in Puget Sound. The report contains both a literature review and summarizes species identification data collected by Dr. Brooks at several locations and over a period of years.

This report makes it clear that there is no direct evidence that *M. edulis galloprovincialis* is a non-native species introduced by humans to the west coast. Instead Brook reviews some literature suggesting that this mussel may possibly be a native species along the southern Pacific coast of the USA. There seems little chance that this controversy is going to be definitively resolved any time soon. No matter when or how it was introduced, *M. edulis galloprovincialis* is now firmly established on the west coast and will likely continue to expand its distribution until it fills all suitable niches on the west coast, if it already has not done so. Mussels can disperse widely along coastlines as has been seen with many aquatic species that have similar long larval dispersal and an adult form that can easily attach to boats.

This issue of genetic dilution associated with hybridization between a natural and hatchery reared species is not an easy one to resolve conclusively. It seems as if aquaculture of gallo mussel will result in higher levels of hybridization between the two species of mussel. Brooks reports (page 11 line 11) that in Totten inlet, where intensive culture of *M. edulis galloprovincialis* has been practiced for the last 20 years, that between 3 and 10% of wild mussel may contain M. *edulis galloprovincialis* genes. But of course there are no data concerning what the level of hybridization was in this location before mussel aquaculture was initiated. It would certainly be useful to inform future application for "gallo" mussel aquaculture permits to continue periodic monitoring of Totten Inlet to determine if hybridization increases over time.

The question that still remains to be answered is to what extent these levels of hybridization between the two mussel species are going to harm native/wild stocks of mussels. There are no data or guidelines concerning how much interbreeding is acceptable and when it may pose a problem for the endemic *M. trossulus.* Consequently, it is not possible to provide absolute assurances concerning the consequences of hybridization between the two species. What is certain is that there is no evidence from Europe, where *M. edulis galloprovincialis* naturally co-occurs with *M. edulis,* that *there* are any problems with such a species overlap. Furthermore, because *M. edulis galloprovincialis* and *M. trossulus* are able to interbreed so readily it is an indication that they are not widely separated species. Hence the exchange of genetic material between the two species may be akin to the natural processes in species that allow greater genetic diversity and possible flexibility in response to changing environmental conditions. It is interesting to speculate that with global climate change the environment in Puget Sound may becoming more conducive to *M. edulis galloprovincialis* and less hospitable for *M. trossulus.* So

in addition to hatchery propagation increasing *M. edulis galloprovincialis* this species may start to naturally reproduce more widely in Puget Sound, thereby allowing it to become a permanent part of the benthic community. It is also possible that the hybrid between the two species may be more successful that either pure species.

2: Technical Errors, Deficiencies, and/or Issues of Concern: None

3: Reference Documents Not Provided, and/or Not Peer-Reviewed

Brooks cites his own and others data to make the important point that *M. edulis galloprovincialis* and *M. trossulus* have very different requirements for water temperature and salinity, making direct competition between the two species likely to be relatively uncommon in Puget Sound. Unfortunately, the references cited in Brooks' report (penultimate and final paragraph on page 2) to this environmental tolerance work (Heritage 1983, and Bower 1989) are not included in the bibliography of his report. This means that I can not check these citations and so do not know if it is data that has been published in peer reviewed journals. Brooks's citation to his own work (Brooks 1991) is his own unpublished doctoral thesis which was also not available to me.

4: Editorial Observations and Comments: None

5: Additional information/analyses required

Risk Analysis for the proposed NTI mussel farm
Ken Brooks

1: Technical Observations and Applicability to Environmental Review of the Proposed Action (Your observations, concurrence and/or disagreement, based on the information provided)

This document contains some useful information about what information needs to be taken into account when developing a risk analysis. Especially useful is the introduction of the concept of 'fallowing" of an aquaculture area (see page 5 penultimate paragraph) to allow any build up of organic material to be reduced over time. Brooks reports that a period of 4 months may be sufficient for sediment particulate organic matter and sulfide concentrations to be reduced to background levels. Such geochemical information on sediment composition can be easily measured and used by aquaculturists to manage their farms. In my opinion allowing aquaculture farms to have available a larger area of bottom over which they can periodically reposition their aquaculture rafts is a good way to reduce possible long-term adverse effects of the farms on benthic processes. That is, if regulations constrain them to position their rafts at one fixed location that will more likely lead to adverse effects to underlying benthic communities.

2: Technical Errors, Deficiencies, and/or Issues of Concern

Unfortunately, the text of the report made available to this reviewer was found to be very incomplete. For example, the section on page 11 and 12 seems to be outline form only. I am not sure what Brooks intended to do for these parts or should he have just referred to a separate documents? There are also three pages of text after the references section that duplicates material earlier in the report.

3: Reference Documents Not Provided, and/or Not Peer-Reviewed: None

4: Editorial Observations and Comments

In section 4.1 page 10 it was difficult to follow what Dr. Brooks meant in the section labeled "Deposition of TVS." The scientific convention is that a writer defines acronyms at their first use in a document which Dr. Brooks does not always do. After reading Dr. Brook's other reports it is apparent that he is using TVS in the sense of the wastewater treatment industry to mean "Total Volatile Solids". This is the same as what is commonly referred to as "Particulate Organic Matter (POM)" in the marine science literature. In any event, in any final EIS report that is prepared it is imperative to use standard acronyms that are defined at their first use.

Some of the text included seems somewhat tangential to providing the scientific information upon which the EIS is going to be based. The final "Summary" section (starting on page 15) seems more like Dr. Brook's personal opinion rather than an objective review and it is not of direct relevance to the EIS.

5: Additional information/analyses required

Measurement of nutrients in bottom water and adjacent to Deepwater Point mussel farm in Totten Inlet

K.R. Brooks

1: Technical Observations and Applicability to Environmental Review of the Proposed Action (Your observations, concurrence and/or disagreement, based on the information provided)

Dr. Brooks measured nutrient release and some sediment chemical components on one day at distances from mussel rafts and at a nearby reference site. Their results indicate that only the sediments underlying the mussel raft and in its immediate vicinity show any appreciable changes associated with enhanced organic deposition. In another report **"Benthic response at the Deepwater Point Mussel farm in Totten Inlet**" (page 22 top of page) Dr. Brooks indicates that biodeposits from mussels on the rafts are detectable out to distances of ca 180 m form the perimeter of the raft system.

This is valuable information obtained from an active mussel aquaculture site. It indicates that under those physical conditions (flow, phytoplankton concentrations, bottom water oxygen content, etc) and mussel stocking densities that the receiving sediments were not being overly enriched by mussel deposits. This is an important finding and a condition that is good for mussel farms to maintain. If sediments at distances away from the footprint of the mussel rafts start to show evidence of over enrichment (e.g., accumulation of Sulfide) the mussel stocking densities should be reduced or the rafts relocated. See additional useful discussion about fallowing of areas in the Brooks "Risk Analysis Report" (summarized in my review of that document)

2: Technical Errors, Deficiencies, and/or Issues of Concern

In this report Brooks summarizes results from a small scale preliminary (his words..page 3 penultimate paragraph) study on sediment biogeochemistry and nutrient release from sediment underlying the mussel aquaculture rafts Deepwater Point. He states that this study was undertaken by the Pacific Shellfish Institute and funded by NOAA. It is unclear to me if this is the only report from that study or if Brooks simply summarizes here a more detailed final report. Certainly, there are no citations by Brooks for any other reports**.** The study suffers from limited replication as samples were collected only on one day.

Brooks incorrectly states that some nitrogen is bound to iron and buried in the sediments (page 8, five lines from bottom). Nitrogen does not get bound to iron; rather phosphorous can be bound to iron oxides and in this manner is prevented from being regenerated back to the water column.

3: Reference Documents Not Provided, and/or Not Peer-Reviewed: None

4: Editorial Observations and Comments: None

5: Additional information/analyses required: None

The epibenthic community observed in association with intensive raft culture of *M. galloprovincialis* in Totten Inlet
K.R. Brooks

1: Technical Observations and Applicability to Environmental Review of the Proposed Action (Your observations, concurrence and/or disagreement, based on the information provided)

Brooks quantified the species composition and biomass of organisms (commonly called the fouling community) other than the cultured mussels attached to the ropes and structures of the mussel rafts at Deepwater Point. This information on species composition and biomass was then used in the carbon flow model developed by NewFields Northwest to estimate how much of the phytoplankton production is being consumed by the additional suspension feeding animals in this fouling community. The biomass data has also been used by NewFields Northwest to estimate how much additional nitrogen may be removed from Totten Inlet associated with the biomass of these additional organisms being removed concurrently with the mussel harvest.

In the discussion (p 17) Dr. Brooks correctly points out that these additional organisms are an important source of food for many fish and mobile crustaceans. In addition, the mussels raft and the hanging ropes themselves provide the type of complex habitat that is used as refuge by many species of fish. These various attributes are well-recognized to be some of the positive contributions of aquaculture gear to ecosystem function. For example, Dealteris et al. (2004) have shown that both on-bottom and off-bottom aquaculture holding gear provides the type of spatially complex habitat that is sought by many species of mobile animals. These aquaculture structures provide a surface for plant and animal colonization (Mazouni et al. 2001) that then provide a food source for many animals.

Dealteris, J.T., Kilpatrick B.D., and Rheault R.B., 2004. A comparative evaluation of the habitat value of shellfish aquaculture gear, submerged aquatic vegetation and a non-vegetated seabed. *J. Shellfish Res.* **23**:867-874.
Mazouni N., Gaertner J-C., and Deslous-Paoli J-M., 2001: Composition of biofouling communities on suspended oyster cultures: An *in situ* study of their interactions with the water column. *Mar. Ecol. Prog. Ser.*, **214**, 93-102.

2: Technical Errors, Deficiencies, and/or Issues of Concern: None

3: Reference Documents Not Provided, and/or Not Peer-Reviewed: None

4: Editorial Observations and Comments: None

5: Additional information/analyses required: None

Benthic response at the Deepwater Point Mussel farm in Totten Inlet

1: Technical Observations and Applicability to Environmental Review of the Proposed Action (Your observations, concurrence and/or disagreement, based on the information provided)

The introduction to this report is a good comprehensive review of what happens to the infauna and sediment chemistry when organic material remaining in bivalve feces and pseudofeces is deposited to the sediment surface. There, the organic material supports aerobic respiration in metazoans and microbial heterotrophs. Once the level of aerobic respiration by organisms in the sediment exceeds the rate of resupply of oxygen from the bottom water then organisms with anaerobic metabolism become dominant decomposers of organic material. One such group are sulfur reducing bacterial which reduce sulfate in seawater to sulfide. It is as a consequence of this respiration that sulfide accumulates. Hydrogen sulfide is highly toxic and can kill many benthic species that do not have adequate access to oxygen from the overlying water column (see reviews by Pearson and Rosenberg (1978) and Diaz and Rosenberg (1995).

Pearson, T.H. and R. Rosenberg, Macrobenthic succession in relation to organic enrichment and pollution of the marine environment, *Oceanogr. Mar. Biol. Ann. Rev., 16*, 229-311, 1978.

Diaz, R. J. and R. Rosenberg, Marine benthic hypoxia: a review of its ecological effects and the behavioral responses of benthic macrofauna, *Oceanogr. Mar. Biol. Ann. Rev. 33*:245-303, 1995.

Dr. Brooks studied benthic response at the Deepwater Point mussel farm in order to see if that would help understand the effects that the proposed mussel rafts at North Totten Inlet may have on benthic processes. This seemed like reasonable idea to me until I realized that the two sites may not have comparable sediment characteristics. Sediment type and current velocity, which can be an important determinant of grain size, are key factors in governing many sedimentary biogeochemical processes and ultimately in controlling how susceptible those sediments and the benthic fauna are to nutrient enrichment. One of the major conclusions listed in the Abstract was that ”Significant biological effects were not observed in major biological endpoints (species richness, total abundance Shannon’s index etc)” associated with the mussel aquaculture rafts. Dr. Brooks makes it clear in this report that because most of the animals at this location were epibenthic associated with predominately coarse grain size sediments, such animals may not have been very susceptible to sediment organic enrichment and the consequent build up of toxic hydrogen sulfide. Furthermore, as Brooks makes it clear in the final summary section (page 40), if the study had been performed in an area with a more developed infaunal community typical of fine grain sediments there may have been a more obvious detrimental effect of the mussel raft on the benthos. Dr. Brooks did not specifically address this point about differences in sediment type between the existing mussel rafts at Deepwater point and at the proposed site at NTI. I have now added a clarification comments at the bottom page 20 which are directly relevant to the proposed mussel aquaculture site at NTI

Even though this detailed study at Deepwater Point mussel farm cannot be used to say definitively that there will be no effect on benthic communities I do think that the study provides some very important insights. Primarily, it shows that at the typical densities of mussels being

grown on the rafts in Totten inlet, and with the prevailing hydrographic conditions, that there are no discernable negative effects from the rafts outside of about 150 m. This is important as it clearly delimits the zone of impact as being rather small compared with the overall scale of Totten Inlet.

2: Technical Errors, Deficiencies, and/or Issues of Concern

This following text was informed by discussion with my colleague, Dr. J.C. Cornwell, Horn Point Laboratory, University of Maryland Center for Environmental Science. Dr. Cornwell is an experienced marine geochemist, which is an area in which I am conversant but not expert.

Dr. Brooks reported that he measured "Free sulfide" concentrations in sediment samples with an Orion 290A advanced portable ISE/pH/PRP meter with an Ionplus model 9616 BNC silver electrode. He used this for measuring sulfide by placing 5 ml of total sediment into a sulfide antioxidant buffer, (NaOH and EDTA with added L-ascorbic acid) in a beaker and stirring in the presence of the buffer solution and measuring sulfide after 5 minute equilibration Dr. Brooks regularly calibrated the electrode against a series of Na_2S standards. The sulfide values reported by Dr. Brooks are primarily measuring free pore water sulfide. But the EDTA and ascorbate in the buffer may bind some Fe from FeS, increasing the free sulfide concentration/activity Dr. Brooks was measuring. So in addition to the pore water sulfide he may have been liberating some of the particle bound sulfide. The dissolved sulfide assays performed by Dr. Brooks are useful in mapping the spatial distribution and relative concentrations of sulfide between locations. And apparently this methodology is used in monitoring the ecosystem impacts of aquaculture farms [e.g., salmon pen impact analysis (Wildish et al. 1999)]. But the method does not provide an accurate estimate of how much sulfide is in the dissolved phase and how much in the particulate phase. Brooks and Maknken (2003), in their studies of ecosystem effects of salmon culture, clearly note this distinction (p 268) "It should be emphasized that the sulfide probes used in collecting these data measure the total soluble sulfide (HS^-, H_2S, S^{2-}) available in sediments. They (probes) do not measure FeS^- or FeS_2."

In many geochemical studies an approach is used that measures separately the free sulfide in the dissolved pool and the sulfide that is particle bound (described in a following paragraph). The dissolved free sulfide is the material that is toxic to organisms but it is also important to know how much sulfide is bound to sediment. This is because this bound sulfide is an index of how much organic enrichment has taken place at a site over time and hence how long that site must be fallowed to allow this iron bound sulfide to be remobilized. This information on dissolved and solid phase sulfide may be useful for obtaining the most complete assessment of long-term (years) cumulative effects of aquaculture rafts. In Japan the have enacted a "Law to Ensure Sustainable Aquaculture Production" which uses acid volatile sulfide levels in sediment under and adjacent to aquaculture farms as one indicator of the assimilative capacity of the sediments (Yokoyama 2003) and the long-term effects of aquaculture (e.g., Tanigawa et al. 2007). There are still some problems to be overcome in rigidly applying set values of AVS as an indicator of ecosystem effects (Yokoyama 2003), but ongoing research is being undertaken to develop workable methodologies (e.g., Abo and Yokoyama 2007)

The point is that this sulfide probe method used by Brooks is not wrong. It is just that the data he has collected for Totten Inlet does not provide a baseline starting value that can be used in the future for assessing the long-term accumulation of particle bound sulfide in the vicinity of the aquaculture rafts. I do not perceive this as being a major problem as it would be relatively simple to collect some sediment samples from the North Totten Inlet site and have these analyzed for both dissolved and solid phase acid volatile sulfide.
The following is a method can be used for determining the amount of sulfide that is accumulating in both the dissolved phase in pore water and also the amount bound to sediment particles as so called "solid phase" sulfide. Samples are extracted from intact cores that are only opened and prepared in an oxygen-free atmosphere (i.e, in a glove bag purged with nitrogen gas). Total dissolved sulfide samples (1 ml) are fixed with 10 µl of a mixed diamine reagent (250 – 1000 µmol L^{-1}, Cline 1969) and stored at room temperature until analysis by methylene blue colorimetry no more than 1 week later. Homogenized sediments are analyzed for acid-volatile sulfides (AVS) using a 6 N HCl-extraction and $PbClO_4$ titration (Cornwell and Morse 1987). After analyses for AVS are complete, sediments are further processed for chromium-reducible sulfides (CRS) concentrations following Canfield et al. (1986). Other methods for analyzing AVS, based on GASTEC gas detectors columns, are used in Japan (e.g. Tanigawa et al. 2007)

Abo, K. Yokoyama H. 2007. Assimilative capacity of fish farm environments as determined by the benthic oxygen uptake rate: Studies using a numerical model. Biol. Fish. Res. Agen. 19:79-87.

Brooks, K.M. Mahnken C.V.W. 2003. Interactions of Atlantic salmon in the pacific northwest environment. II. Organic wastes. Fisheries Research 62:255-293.

Canfield DE, Raiswell R, Westrich JT, Reaves CM, Berner RA (1986) The use of chromium reduction in the analysis of reduced inorganic sulfur in sediments and shales. Chemical Geology 54:149-155.

Cline JD (1969) Spectrophotometric determination of hydrogen sulfide in natural waters. Limnol Oceanogr 14:454-458

Cornwell JC, Morse JW (1987) The characterization of iron sulfide minerals in anoxic marine sediments. Marine Chemistry 22:193-206

Tanigawa T., Yamashita A., Koizumi Y. 2007. Effects of effluents from a new fish farming site on the benthic environment. Biol. Fish. Res. Agen. 19:69-77.

Yokoyama, H. 2003. Environmental quality criteria for fish farms in Japan. Aquaculture 226:45-56.

Wildish, D.J., H.M. Akagi, N. Hamilton and B.T. Hargrave, 1999. A Recommended Method for Monitoring Sediments to Detect Organic Enrichment from Mariculture in the Bay of Fundy. Canadian Technical Report of Fisheries and Aquatic Sciences No. 2286. 31 pp.

On page 2 Dr. Brooks makes the point that mussels are not efficient grazers and that only 16 to 20% of the food filtered is incorporated into mussel tissue. He then states (page 2 19 lines from bottom of the page) that "the remainder is recycled back into the environment - much of it in particulate form" Although this is a minor point that reasoning is not quite correct. Of the food ingested through the mouth typically about 70% is digested and absorbed with the remaining ca 30% of undigested material being lost in feces. Of the material digested and absorbed some is loss as excreta but the vast majority (75%) is respired and lost predominately as carbon dioxide. So of the material filtered only about 30% is actually recycled as particulate material. If the mussels are feeding at high seston loads and producing pseudofeces then a greater proportion of the filtered material may be voided as particulate waste

3: Reference Documents Not Provided, and/or Not Peer-Reviewed

4: Editorial Observations and Comments

5: Additional information/analyses required

Baseline Information describing Sediment Physiochemistry of Totten Inlet and the macrobenthos of the proposed North Totten Inlet mussel farm
K.R. Brooks

1: Technical Observations and Applicability to Environmental Review of the Proposed Action (Your observations, concurrence and/or disagreement, based on the information provided)

In the Abstract on page 1, Dr. Brooks states that the sediment at Totten Inlet have highly elevated levels of particulate organic carbon and slightly elevated sulfide. On page 2, Dr. Brooks states (Page 22 last sentence ".----(Totten) inlet appears to be near its assimilative capacity for carbon" This is an important point as it suggests that the sediments in Totten Inlet may be susceptible to becoming anoxic if suspended mussel rafts deposit sufficient organic matter in their feces and pseudofeces to increase sediment microbial metabolism. This rather important point needs some detailed discussion among the peer reviewers to ensure that we understand the implications of the inherently low bottom water dissolved oxygen and high organic levels in the sediments of Totten Inlet has for siting new aquaculture rafts.

In Dr. Brooks study of sediments under the Deepwater Point mussel rafts (see his report "Benthic response at the Deepwater Point Mussel farm in Totten Inlet top page 24) he found no evidence of organic enrichment from the mussel rafts in sediments much beyond about 150 m from the immediate footprint of the rafts. But the sediments underlying this particular mussel farm are coarser than most in Totten Inlet and the benthic communities typical of such coarser sediments are not as susceptible to sulfide build up as those found in finer sediments. Interestingly, Dr. Brooks also found that the sediment grain characteristics near the proposed North Totten Inlet mussel raft site were also not homogenous. He found (page 66 paragraph 3) that sand was in the immediate location of the farm and to the North East. The higher porosity of such sandy sediments allow greater sediments pore water exchange with the overlying water column than the finer grain muds found elsewhere near the proposed mussel farm. These more porous sandy sediments allow greater oxygen penetration into the sediments and hence reduces the rate of sulfide build up. Such spatial variations in the composition of the underlying sediment, together with information on how prevailing water currents will transport the biodeposits, should be considered when determining the optimum location for mussel aquaculture rafts. By positioning the rafts at sites that can best assimilate the particulate waste it will appreciably reduce any build of toxic sulfide in the sediments. Dr. Brooks did not add any additional information concerning this specific comment. But from the various reports it is apparent that the proposed area of the NTI mussel lease has such good tidal flow that it will lead to strong dispersal of any biodeposits. This dispersal will mean that there should not be discernible changes in the sediment biogeochemistry and benthos beyond a zone of a few hundred meters from the propsed mussel rafts. This statement is based on Dr. Brooks report "Benthic response at the Deepwater Point Mussel farm in Totten Inlet" (page 38 second paragraph) that moderate to minor accumulation of sulfide were limited to a zone with 145 m from the perimeter of the mussel rafts at peak production. On page 39 (third paragraph) of that same report, Dr. Brook states that that effects on the benthic animal community were apparent to a distance of ~ 60 m from the mussel rafts

But if changes in benthic communities are observed it would seem a good idea to allow the rafts to be moved slightly to the NE. As can be seen in Fig 15 the sediments in that area are appreciably coarser than under the proposed raft location. The epibenthic communities associated with coarser sediments will be more resilient to the effects of sediment organic enrichment. Further, these coarser sediments are likely maintained by the high bottom water currents that cause the "scour bowl" effect in this part of Totten Inlet (e.g., last paragraph p 68)

Dr. Brooks discusses potential adverse effects of sediment over-enrichment on benthic communities in his report "**Risk Analysis July 17, 2005"** (page 10 penultimate paragraph). Dr. Brooks states in that report that monitoring underneath the rafts will be performed "with respect to anticipating changes in the macrobenthic community." But then no details are provided of what such monitoring might entail or how the mussel growers may respond if the sediments do become anaerobic. Dr. Brooks reinforces in the introduction to this report this point about the need for monitoring sediment conditions under the mussel floats. He then suggests (page 3 second paragraph and also p65 second paragraph) that geochemical analyses that are relatively cost efficient provide useful information for understanding the impacts of aquaculture farms on benthic habitats. I agree with this conclusion that analysis of dissolved and particulate sulfide concentration is a valuable monitoring tool. But see my comments (above) that sulfide electrodes are not a reliable method for measuring accumulation of particulate phase sulfide. Overall, this was a thorough and well conducted survey of the sediments and benthic communities at the proposed mussel aquaculture site. I am in total agreement with Dr. Brook's assessment (page 65 second paragraph) that because the sediments in Totten Inlet are already showing symptoms of high organic matter deposition it will be difficult in the future to determine if changes in the benthic environment are associated with the mussel farm or are part of these system-wide changes in organic carbon deposition.

I support the suggestion by Dr. Brooks of primarily geochemical analysis of the adjacent sediments when the mussels are feeding most actively during summer. These data will need to be compared with a suitable reference site to ensure that possible changes due to the mussel lease can be separated from changes in sediment enrichment due to the high background levels of organic deposition in Totten Inlet that I discuss above.

2: Technical Errors, Deficiencies, and/or Issues of Concern

There appears to be a contradictory statement in the Abstract. On line 5 Dr. Brooks writes that "Free sulfides were moderately low..", yet on line 19/20 he states that "sediments were organically enriched with **high** total organic carbon and **sulfide** concentrations.

I have the same concern about the electrode method he used to measure sediment free sulfide as mentioned above for the report "Benthic response at the Deepwater Point Mussel farm in Totten Inlet."

3: Reference Documents Not Provided, and/or Not Peer-Reviewed: None

4: Editorial Observations and Comments: None

5: Additional information/analyses required: None

Supplemental Study of Dissolved nutrient and particulate organic matter in the waters near the proposed mussel farm in North Totten Inlet

K.R. Brooks

1: Technical Observations and Applicability to Environmental Review of the Proposed Action (Your observations, concurrence and/or disagreement, based on the information provided)

It seems as if a lot of this material overlaps quite considerably with the report of NewFields Northwest report on water column processes. They also developed a model to estimate how much of the phytoplankton in Totten Inlet would be consumed by mussels growing on the proposed new rafts. Their assessment was that < 1% of the estimated carbon production throughout the year in Totten Inlet would be consumed by these extra mussels. This is reasonably close to Brooks's assessment in this report that ~ 8% of the carbon production would be consumed by the new mussel farm. This gives some confidence to these estimates because the models used in these two reports are very different with different assumptions etc.

2: Technical Errors, Deficiencies, and/or Issues of Concern

3: Reference Documents Not Provided, and/or Not Peer-Reviewed

4: Editorial Observations and Comments

5: Additional information/analyses required

Literature Review and model evaluation describing the environmental effects and carrying capacity associated with the intensive culture of mussels

1: Technical Observations and Applicability to Environmental Review of the Proposed Action (Your observations, concurrence and/or disagreement, based on the information provided)

This document is composed of nine science chapters plus an introductory and conclusion chapter. It is difficult for me to review this information as completely as I have done for Dr. Brooks other information for several reasons. Some of these chapters seem to be early drafts of material that Dr. Brooks has presented in greater detail in other documents that I have already reviewed above. And in other chapters, Dr. Brooks addresses issues that are covered in greater depth in the reports from Newfields Northwest and Evans Hamilton. I gather that this large degree of overlap between this document and these other reports is because these latter reports were prepared to address the concerns raided by earlier technical reviews. No matter what the cause of this overlap, I think that it is rather confusing to have so much duplicative technical material submitted as part of the written record. I suggest that Taylor Resources work with Dr. Brooks to select the material from this "Literature Review and model evaluation .." document that is still germane and only include that material as part of the written record. I make some suggestions below regarding what I feel is useful information.

I provide here a few comments about each of the chapters:

Chapter 1: **Introduction:** Not really necessary

Chapter 2: **Mussel culture**: Not much useful information. Consider omitting.

Chapter 3: **Taxonomy and identification of Mytilus edulis spp**. A very useful description of the identifying characteristics of the mussel species being discussed in the EIS.

Chapter 4: **Physiology of the *M. edulis* species complex**. Some of this basic information on mussel feeding and reproduction is useful background information and relevant to the EIS. Dr. Brooks uses some of the information he reviews to provide rough estimates of likely ecosystem effects of the increased mussel culture on organic matter deposition and nitrogen removal (page 12 - 15). These calculations overlap the mathematical model estimates of ecosystem effects included in the Newfield Northwest report. This is confusing as it is not clear which models are being put forward by the consultants as the most useful for informing the EIS. I recommend that the parts of the text dealing with estimating ecosystem effects be deleted from here.

Chapter 5: **Genetic Characterization and distribution of marine mussels:** A great deal of the most useful information in this chapter is also covered in the separate document by Dr. Brooks "**The frequency of *M. edulis galloprovincialis* alleles in Washington State"** Therefore, in order to simplify the way that related information is submitted I suggest that chapter 5 be deleted. If any information considered to be essential by the applicants to the EIS would be lost

then that information could be included in "**The frequency of *M. edulis galloprovincialis* alleles in Washington State"** document. An example of the type of information that does not seem to me necessary for preparing the Totten Inlet EIS is the information on "Doubly uniparental Inheritance of MT DNA" (Section 5.3 on page 28)

Chapter 6: **Origin and geographic distribution of the *Mytilus edulis* species complex in the Pacific Northwest.** Again some of the information contained here is reported in the separate document "**The frequency of *M. edulis galloprovincialis* alleles in Washington State".** That duplicated information on species origin could be deleted from here leaving just the useful information in section 6.2 onwards on the spatial distribution of mussels in the intertidal zone.

Chapters 7: P**hysical and Chemical Characteristics of Totten Inlet**: This chapter should be omitted as it has been superseded by the Newfields Northwest and Evans Hamilton reports.

Chapters 8: **Phytoplankton Production in Totten Inlet**: This chapter should be omitted as it has been superseded by the Newfields Northwest report.

Chapters 9: **Bivalve Resources in Totten Inlet and Estimates of Carrying Capacity**: This chapter should be omitted as it has been superseded by the model based on Rodhouse et al included in the Newfields Northwest report.

Chapter 10. **Environmental effects associated with suspended mussel culture**. The information included here has been greatly expanded in a number of Dr. Brooks other reports and so I think this chapter can be deleted.

Chapter 11**: Summary, Conclusion and Recommendations**: I think that this chapter is not that appropriate for technical information submitted in support of EIS. This chapter is more of Dr. Brooks' opinion and is based on him interpreting the factual information to provide a response to a series of questions relating to environmental effects of mussel culture. I think that such interpretations should be left to the persons tasked with preparing the EIS.

2: Technical Errors, Deficiencies, and/or Issues of Concern

3: Reference Documents Not Provided, and/or Not Peer-Reviewed

4: Editorial Observations and Comments

5: Additional information/analyses required