Hi Nathan. Thank you for the opportunity to comment on this draft EIS. Clearly you have put a lot into this. I hope that these comments can strengthen this document.

Jim Kaldy

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From: "Jim Kaldy" <zostera1@gmail.com>
Date: Feb 14, 2014 4:54 PM
Subject: note to self
To: "Jim Kaldy" <zostera1@gmail.com>
Cc:
Comments on Draft EIS: Management of *Zostera japonica* on commercial clam beds in Willapa Bay, Washington.

Comments provided by:

James Kaldy
South Beach, OR 97366

14 February 2014

Dear Mr. Lubliner:

Thank you for the opportunity to comment on the draft EIS regarding the management of *Zostera japonica* in Willapa Bay. There is a large volume of information presented and I recognize that there are very real constraints (e.g. document availability, publication of new work, time limits, etc.) on projects such as this undertaking. This is a large undertaking.

I am a seagrass ecologist by training and have published over 25 peer reviewed scientific papers focused primarily on the biology, physiology and ecology of seagrasses. I have extensive research experience with seagrasses along the Atlantic and Gulf coasts as well as in the Pacific Northwest. As a result, I am very familiar with the scientific literature regarding seagrass ecology. My comments reflect my personal opinions.

General Comments

Reading this EIS, I found that there was pretty good use of the primary scientific literature. However, I did notice that there are numerous citations to unpublished “white papers” that have not been vetted through the scientific community. In some places there is a distinction made between published and unpublished work, while in other areas this distinction is not noted. These documents certainly provide valuable information, but I don’t believe that they should be considered with the same level of confidence as published results.
There have been a number of recent peer reviewed publications regarding the biology, physiology and ecology of *Zostera japonica* in the Pacific Northwest that were not included in the draft EIS. Specifically these include:


Of particular relevance to this EIS is the Shafer et al. review article published in 2013 in Environmental Management. This paper summarizes the most current knowledge of *Z. japonica* biology and ecology in North America as well as suggesting a number of important actions that need to be considered. The other works provide detailed information on the physiological tolerances of the plant. Feel free to contact me if you have problems obtaining copies of these papers.

This draft of the EIS appears to rely heavily on the comments and statements of the shellfish industry. For example, the “economic analysis” (section 2.5, pg 30) appears to be based on numbers from a single shellfish grower, extrapolated to the entire estuary. This is a reasonable “first approximation” however it is not a complete socioeconomic analysis. There has been no valuation of potential positive benefits (e.g. ecosystem services such as nutrient removal, pH amelioration, etc.). The economic impact analysis (publication 14-10-002 provided as part of the EIS package) only assess how the fees and costs associated with the NPDES permitting process would influence the shellfish growers; this also is not an full economic impact analysis. It is my opinion that an independent economic analysis would be beneficial to the process.

With respect to Imazamox, I think that there are several statements that should be clarified. First, the statement about persistence in the marine environment is unsubstantiated. It has been shown that Imazamox decomposes rapidly in oxic, loamy soils (e.g. soybean fields); however, estuarine sediments are not well oxygenated agricultural soils. Estuarine sediments are waterlogged and tend to be anoxic within the top 1-3 mm (Day et al. 1989 Estuarine Ecology Wiley & Sons; Bianchi 2007 Biogeochemistry of Estuaries, Oxford Univ. Press) and may have high organic content as well as charged clay particles. Photodegradation in oxygenated
conditions is considered the primary degradation mechanism; however, light levels at the sediment surface (under seagrass canopy) may not be sufficient to break down the chemical structure of Imazamox. Additionally, the oxygen status of the water column may be influenced by ocean conditions. Tidal exchange has been shown to advect hypoxic water from coastal upwelling into estuaries (Hickey and Banas 2003 Oceoaporation of the US Pacific Northwest coastal ocean and estuaries Estuaries 26: 1010-1031; Brown & Ozretich 2009 Coupling between coastal ocean and Yaquina Bay. Estuaries and Coasts 32: 219-237; Brown and Power, 2011 Historic and recent patterns of dissolved oxygen in the Yaquina Estuary. Estuarine Coastal Shelf Science 92: 446-455) which coupled with sediment anoxia can further reduce the oxygen availability for Imazamox degradation. Upwelling is common during the proposed Imazamox spraying period during May and June. Chemical degradation of Imazamox has never been tested in estuarine sediments; however, studies indicate that “imazamox does not degrade under anaerobic conditions” (EPA 2008, pg 4). Consequently, the persistence or chemical degradation of Imazamox in a marine system may be different from that of a soybean field. I think it would be prudent to consider monitoring for imazamox and its degradation products.

Second, in my opinion the susceptibility of phytoplankton, microphytobenthos (MPB) or unicellular microalgae (or macroalgae) to Imazamox has not been adequately evaluated. Contrary to the statement made on pg 18, testing has shown that some phytoplankton (specifically a planktonic marine diatom, Skeletonema costatum) were susceptible to Imazamox at concentrations an order of magnitude below the proposed permit level (EPA 2008, pg 22). The draft EIS (Pg 66) indicates that the lowest effect level was 10 to 40 ppb for algae, diatoms and aquatic vegetation. The proposed permit level for imazamox is 500 ppb. On pg 93, paragraph 6, there is discussion of experiments conducted on green and blue-green algae (Neatherland et al. 2009). It is important to note that the Neatherland et al. (2009) experiments were conducted with species that are not generally the dominant component of estuarine phytoplankton or MPB which support bivalve communities. Blue-green algae are actually prokaryotes (in a different kingdom from other phytoplankton or MPB’s) and are characterized by having different photosynthetic pigments and may have different biochemical pathways (e.g. influencing susceptibility to ALS inhibitors). Consequently, the response of blue-green algae to Imazamox may not be a good model system for extrapolating the effects to diatom dominated estuarine systems. I suspect that the purpose of the blue-green algal experiments was to evaluate the use of Imazamox to break up blue-green harmful algal blooms (HAB’s). I think it would be reasonable to consider monitoring or evaluating phytoplankton and MPB (or sediment microalgae) response to Imazamox.

Other specific comments

- Pg 70, 4th paragraph. Halophila stipulacea is a second seagrass species that some researchers consider to be “invasive”.
Recent publications have described optimal temperatures for growth and photosynthesis of *Z. japonica*. See references above.

See zonation description in Shafer et al. 2014.

See comparison of photosynthetic parameters of Zj and Zm Shafer and Kaldy 2014.

Statement about 40% reduction in water flow. The plaster of paris dissolution method does not reliably quantify water flow. Clearly the presence of seagrass reduces water flow but the magnitude of the reduction is not well constrained. See Porter et al. 2000 for further explanation. (Porter et al. 2000 Gypsum dissolution is not a universal integrator of ‘water motion’. Limnology and Oceanography 45: 145-158).

It is not clear how this paragraph relates to the section, since this was clearly the Dr. Bando’s opinion and not the research on interactions.

Statement that nitrogen is primary limiting nutrient. While this is true in many temperate estuaries particularly along the Atlantic coast of North America it is not necessarily true in PNW estuaries that receive high ambient loads of nitrogen from coastal upwelling as well as from watersheds dominated by nitrogen fixing Red alder (*Alnus rubra*) (Brown and Ozretich 2009 cited above). There is little evidence for seagrass N limitation based on C:N:P ratios in PNW estuaries (Kaldy 2006 Carbon, nitrogen, phosphorus and heavy metal budgets: how large is the eelgrass sink in a temperate estuary Mar Poll Bull 52: 332-356; Kaldy & Lee 2007 Aquatic Botany 87: 116-126), where plants tend to have >2% leaf N and sediment nutrient concentrations above the limitation threshold of about 100 µM.

Recent work suggests ... habitat that supports other biota”. This statement appears to be based on a letter report by Dr. Richard Wilson that was provided during a previous public comment period. The peer reviewed literature indicates that seagrasses enhance microalgal production that supports shellfish. Several studies have concluded that microalgal production in seagrass beds accounts for >60% of the total ecosystem primary production (Morgan and Kitting 1984 L&O 29: 1066-1077; Moncreiff et al. 1992. Marine Ecol Prog. Sers. 87: 161-171, Moncreiff and Sullivan 2001 Mar Ecol Prog. Sers. 215: 93-106; Kaldy et al. 2002 Estuaries 25: 528-539). Microalgae fix more carbon than the seagrass because they have higher photosynthetic rates, lower light saturation points and the seagrass blades provides more surface area for the microalgae (e.g. diatoms) to colonize than mudflat. Additionally, work from the Atlantic coast suggests that there is a positive relationship between seagrass presence and hard clams (Irlandi 1994 Oecologia 98: 176-183, Irlandi and Peterson 1991 Oecologia 87: 307-318).

Herring spawn has been documented on several occasions in Oregon estuaries by ODFW staff. ODFW staff have documented with maps the extent...
of spawn, and estimated the amount of spawn (tons) and the number of spawning fish. See citations in Shafer et al. 2013.

- Pg 91, section 3.2.4, second paragraph. See comments above. The literature suggests that there is increased microalgal production associated with seagrass beds. Stable isotope data as well as empirical production estimates indicate that microalgal production accounts for >60 of the total primary production (Morgan and Kitting 1984 L&O 29: 1066-1077; Moncreiff et al. 1992. Marine Ecol Prog. Sers. 87: 161-171, Moncreiff and Sullivan 2001 Mar Ecol Prog. Sers. 215: 93-106; Kaldy et al. 2002 Estuaries 25: 528-539). This type of work has not been conducted in the PNW, which is further reason to require monitoring of the microalgal community.