

Ocean Sci. Discuss., referee comment RC2
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Comment on os-2021-21

Jennifer Jackson (Referee)

Referee comment on "Wind-driven upwelling and surface nutrient delivery in a semi-enclosed coastal sea" by Ben Moore-Maley and Susan E. Allen, Ocean Sci. Discuss., <https://doi.org/10.5194/os-2021-21-RC2>, 2021

The manuscript by Moore-Maley and Allen uses model output from a high-resolution biophysical model to examine upwelling in the Strait of Georgia. This is an important research question. Upwelling is often discussed in the Strait of Georgia but has never been examined in detail or quantified. The authors use five years of model output (focusing on temperature and nitrate) and high-resolution wind climatology to study upwelling. In general, the manuscript is well-written and interesting and will add important knowledge about physical processes in the Strait of Georgia. That being said, I did struggle with sections 3.2 and 4 and think that considerable improvement is needed, particularly in these sections, before the manuscript is published in Ocean Sciences. I therefore recommend major revisions. Details are listed below.

Major comments

My first major concern is with the interpretation of the principal component analysis in section 3.2. Lines 303 to 310 describe the dominant modes from the EOF spatial patterns and based on EOF results shown in Figure 6. Despite the importance of these results for the manuscript, I think that important information is missing from the description on the EOF results. This includes:

- The percentage of variance calculated within each mode
- A description of how the modes were diagnosed (e.g. beyond a picture, how is it known that mode 1 of for nitrate is upwelling along the western shore?). Some work was done to diagnose the different modes in Figures 7 and 8 but these results were not always conclusive
- A description of what a mixing-heating pattern is (lines 305 to 306) and how this in particular was diagnosed.
- Throughout section 3.2 (and in the figures), there are several references to positive and negative variance. I don't know what positive and negative variance means in regards to these results. Please clarify.

My second major concern was the lack of discussion of stochastic events (i.e. storms) in the manuscript. HRDPS shown in Figure 4 shows the stochastic nature of the events that cause upwelling and downwelling, and the impact some of these events have on surface temperature and nitrate. Despite the frequency and strength of these events, there doesn't appear to be a stochastic (1 to 3 day) frequency in the power spectra on either Figures 5 or 8. If the authors are arguing that storm driven upwelling or downwelling are the dominant modes for temperature and nitrate variability in the Strait of Georgia then why don't stochastic events evident in the power spectra?

My third major concern was the confusion of reading a manuscript where many mathematical symbols are used throughout. To make this manuscript easier to read, I suggest adding a table that details all of the mathematical symbols.

My fourth major concern is section 4.1. This was a complex section and I'm not clear exactly how it strengthened the manuscript. Specifically, I think that some discussion is needed to explain why using a 2 layer model is realistic in a such a complex region where 3 to 4 layers (e.g. Stevens et al., 2021, Johannessen et al., 2014) are often observed. I suggest rewriting this section to emphasize to the reader why these case studies are needed and how they influence the results of the model. I also suggest that, if the case studies are used, the authors include figures of the results so that the case studies are easier to interpret.

I think some key references are missing. These include:

- Johannessen, S.C., Macdonald, R. W., and Strivens, J.E. 2021. Has primary production declined in the Salish Sea? *Canadian Journal of Fisheries and Aquatic Sciences*,
- Johannessen, S.C., Masson, D. and Macdonald, R.W. 2014. Oxygen in the deep Strait of Georgia, 1951-2009: The roles of mixing, deep-water renewal, and remineralization of organic carbon. *Limnology and Oceanography* 59(1): 211-222
- Del Bel Belluz, J., Peña, M.A., Jackson, J.M. *et al.* Phytoplankton Composition and Environmental Drivers in the Northern Strait of Georgia (Salish Sea), British Columbia, Canada. *Estuaries and Coasts* **44**, 1419–1439 (2021).
<https://doi.org/10.1007/s12237-020-00858-2>

Minor concerns

- Line 28 – I suggest adding references to previous research on upwelling in enclosed basins
- Lines 45 to 57 – I found this paragraph confusing and it was difficult to understand the point of the paragraph. I suggest rewriting this paragraph so the point is more clear.
- Lines 151 to 152 – Please add a reference here
- Line 165 – How realistic are these 2.5 km winds in some of the narrow channels within the Salish Sea? Do these coarse winds (relative to the complexity of the study area) impact the results?
- Line 208 – Figure 1 includes Juan de Fuca Strait yet this states that only the region to the tidal mixing area (Haro Strait?) is considered. Please clarify.
- Lines 209 to 213 – As a reader it was difficult to interpret what the authors are stating here. If possible, I suggest adding this information to a figure. Otherwise, please make this information clearer so that it is easier to interpret.
- Lines 214 to 226 – Are the references at the end of this paragraph for the whole PC and EOF equations? Please clarify
- Lines 257 to 258 – I don't understand the sentence starting with 'There is also a tendency...' Please clarify.
- Figure 2 – The letters in the figure to identify the panels (i.e. a to d) does not match the description in the caption.
- Lines 274 to 275 – I can't see this result in the figures.
- Lines 291 to 292 – It is really difficult to see the correlation between winds and temperature/nitrate at individual locations in Figure 4.

- Lines 306 to 310 – As mentioned above, it is not clear to me how these interpretations were made.
- Figure 7 – What do positive and negative winds and PC amplitude mean?
- Line 336 – How does the averaging window of 54 hours impact the storm data? In other words, does this averaging window minimize storm energy?
- Figure 8 – Again, what do positive and negative PC amplitudes mean? Also, Figure 8b shows significant energy at fortnightly and monthly frequencies. This EOF was interpreted as being dominated by tidal mixing. Please explain why tidal mixing would have significant energy here at these frequencies?
- Lines 571 – I think that much of the observational data used are available on CIOOS. I suggest that the authors add the CIOOS data link to the acknowledgements (<https://cioospacific.ca>).