Ocean Science Discussions

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Comment on os-2021-21

Anonymous Referee #1

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-RC1, 2021

GENERAL COMMENTS:

Moore-Maley et al. (2021) use output from a biophysical model to study wind-driven upwelling in the Strait of Georgia (SoG). The authors show that the predominant wind forcing is along the axis of the Strait, and use EOF analysis to reveal cross-strait "modes" in the variability of surface nitrate concentration (and, to a lesser degree, surface temperature). They go on to relate the variability of the cross-strait modes with that of the along-axis wind forcing. In the discussion section, the authors consider the response of a two-layer model of a rectangular basin and use that to frame a discussion about cross- and along-axis symmetry as a response to wind forcing.

The paper is commendably well-written. The introduction section is excellent, the graphics are nice, and the discussion and conclusion sections are for the most part clearly formulated. The choice of EOF analysis as the main diagnostics tool is appropriate in principle - although I think the methodology could be improved in several ways to strengthen the paper.

The main weakness of the paper is pointed out by the authors themselves (Section 4.5): The analysis rests on the surface expressions of nitrate and temperature, both of which are affected by a multitude of factors and thus are complicated and indirect proxies for upwelling. It is therefore not surprising that the relationship

between wind forcing and cross-axis empirical modes in fact appears relatively weak. It is my opinion that
the main conclusion of the paper would be substantially strengthened by including the analysis of one or
more variables more directly related to the dynamical process of upwelling. If the basic mechanism were
established, the following discussion of surface nitrate and temperature would be strengthened substantially
(or, if not established, that would raise some interesting questions). Conversely, if this is not done, the authors
should add clear modifiers to statements such as "[this study] explicitly identif[ies] wind driven upwelling in
the SoG" (line 83).

Nevertheless, I generally found this paper and the analysis within interesting and well-reasoned, and I believe
it could constitute a significant contribution to the understanding of nutrient availability in the upper waters
of the SoG, with implications for the understanding of ecosystem functioning in the area. The paper could
also contribute meaningfully to the understanding of wind-driven upwelling in enclosed bodies of water more

generally. I therefore recommend that this paper be published after appropriate revisions.

SPECIFIC COMMENTS (MAJOR)

1.

The authors base their analysis on the surface expressions of temperature and nitrate. For both parameters, it
is hard to separate the effects of upwelling, lateral advection, and wind-driven mixing, and both parameters
are presumably strongly influenced by tides and diurnally varying surface forcing. While I understand that
the authors wish to limit the scope of their study, I think the main conclusion (wind-driven upwelling plays a
major part in the dynamical response to wind forcing events and their e ects on nutrient distribution) would
be much stronger if the authors included an analysis of upwelling more directly. An advanced methodology
would not necessarily be required, and relevant parameters should be available to the authors in the model
output. For example, pycnocline/isopycnal depth at select locations along the respective sides could be used in
a simple comparison with along-axis winds, or they could use cross-axis isopycnal tilt at

an appropriate crosssection.
The point would be to more conclusively establish the asymmetric upwelling pattern as a response
to wind forcing events before going on to the more detailed spatial analysis and more complex discussion of
mechanisms. It would also in my view tie the theoretical framework of section 4.1 more together with the
results of the study.

2.
Figures 5/8 and the discussion of spectra: The spectra as they are are very noisy, and it is currently difficult to
discern peaks that are central in the description and analysis. I strongly suggest using block averaged spectra
(the record lengths should be more than sufficient to do so). This should make the signals of interest clearer
with respect to the background noise while still resolving the entire frequency band of interest. I also suggest
including error bars on the spectra given that minor peaks are given significance in the interpretation.

3.
In general, the relationship befween EOF indices and winds do not strike me as obviously strong, neither
in figure 7 nor in the correlation analysis. This does not mean that the wind-driven upwelling mechanism
suggested by the authors does not occur - especially given that tides and diurnal variability in surface forcing
likely adds a lot of "noise" and may affect the mode structures in different ways.
I wonder if the authors would be better off focusing explicitly on the *subtidal* variability in their EOF analysis
- rather than performing the EOF analysis on full-resolution data and then "detiding" indices and winds when
looking at correlations. It seems to me that the dynamically relevant time scales are all longer than a day: wind
pulses and ocean responses both appear to have longer time scales (hence the filtering in figures 4 and 7?), and

the response time of the upwelling process is also shown by the authors to be more than one day. The authors
already do the same at the opposite end of the spectrum by applying a 50 d high-pass filter. Could appropriate
filtering to explicitly focus on subtidal signals restrict the analysis to the frequency band of interest - and make
the results of the EOF analysis easier to interpret?

4.
Line 312-314: I have some difficulty seeing this described relationship between the mode

loading time series and
the winds in Fig 7. I suggest giving the reader some more specific pointers in the text, and perhaps indicating
"spikes" in Fig 7.

5.
Some sort of characterization of the time scale of wind events is needed - perhaps around Line 273. A typical
time scale is alluded to later in the manuscript (Line 395, 404), but never really described based on observations
or literature. I would suggest adding a description of the typical duration of both wind events (from HRDPS)
and upwelling events (from the ocean model).

6.
Section 4.1: While I found the theoretical exploration of the two-layer basin useful, I wonder if the SoG doesn't
deviate from the model in another fundamental way: It seems to me that since the the SoG is *not*, in fact,
closed at either end, along-axis pressure gradients may not be able to build up to the degree implied by the
example. I think this warrants some discussion, most likely in Section 4.2.

SPECIFIC COMMENTS (MINOR)

Line 3: It should be made clear that the skilled reproduction of observations of all these parameters are not
shown in this study, but come from previous work.

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Line 7: "climatology" is a little confusing - suggest rephrasing to "predominant wind pattern" or simply "Alongaxis
winds steered..".

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Line 30: "Basin scale" here should be replaced by "dynamical width" or similar.

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Line 92-94: Please provide a reference for the estuarine circulation/exchange.

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139: "partial steps at the bottom boundary" - the meaning of this is not clear to me. Please clarify.

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Line 146: In Section 2.2, the model is described, including the configuration of the biogeochemical parameters
(silica, plankton species etc). Most of these are never mentioned again. If they have little influence on the
results, the authors may want to mention that here. If not, the authors should at least mention in the discussion
section how the biogeochemical components of the model might play into the results presented here (does
biological consumption end nitrate spikes, for example?).

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Line 181: Please indicate (roughly) the timing of the freshet.

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Line 260: "Provides significant physical driver" - this statement should be qualified to be less strong (perhaps
include a "potentially" or similar?). Also rephrase (" *a* significant driver?).

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Line 263: Please explain why these particular locations were chosen. For example - why

the Texada spot and
not a spot across from Central VI?

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Line 272: "Averaged over the SoG region": Please be a little more specific about what area winds were averaged
over.

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Line 297: I find the use of "low-frequency" here unclear - especially since it seems to include the diurnal band.
Please clarify.

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Line 303: "which represent" should be qualified (e.g. "which we interpret to represent").

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Line 306/328: Temperature mode III also seems to have a strong N-S structure, but it is consistently referred
to as a cross-axis mode. Please clarify/comment.

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Line 330: "time-averaged" here is confusing - reads as an average across all time points.
Please rephrase.

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Line 334: "small amount of correlation" : confusing, please rephrase.

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Line 341: "Visibly correlate" - I suggest avoiding this terminology if no significant correlation was found in
the quantitative analysis..

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Line 367: Please explain briefly which assumptions have gone into transforming Ri(U) to Ri(tau).

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Line 374 & 376: Surely "the coasts" are always important? Please rephrase.

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394-396: There is an apparent contradiction here - should the conclusion not be the opposite? Please clarify..

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Line 414-415: "If the cross-axis..fluxes": This is not self-evident to me. Please add some explanation or a
reference.

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Figure 1: Please add a scale bar. I would also suggest changing the color of the Texada star marker as it

currently disappears into the background a bit.

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Figure 4: It is difficult to see the wind time series here. I also find that much of what is in the figure caption
belongs in the text proper.

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Figure 4: Please indicate the timing of the snapshots shown in Figure 2.

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Figure 5: Spectra should probably be computed based on the productive seasons only - as for the profiles
above.

TECHNICAL CORRECTIONS

Figure 6 needs a length scale. Could be achieved by using length instead of grid coordinates on the x/y axes
(grid coordinates are not very useful in any case).

Line 239 - 240: I recommend using a standard date format - the Ocean Science convention seems to be "25
July 2007".

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Line 551: "right" -> left?

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Figure 2: It would be useful to include an indication of the predominant wind direction above each 3 plots.
Maybe using some simple arrows arrows or adding direction in text at the top axis title.

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Figure 5: Could the di erence between the colors used for the median profile and IQ range be made a little
stronger in a and d? Currently a little difficult to see the median profiles.

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Figures 5, 8: Please add units to PSD y-scales.

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Figure 6 needs a length scale. Could be achieved by using length instead of grid coordinates on the x/y axes
(grid coordinates are not very useful in any case).

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Figures 7, right: The sharp red color makes it difficult to discern the other time series. Please reconsider the
color and/or opacity of these lines.

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Figures 8 (bottom): I suggest changing color of the horizontal line in case of difficulties for colorblind readers.

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Title and elsewhere: Should "wind driven" be hyphenated ("wind-driven") since it is a compound adjective?