(line numbers refer to the marked-up manuscript)

Response to Anonymous Reviewer 1

In my opinion, this version of the manuscript constitutes a significant improvement upon the initially submitted version. In the revised version of the manuscript, the authors have refined their methods and improved their description of study and its results, and the presentation of the material has become clearer as a result. The new sections at L293 and S3.2 are very clarifying in terms of how they conduct the study and interpret their results – it is now clearly laid out how the authors arrive at physical interpretations of the EOF modes, and uncertainties/limitations in these interpretations are thoroughly laid out. Throughout, the writing is clear and concise, and the findings are evaluated sceptically. The updated graphics are nice, and the added supporting figures for the analytical model are useful.

I maintain that for studying this kind of process, the ideal order would be to first study the "dynamical" variables (density) and then look at resulting effects on tracers - but I understand the authors' decision and accept their approach.

My initial concerns have largely been addressed. I would like to congratulate the authors on an excellent manuscript, which I recommend be published. A small number of minor suggestions are attached below.

We are grateful to Reviewer 1 for their thoughtful follow-up comments on this manuscript, and we are pleased to hear that our last round of revisions satisfied the reviewer's initial concerns. We have addressed the remaining outstanding issues that Reviewer 1 has identified and included our responses below.

Minor comments

1. *Section 3.2: Recommend reiterating here that PCA analysis is only done for productive seasons.*

   We have added a statement clarifying that the productive season is used in the first sentence of Section 3.2 at line 351.

2. *Fig 8: Please indicate confidence intervals on spectra. Related: please clarify what is meant by "significant" at L461.*

   We would like to acknowledge this comment as it forced us to think a bit more about how we interpret the PC spectra. We concluded that meteorologically-forced signals in the context of our study follow stochastic, autoregressive "red noise" spectra due to persistence in atmospheric patterns. We then define significance as a peak which deviates from this stochastic process beyond some confidence interval. Solar and tidally-driven processes will have significant peaks at certain frequencies, but the absence of significance is actually evidence for wind-driven upwelling and mixing (although the 17d peak in nitrate mode 3 is significant). We determined the confidence interval for red noise by fitting an AR1 process to the 4 PC modes presented in the main text, and then finding the 99th percentile of 1000 random simulations. We have added this curve to Fig. 8 and have made text modifications throughout the manuscript at lines 231-235, 275-277, 376-379 and 424-431 to communicate this concept. We also introduced a supplement with companion figures to Fig. 6 and 8a (S2, S3) that include the first five modes for each tracer.

3. *Fig 8: For easy reference to the text at page 21, it would be useful to add a second x-axis with cycle periods (days), or at least label the lines in figure 8a with periods.*

   We labelled the lines in Fig. 8a with periods to avoid over-cluttering the figure.

4. *Fig 10: Recommend clarifying "infinite channel" – it took me some time to figure out the meaning of the title in 10b. Maybe specify "infinite in the x-direction" or similar?*

   We added sentences at lines 472-473 to introduce the infinite channels leading up to the next section at line 476. These sentences clarify that the infinite channels have zero gradients parallel to the channel walls.

5. *L108/148: Spell out or define BC for readers not familiar with this area.*

   We removed "BC" at line 112 and added "British Columbia" at line 167.

6. *L2/L19: hyphen?*

   We hyphenated "wind-driven" at lines 2, 19 and throughout the manuscript.

7. *L667: Please refer to figure 6f.*

   We have referenced Fig. 6f at line 537.

8. *Figure 4: Recommend making sure VI/SC points are discernible in B/W (different symbols?).*

   We changed the Sunshine Coast symbols to stars in Fig. 4.

Response to Reviewer 2, Jennifer Jackson

The authors have done a meticulous job of addressing my concerns from the first round of revisions. The manuscript is now much clearer and easier to follow. I recommend the manuscript for publication. Below are a few minor suggestions.

We are grateful to Dr. Jackson for her thoughtful follow-up comments on this manuscript, and we are pleased to hear that our last round of revisions satisfied her initial concerns. We have addressed the remaining outstanding issues that Dr. Jackson has identified and included our responses below.

Minor comments

1. *Evans et al., 2018 is cited but is not in the references*

   We have made sure that Evans et al., 2018 and all other citations are now included in the references.

2. *Lines 245 to 247 – I think that references are needed here*

   We modified this text to present our basis for mode attribution in steps so that we could link back to the study area background presented in Section 2.1 and then eliminate the river and biological processes before presenting our final mode attribution candidates. We added references to Fleming and Clark 2005, Pawlowicz et al., 2019 and Halverson and Pawlowicz 2016 to support our disregard of the rivers and Del Bel Belluz et al, 2021 to support our disregard of the biology. The new text is at lines 249-263.

3. *Lines 249 to 251 – I disagree with this statement – while try for rain or snow-fed rivers, glacial-fed rivers such as the Homalco and Fraser often peak in the summer.*

   We modified this statement at lines 252-254 to acknowledge that glacial rivers are indeed at high flow stage during the summer and to clarify that we do not consider the rivers as drivers of surface tracer variability because the time scales of this variability are longer than the synoptic time scales of interest.

4. *Figure 5 – Please clarify that the output shown in b to k are the average of the gridpoints in Figure 4a? Also, what is the variability of this average? It is unlikely that you can put a measure of variability on these already complicated figures but if you are calculating an average over all of these gridpoints then I think it is important to somehow show variability.*

   The lines in Fig. 5 are the spatial medians across the magenta region and blue/gold gridpoints shown in Fig. 4a. We have now clarified this detail in the Fig. 5 caption. We added a companion figure to Fig. 5 in the supplement that includes the interquartile range (IQR) as an envelope around each time series (Fig. S1) and added a sentence at lines 348-349 that links the IQR to the along-shore variability that we observe in the surface tracer images in Fig. 2.

5. *Line 347 – I am still unclear how Figure 6c is indicative of wind mixing and diurnal heating and cooling.*

   We modified the text at lines 364-366 clarifying that the spatial uniformity of the temperature anomaly in the interior SoG is our basis for suspecting wind mixing or diurnal heating/cooling, based on mode attribution criteria (1).

6. *Lines 347 to 348 – The cross-axis temperature gradient is very small and I don't see how upwelling can be inferred from Figure 6c.*

   We have removed our mention of the cross-axis temperature gradient at line 367 since it is not central to our results.