

Ocean Sci. Discuss., referee comment RC1
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Comment on os-2021-66

Jennifer Jackson (Referee)

Referee comment on "A clustering approach to determine biophysical provinces and physical drivers of productivity dynamics in a complex coastal sea" by Tereza Jarníková et al., Ocean Sci. Discuss., <https://doi.org/10.5194/os-2021-66-RC1>, 2021

The manuscript led by Jarníková applies a clustering approach to ocean model output from the Salish Sea to evaluate regional differences including which physical factors are associated with phytoplankton growth. The manuscript is clear and well-written and has several interesting findings including a clear partitioning of physical processes and phytoplankton dynamics into different regions. I have a few major concerns and minor comments that should be addressed before I can recommend this manuscript for publication.

Major concerns

My first major concern is the lack of comparison phytoplankton model output with literature. Figure 9 shows that diatoms are the dominant phytoplankton group in the summertime. Yet Figures 5 and 7 in a recent observational paper by Del Bel Belluz (2021), whose study years overlap with this manuscript, shows that smaller cells (grouped as dinoflagellates and ciliates in this Jarníková et al.'s manuscript) are in fact usually dominant. In addition, from remote sensing data, Suchy et al. (2019) show that the spring chlorophyll concentrations were anomalously high in 2015 yet these high chlorophyll concentrations were not replicated by the model. I suggest that the authors add a section on model evaluation that compares model and observed phytoplankton functional groups and concentrations in the different regions. This would help solidify the model results.

My second major concern is the interpretation presented in section 4.1, which suggests the difference in phytoplankton biomass between the central and northern Strait of Georgia is linked to strong summer wind events in the northern Strait of Georgia (lines 320 to 323). When I look at Figure 5, I see that summer winds are in fact stronger in the central Strait of Georgia so I don't think that the explanation holds. Could the authors please clarify how they came to this result?

My third major concern is that line 108 states that SalishSeaCast has been operational since 2014 and runs daily forecasts and nowcasts. Could the authors please explain then how this study analyzes data starting in 2013?

Minor concerns

- Line 2 – for clarity, I suggest that you list the four factors here
- Lines 15 to 31 – I found this section difficult to read, with long sentences and no clear points. I suggest that this section is rewritten more concisely, with points related to this study made more clear
- Figure 1 – I suggest that somehow the different regions (NSoG, CSoG and JdF) are labeled on the map. I know that the point of clustering was to help define these regions but it is difficult for people not familiar with this region to know what area the authors are referring to
- Lines 42 to 47 – The link between phytoplankton and fish is still tenuous. I suggest that the authors add a clearer ecological motivation for this study or add some more information about why phytoplankton is important for higher trophic animals.
- Lines 74 to 79 – This is where the question that the manuscript is trying to answer is defined yet I don't see a clear scientific question or objective here.
- Lines 93 to 94 – This wind data spatial resolution is too coarse to properly reproduce winds in most of the BC fjords. I suggest adding a sentence here that acknowledges that limitation yet explains why the model output from fjords is still accurate.
- Line 126 – I don't know how a cluster is characteristic. Was there a way that clusters were quantified to decide which 4 to choose?
- Figure 2 – It took me a while to figure out what the vertical axes labels meant. I suggest that you make these clearer (i.e. add the description of the variables) to help

the reader.

- Lines 133 to 134 – I don't understand this sentence. Please clarify.
- Lines 158 to 159 – I find this sentence confusing because I don't know what three yearly signals means.
- Lines 163 to 164 – How do the authors know that bottom-up effects dominate? Please explain.
- Lines 176 to 177 – Why was the clustering applied to 4 years separately instead of applying clustering to the whole 4 year time series?
- Lines 179 to 198 – As a non-clustering expert, I found these sections difficult to understand. I suggest that the authors simplify these sections so that they are geared towards non-experts.
- Figure 7 – It is hard to see the red dots
- Lines 233 to 234 – What is the source of the freshwater runoff data?
- Line 244 – I suggest that the authors define VED
- Line 262 – I think it would be good to mention here that most of the Discovery Islands, where the highest tidal energy is, isn't resolved in this model
- Line 269 – I'm not sure that you can classify the spring bloom in CSoG as early – based on Figure 8, it looks to me like the timing of the spring bloom is similar in all regions.
- Figure 10 – With the exception of 2015, it is difficult to see that the spring bloom started earliest in 2015. I suggest that the authors change the color scheme to make this clearer.
- Lines 379 to 380 – I don't see this in Figures 8 or 10 that the spring bloom started in CSoG and radiated outwards
- Figure A1 – In cluster 5, which is labeled as JdF, why are there dots from the NSoG?
- Figure A2 – The caption says salinity but I think the authors mean silicate? Also, what are the three biological clusters mentioned in the caption?