Ocean Science Discussions

Ocean Sci. Discuss., author comment AC1
https://doi.org/10.5194/os-2021-21-AC1, 2021

Reply on RC1

Ben Moore-Maley and Susan E. Allen

Author 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-AC1, 2021

We are grateful to Reviewer 1 for their thoughtful and detailed comments on this manuscript, and we have included responses below on a comment-by-comment basis. Our overall takeways from these comments are the following: (1) the spectral analysis is noisy and the features of the spectra discussed in the manuscript are difficult for the reader to see, (2) the correlation between the along-axis wind stress and the PC loadings is poorly presented, and (3) the indirect connection between the two surface tracers and the upwelling process weakens the conclusions drawn about upwelling. To address these issues, we propose to (1) apply the multitaper method to our spectral analysis to reduce variance, (2) rewrite our discussion of the correlation between PC loadings and along-axis wind stress to improve clarity and include significance testing and spectral coherence, and (3) use the improved comparison between the PC loadings and the wind stress record to build a stronger connection between surface nitrate and upwelling. We are confident that these revisions along with the proposed changes below will satisfy the concerns raised by Reviewer 1 and improve the overall quality of the manuscript.

Major Comments:

Major Comment 1 (*Strength of surface tracers as proxies for upwelling rather than vertical density structure variables*):

We agree that an analysis of the density structure is ultimately needed to better understand the mechanism of upwelling in the system. However the correlation between PC loading and wind stress for nitrate PCA modes 1 and 3 is statistically significant, and we find this correlation to be compelling and sufficient evidence that wind-driven upwelling is producing these coastal surface nitrate anomalies even though the vertical density structure remains unexplored. Since this study focuses on the influence of upwelling on surface nutrients, we believe that nitrate is the most relevant variable to present. We include temperature to demonstrate how the different surface processes between the two tracers manifest in their upwelling responses to wind, but the focus is nitrate. To better support our claim that coastal nitrate anomalies are upwelling-sourced, we will make the following revisions to the correlation analysis between the PC loadings and the wind stress record: (1) more clearly describe the direction, or sign, of the wind stress record being included in the correlation calculation, (2) quantify the significance of the correlations using a p-value test or equivalent, and (3) present the spectral coherence between the PC loadings and the wind record to show the dominant frequency bands where wind influence is strong. Finally, with respect to the original request for analysis of the density fields, we will be submitting a more physics-oriented article in the coming year that diagnoses the density structure more explicitly.

Major Comment 2 (*Noisy spectra, consider block averaging*):

We will use the multi-taper method to reduce the variance of the spectra without compromising low-frequency resolution, and we will report the 95% confidence intervals across the tapers.

Major Comment 3 (*Poor presentation of wind stress correlation, consider bandpass filtering*):

We will explore the prospect of bandpass filtering to remove high frequency data from our correlation analysis, and we will clarify our reporting of the correlation between wind stress and different modes to better separate the positive wind stress cases and the negative wind stress cases. We will also move the temperature modes II and III to the supplement to reduce clutter. In preparing this response, we have also found that the spectral coherence between wind speed/stress and the nitrate modes (especially mode 3 along the eastern shore) is consistently strong in the fortnightly band. We will include these coherence plots to further support the relationship between wind and nitrate.

Major Comment 4 (*Difficulty seeing relationship between PC loadings and wind in Figure 7*):

This figure is overly burdened by the presence of temperature modes II and III. We will move these modes to the supplement and minimize their discussion in the main text. This action will help declutter this figure, but we will also clarify the text with respect to the nitrate modes.

Major Comment 5 (*Wind time scales poorly defined*):

We will quantify the dominant wind time scales from the ECCC and HRDPS records and compare to recent atmospheric studies (primarily Bakri et al. 2017, Int. J. Climatol. and Thompson et al. 2020, Int. J. Climatol.).

Major Comment 6 (*Significance of open end channels in 2-layer model*):

We will add more emphasis to this aspect of the basin geometry as a caveat to the hypothetical along-axis pycnocline tilt. We elaborate below in our response to the comment at line 414-415.

Minor comments:

Line 3 (*Attribute model tuning to previous works*):

Will clarify.

Line 7 (*"climatology" is confusing - rephrase to "predominant wind pattern" or "alongaxis winds steered"*):

Will rephrase.

Line 30 (*"Basin scale" here should be replaced by "dynamical width" or similar*):

Will rephrase.

Line 92-94 (*Please provide a reference for the estuarine circulation/exchange*):

Will expand Pawlowicz et al. 2007 reference to include this statement.

Line 139 (*"partial steps at the bottom boundary" - the meaning of this is not clear to me*):

The "partial steps" aspect of the model is not directly relevant to this study, so we will remove the statement.

Line 146 (*How are the auxiliary biogeochemical parameters like silica, plankton species, etc. relevant?*):

Biological consumption indeed controls the residence time of nitrate in the surface, and the undiscussed tracers are important for resolving this sink accurately. This biological sink is fundamental to our choice of nitrate as a tracer. We will improve the emphasis and clarity of this point.

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

We will add a freshet timing definition to Section 2.1.

Line 260 (*"Provides significant physical driver" - this statement should be qualified to be less strong*):

Will rephrase.

Line 263 (*Please explain why these particular locations were chosen*):

Since the upwelling response is coherent along most of the coastline, the choice of these locations is somewhat arbitrary as they are intended to simply orient the reader to the upwelling behavior of the system, before continuing on to the more quantitative PCA. To make these choices less arbitrary, we will use spatial averages over 4 regions instead of 4 isolated locations.

Line 272 (*"Averaged over the SoG region": Please be a little more specific*):

We will include polygons on the map (Figure 1) to show the spatial averaging regions for wind, nitrate and temperature.

Line 297 (*I find the use of "low-frequency" here unclear - especially since it seems to include the diurnal band*):

Will rephrase as "sub-tidal"

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

Will rephrase.

Line 306/328 (*Temperature mode III seems to have a strong N-S structure, but it is referred to as cross-axis*):

We will move temperature modes II and III to the supplement and trim the presentation in the main text to remove clutter and confusion.

Line 330 (*"time-averaged" here is confusing - reads as an average across all time points*):

This wind stress metric is central to our analysis of the EOF results, so we will define it more clearly in a separate sentence.

Line 334 (*"small amount of correlation" is confusing, please rephrase*):

We will define this correlation more clearly as the "correlation to the reverse wind stress direction" in a separate sentence and in our overall discussion of Figure 8.

Line 341 (*"Visibly correlate" - avoid if no significant correlation was found in the quantitative analysis*):

We will rephrase all discussion of correlation in quantitative terms.

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

The primary assumption in this transformation is that the bulk vertical shear stress is described by the surface layer friction velocity squared (u*^2) which is equal to the kinematic wind stress (tau/rho). This assumption is common in simplified 2-layer models of lakes (e.g. Spiegel and Imberger 1986 J. Fluid Mech.). Regardless, we use this quantity as a scale parameter only and not an indicator of turbulence. Nevertheless, we will add a clarifying statement describing the origin of our bulk Richardson number and our specific use case.

Line 374 & 376 (*Surely "the coasts" are always important?*):

We will rephrase this statement to clarify that we are talking about timescales before the coastal pressure gradient forces have become established.

Line 394-396 (*There is an apparent contradiction here*):

This is a typo. Zeta_{end} does not contribute significantly to upwelling on typical wind time scales.

Line 414-415 (*"If the cross-axis ... fluxes": This is not self-evident to me*):

We are describing the behavior of the infinite coast scenarios of the two layer model at the corners of the rectangular basin at (0, 0) and (L_C, L_A). An analytical solution is not easily found, but qualitatively, the upwind lower layer transports along the basin sides combined with the cross-wind lower layer transports at the basin ends result in upwelling at the upwind corner (0, 0) and downwelling at the downwind corner (L_C, L_A). These corner signals will grow in time, and propagate anticlockwise along the basin end walls, setting up cross-axis pressure gradient forces at the basin ends that directly oppose the cross-shore Ekman fluxes. The flow near the ends that was cross-shore will respond to the new force balance by becoming more along-axis, eventually leading to an along-axis, lake-like pycnocline tilt. The open channels at the end of the basin mentioned by Reviewer 1 in Major Issue number 5 are a major caveat to this setup. We will rewrite the opening sentence of this paragraph to more clearly describe this scenario. We also propose adding a diagram of this qualitative solution to aid the reader.

Figure 1 (*Please add a scale bar and improve contrast of the Texada star marker*):

Will add/modify.

Figure 4 (*Difficult to see the wind time series. Much of what is in the figure caption belongs in the text*):

We will add a zoomed panel to emphasize the period between the cutoffs.

Figure 4 (*Please indicate the timing of the snapshots shown in Figure 2*):

Will add.

Figure 5 (*Spectra should be computed based on the productive seasons - as for the profiles*):

We will make this change in addition to using variance-reduction methods.

Technical corrections:

Figure 6 (*Needs a length scale*):

Will modify.

Line 239 - 240 (*I recommend using a standard date format*):

Will modify.

Line 551 (*"right" -> left?*):

This is a typo, will correct.

Figure 2 (*Include the predominant wind direction*):

Will add.

Figure 5 (*Improve contrast between median profile and IQ range*):

Will enhance.

Figures 5, 8 (*Please add units to PSD y-scales*):

Will add.

Figures 7, right (*The sharp red color makes it difficult to discern the other time series*):

Since temperature modes II and III account for the least variance and are not central to our findings, we will exclude them from all figures.

Figures 8 (bottom) (*Change color of the horizontal line in case of difficulties for colorblind readers*):

Will modify.

Title and elsewhere (*Hyphenate "wind-driven" since it is a compound adjective*):

Will change throughout.