The risks of shell-boring polychaetes to shellfish aquaculture in Washington, USA: A mini-review to inform mitigation actions

This paper provides a mini-review of shell-boring *Polydora* species; their impacts, life-history, distribution, spread, control mechanisms etc. The paper also focuses mainly on *P. websteri*. Although the manuscript includes all the relevant information, I feel that it requires significant reorganisation before it can be considered for publication.

On p 4, Line 53, the authors say "Given the negative impacts of Polydora spp. on shellfish aquaculture in other regions, its presence in Washington State warrants a region-focused review to inform further investigation and stakeholder awareness." Furthermore, the authors seem to also focus on *P. websteri* but I find it very confusing in the way that this is addressed.

1) It is not immediately clear why the authors have focused only on *Polydora* species, and not also on related species such as *Boccardia* that have similar life histories and impacts. Either the authors should expand to include shell-boring polydorid species in general, since these are mentioned in Table 1, or they must specify clearly why they are focusing only on *Polydora* – after all, there is always a chance that *Boccardia* species or species from other genera may soon be introduced to the area, or may already be there, but at undetectable levels.
2) Additionally, the authors identify the main *Polydora* species in Washington as *P. websteri*, but only intermittently seem to single out this species in their discussions in the different subdivisions. For example, the authors switch between general discussion of larval developmental mode in *Polydora* species in general and *P. websteri* in particular, but don't provide any information on what is known on the species in Washington State. This occurs intermittently throughout the manuscript, and I suggest that the authors provide more coherent information regarding the species.
3) It would be useful if the order in which the subdivisions are discussed is re-organised to minimise repetition.
    a. For example, the authors discuss the identification of the species AFTER they provide quite a bit of information on the biology. This should, I think, come earlier, to avoid repetition and to better contextualise the specific focus on *P. websteri.* And on the topic of identification – I am not sure what the point is in repeating the phylogenetic trees from an already published paper; they don't add anything to the paper.
    b. Similarly, having a specific subsection on the impacts also repeats quite a bit that is said early on in the manuscript. It should probably be combined with Host Pathology.
    c. The monitoring and regulation section can include a brief overview, and subdivisions considering global, USA and Washington examples.
4) I also think that it would be better if after each topic is reviewed, the information is contextualised with respect to their occurrence in Washington (and or *P. websteri*) – for example, the authors summarise the annual cycles in *Polydora* larvae abundance (seasonal, when high phytoplankton is available), but there are no predictions as to when this may occur in Washington. Check all subsections and recontextualise the information accordingly.
5) Some topics are addressed very superficially.
    a. For example, on p10, lines 195-197: But if eradication of P. websteri is not possible, it could still be contained to a few Puget Sound basins through education,

mitigation, and regulation (Çinar 2013; Paladini et al. 2017) – expand on the kinds of education, mitigation and regulation programmes you have in mind. Even by referring to a different part of the manuscript.

b. lines 197 – 200, you say: If P. websteri has been present but dormant, the high infestation intensity reported by Martinelli et al. (2019) may be the result of a recent outbreak, caused by factors such as genetic changes, relaxation of biotic pressures (e.g. predators), or environmental changes (e.g., ocean warming, siltation). But you don't expand on this – is there any evidence that predation pressure has relaxed, or that the area has experienced ocean warming and siltation?

c. When you discuss control measures used elsewhere, you discuss quite a few, but only discuss how one (using hypersaline work) may be applied in Washington. I suggest you consider all the options and their applicability – why would exposure during low tide not work in Washington? Why are any of the others applicable or not.

6) Please check your citations – in some places a lot of information is provided without any citations. At least one citation (Williams and Grosholz, 2008) does not appear in the reference list and in text citations are listed inconsistently – are they meant to be in chronological or alphabetical order?

7) You need to check the correct identifications of the species cited. You refer to *P. ciliata* as a borer in multiple places – undoubtedly this is how the original authors of the papers identified them, but you need to acknowledge that these identifications (form various places around the world) are probably incorrect as *P. ciliata* is not a shell-borer (see Blake and Kudenov 1978 who suggested that at least all records of this species as shell-borer in Australia may actually be of *P. websteri.* This is also discussed in Simon and Sato-Okoshi 2015). Additionally, *P. uncinata* was synonymised with *P. hoplura* (Sato-Okoshi et al. 2017 and Radashevsky et al, 2017).

8) Although the summary includes a brief overview on future research, etc., I feel that the manuscript will benefit from a more structured 'plan of attack' for future research, and modified management strategies and regulations. It may help to include a flow chart to show how different information may be collated to this end.

Additional specific comments are made on the document.