Responses to Reviewer 1 (original are in black and our responses are in blue)

Since about 10 years MHW have been recognized at regional or global scale. This is a “hot” (or “warm”) topic as this could impact ocean biogeochemistry, marine ecosystems and air-sea CO2 fluxes. For the oceanic carbon cycle, this has been recognized at regional scales, in the North Atlantic (e.g. Chau et al, 2024a; Müller et al, 2025), the subarctic Pacific Ocean (Bif et al, 2025), the Mozambique Channel (Metzl et al, 2025), or at global scale (Mignot et al, 2022; Ford, et al 2025). Here, the authors explore the impact of MHW in the North Pacific during the so-called Blob event in 2015-2017. In this region, the impact of MHW on CO2 flux has been studied (Mignot et al, 2022; Ford, et al 2025) but results presented different scenario: does the MHW lead to an increasing or decreasing of the ocean CO2 sink in the North Pacific. What are the drivers of the observed changes?

Using a model, authors showed an intriguing result: the higher SST during the Blob cannot account for the changes of pCO2, not only at station P but also at regional scale in the Gulf of Alaska. I guess previous work analyzed the pCO2 change only, not the changes in DIC and associated processes. Here authors show that the transport is important. For validation/comparison authors used pCO2 from the Seaflux product (Fay et al 2021). This includes the CMEMS-FFNN product (identified in Figure 3). This product also offers results of the carbonate system properties (TALK, DIC). It would be useful to compare the DIC distribution from the model with the DIC derived from CMEMS-FFNN (Chau et al, 2024b).

We appreciate the time and effort that the reviewer dedicated to evaluating our work and to give insightful comments. In accordance with the reviewer’s suggestions, we performed additional analyses for the validation of DIC and TALK fields using the CMEMS-FFNN product (see Figs. R2 and R3 below).

The introduction is very clear as well as the description of the model, although one has to read Li et al (2025 unpublished) to get a view of the validation for the physics, nutrients and NPP. Here authors add a validation results for pCO2 (Figure 2 and 3). Figures are adapted but I would suggest add a map of the SST anomaly during the Blob for readers not familiar with this region/event. It would have been interesting to inform the regional changes in term of integrated CO2 fluxes (e.g. in TgC/yr) and it would be useful to add a table with the results of annual CO2 flux integrated over the investigated region (45-60N/155-125W) from the model and SeaFlux, before, during, and after the Blob event.

In accordance with the reviewer’s comment, we performed additional analyses of the modeled and observed SST anomaly during the Blob (Fig. R1). Regarding the integrated CO_2 fluxes, we plan to address this topic in the Discussion section and in the supplementary figures (Figs. R4 and R5).

As expected, relatively lower surface ocean pCO_2 causes sustained uptake of atmospheric carbon dioxide even under the Blob (Table R1). The simulated air-sea carbon flux is relatively moderate in magnitude relative to the SeaFlux ensemble mean as shown in Fig. R5 with the exception of JENA-MLS.

Table R1: CO_2 fluxes integrated over the central GOA. The period is 2010–2013 as “Before,” 2014–2015 as “Blob,” and 2017 as “After.”

Tg/yr	2010–2017	Before	Blob	After
SeaFlux	-14.1	-10.9	-19.0	-14.8
Model	-10.5	-8.2	-13.4	-12.9

C-01: The Abstract is somehow long.

We thank the reviewer for this pertinent comment. We will make it more concise when we revise the manuscript.

C-02: Line 31: Friedlingstein et al., (2022), not in reference

We appreciate the reviewers pointing out the missing reference. We will include his paper (https://doi.org/10.5194/essd-14-4811-2022) in the references section.

C-03: Line 36: “In particular, the persistent MHW that occurred in the subarctic NE Pacific from the winter of 2013 to 2015, known as the Blob…”. Here maybe add a map of the SST anomaly for readers not familiar with this region and the Blob event.

We will add maps of the SST anomaly, showing the model’s ability to represent the overall magnitude of warming.

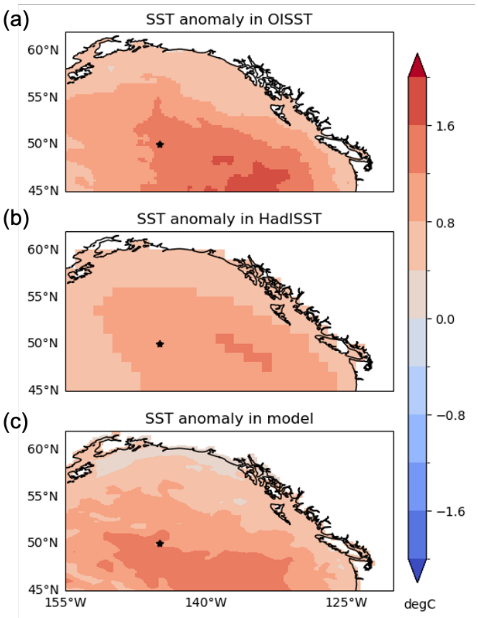

Figure R1: Annual SST anomaly during Blob (2014–2015) relative to 2010–2017 from (a) OISST, (b) HadISST, and (c) the model. The star indicates the location of OSP.

C-04: Line 82: “The model has been validated against a suite of physical and biogeochemical observations (Ito et al., 2025)”. Ito et al 2025, is apparently not yet published. “Biogeochemical observations” is somehow general. These authors compared the model with physical, nutrients (climatology not in-situ time-series data) or NPP, but not pCO2 or carbonate system data such as DIC or TALK (e.g. from GLODAP). It would be useful to specify that the validation concerned only surface waters, i.e. no water column observations used for validation although the results investigate the DIC changes in the water column as presented in Figure 6.

Ito et al. (2026) has been published (12 Feb 2026, **https://doi.org/10.1029/2025JC022996**). We will change the citation.

As suggested, we have compared our model outputs and CMEMS-FFNN product and in-situ observations for DIC and TAlk (Figs. R2 and R3). In the spatial patterns of the 2010–2017 mean, both DIC and TAlk are higher in the model than in CMEMS_FFNN, particularly in the northwestern part of the model domain. However, the differences are relatively small in the central Gulf of Alaska near OSP, and this feature is consistent with other variables such as nutrients and oceanic pCO_2.

At OSP, the model shows higher DIC concentration than CMEMS_FFNN (+11.4 micro mol/kg during the Blob), particularly during autumn when CMEMS_FFNN indicates relatively low DIC. Nevertheless, the temporal variability agrees well between the two, although the model shows a smaller seasonal amplitude. For TAlk, the model generally shows higher concentrations than CMEMS_FFNN (+7.55 micro mol/kg during the Blob) except in 2015. Compared to the in-situ observations, neither the model nor CMEMS_FFNN shows a statistically significant difference in TAlk. For both variables, the model reproduces the decrease in concentrations during the Blob period seen in CMEMS_FFNN, indicating consistent changes before, during and after the Blob between the model and CMEMS_FFNN. To evaluate the model fidelity for DIC and Talk in more detail, additional observational data would be necessary.

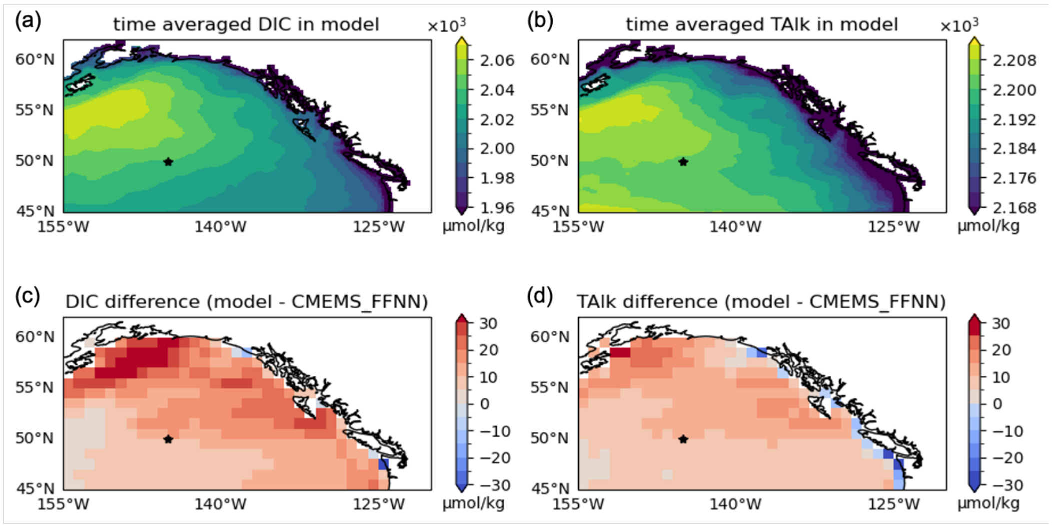

Figure R2: Comparison of surface distributions of (a, c) DIC and (b, d) TAlk, averaged over 2010–2017, between the model and CMEMS_FFNN, one of the SeaFlux products. The star in each panel indicates the location of OSP.

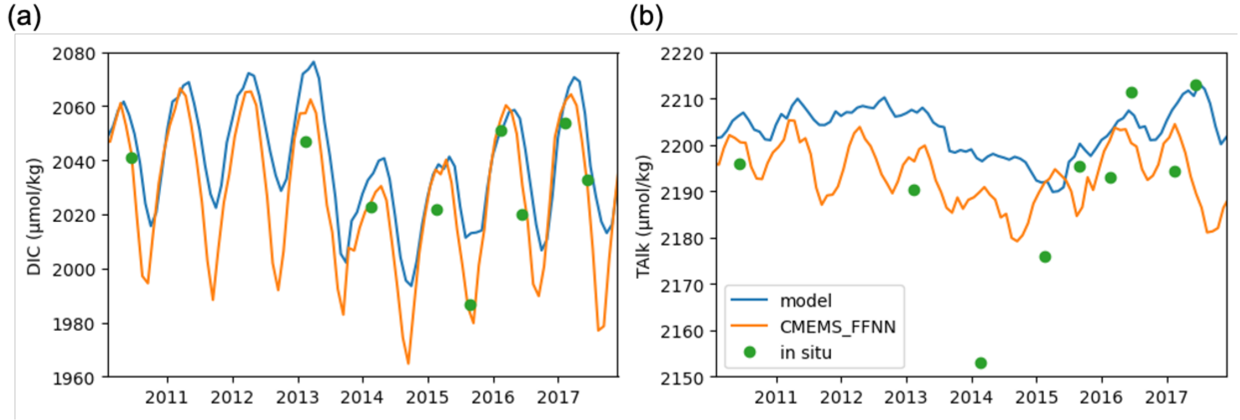

Figure R3: Comparison of the time series of surface (a) DIC and (b) TAlk at OSP between the model and CMEMS_FFNN, one of the SeaFlux datasets. Green dots in both panels represent in situ observations (Franco et al. 2021) averaged over the upper 10 m depth.

C-05: Line 83: “The simulated temporal variability of oceanic pCO_2 is validated with the NOAA (Pacific Marine Environmental Laboratory’s Ocean Climate Stations and Carbon groups) mooring at OSP (Emerson et al., 2011; Cronin et al., 2015).” Why selecting only this data-set for validation? Would be interesting to use other data available in this region (e.g. from SOCAT, Bakker et al, 2016) or mention that there is no data for the investigated period to validate your simulation.

The NOAA mooring data is a long-term in-situ observation covering the simulation period and therefore serve as the primary reference for validation. SOCAT data were examined but the temporal coverage at OSP is sparse with only seven valid data points available during the study period, which limits their use for time-series validation. Both SOCAT and mooring data provide pCO_2 but not DIC and Alkalinity. Shipboard measurements of DIC and Alkalinity are also limited (Fig. R3).

C-06: Line 92: “The regional ocean circulation and biogeochemistry model used in this study follows the configuration described in Ito et al. (2025), thus only a brief description is provided here, while full details can be found in their paper.” This is correct, but the reference is not yet published.

Ito et al. (2026) has been published (12 Feb 2026, **https://doi.org/10.1029/2025JC022996**) and will be updated in the revision.

C-07: Line 162: Before showing the results for pCO2 I think a plot of SST anomalies during the Blob event should presented (for readers not familiar with this region and MHW, see comment C-03). Although this is presented in Figure 3 for OSP times-series, a regional map of SST anomalies would be useful to show the extent of the Blob and thus how this would impact the CO2 flux integrated over the full region (XXX TgC/yr).

Agreed. We will add maps of SST anomalies (see Fig. R1).

We additionally plotted CO2 fluxes in the model and from the SeaFlux. The fluxes were integrated over the central GOA (Figs. R4 and R5). Since CO_2 flux is nonlinearly influenced by the wind speed, temperature, and $\Delta pCO2$, it is difficult to isolate the effect of temperature changes alone. Therefore, CO_2 flux has not been included in the main framework of the paper but this point will be raised in the Discussion section and supplementary figures.

The spatial distribution of the CO_2 flux shows enhanced outgassing (ocean to atmosphere) in the northwestern part of the model domain during the Blob period relative to the SeaFlux product, driven by the elevated oceanic pCO_2 in this region (Figs. 1 and 8 in the original manuscript). This positive bias in the northwest is also evident in other variables such as nutrients.

In the area-integrated CO_2 flux time series for the central GOA (black box in Fig. R4), the model exhibits weaker fluxes than the SeaFlux ensemble mean (−13.4 Tg/yr in the model, −19.0 Tg/yr in the SeaFlux ensemble mean during the Blob, Table R1). Given that both products use JRA55 winds and that differences in CO_2 solubility are negligible, the discrepancy likely arises from differences in ΔpCO_2 and/or the gas transfer coefficient. Notably, the model ΔpCO_2 is smaller than the SeaFlux ensemble mean, particularly in 2015. However, both the model and SeaFlux ensemble show overall good agreement with the mooring observations (Fig. 3 in the original manuscript, and the relative agreement varies by year. Therefore, the weaker CO_2 flux in the model may not necessarily indicate reduced model fidelity.

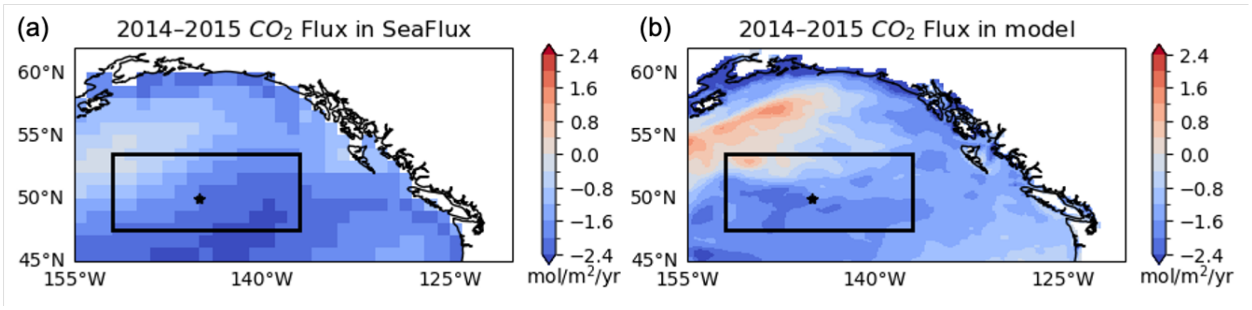

Figure R4: Comparison of the spatial distributions of annual CO_2 Fluxes averaged over 2014–2015 for (a) the ensemble mean of the SeaFlux products and (b) the model. Negative values indicate oceanic CO_2 uptake. The star in each panel marks the location of OSP. The black box (47.5–53.5°N, 208–223°E) in (b) indicates the central GOA area.

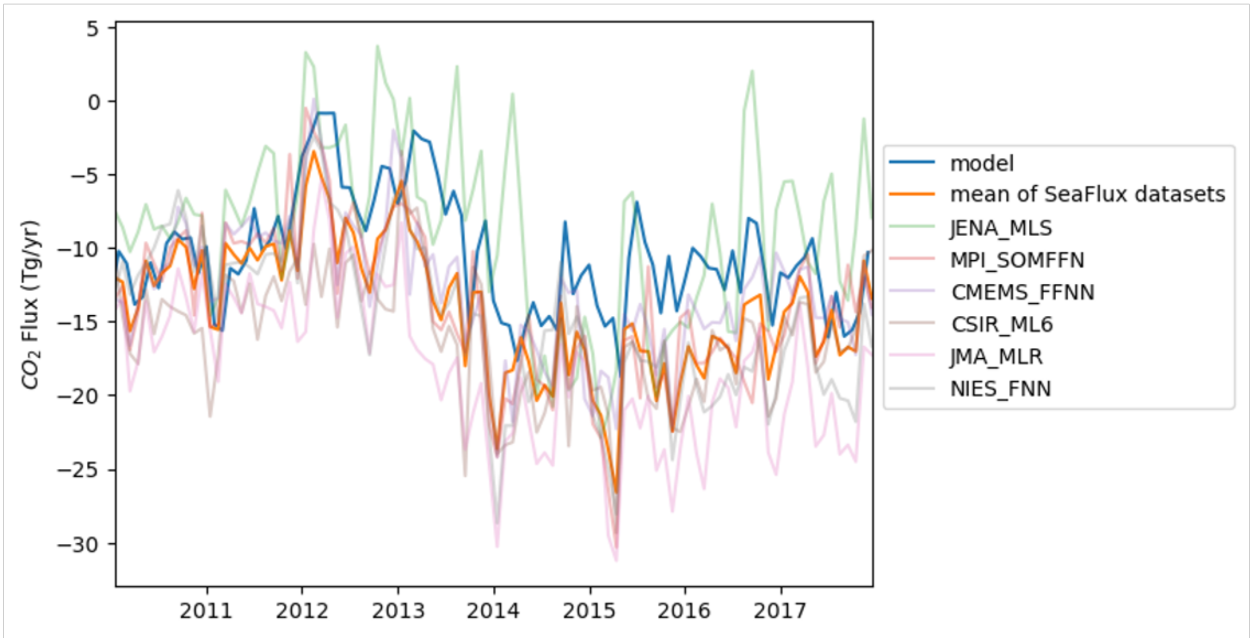

Figure R5: Time series of integrated CO_2 Flux over the central GOA. The SeaFlux ensemble mean is shown in orange, with individual ensembles indicated by thin lines with different colors, while the model is shown as a blue line.

C-08: Line 163: "…indicating that the GOA is on average a sink of atmospheric CO2 (Fig. 1a)." Maybe specify the region is a sink for all seasons?

Figure 3 (a) shows this region does not always take up the atmospheric CO_2 in the GOA, but it is a region of net sink for atmospheric CO_2 in the climatological sense. For clarity, we will re-state that "indicating that the entire region acts as a sink for atmospheric CO_2 in the climatological mean field"

C-09: Line 172: Figure 1 caption: could you specify more clearly each panel (a,b,c,d).

We will change the caption of Figure 1 as "ΔpCO_2 (oceanic pCO_2 minus atmospheric pCO_2): (a) mean over 2010–2017, (b) mean during the Blob period, (c) difference between (b) and (a), and (d) the component driven by SST changes during the Blob in SeaFlux. Blue indicates CO_2 uptake, and red indicates CO_2 outgassing. The star in each panel marks the location of OSP. The SST-driven oceanic pCO_2 driven is calculated as the difference between pCO_2 computed under climatological mean conditions (2010–2017 mean SST and SSS from SODA version 3.4.2, and DIC and alkalinity from GLODAPv2) and pCO_2 computed using SST from 2014–2015, while keeping the other variables fixed at their climatological values, with PyCO2SYS."

C-10: Figure 1d: Not sure to understand Figure 1d: Authors use GLODAP to calculate pCO2 for the period 2014-2015. Are the DIC ALK data from GLODAP correspond to this period or did you correct DIC for this reference year (taking into account the anthropogenic signal). Please clarify.

For clarity, we will change the figure description to "The SST-driven oceanic pCO_2 is calculated as the difference between pCO_2 computed using SST from 2014–2015 and with the climatological mean conditions (2010–2017) and keeping all other variables (salinity, DIC and alkalinity) constant to climatology. SST and SSS are taken from SODA version 3.4.2. For this calculation, a climatological DIC and alkalinity are assumed (taken from GLODAPv2). The values for pCO_2 are computed using PyCO2SYS" in the figure caption.

C-11: Figure 1: As pCO2 (and thus DpCO2) present large seasonality, would it be better to show the maps of annual CO2 flux instead the mean DpCO2?

While pCO_2 (and thus ΔpCO_2) and air-sea CO2 fluxes exhibit strong variability on seasonal to interannual timescale, we chose to show the mean ΔpCO_2 to more directly isolate the impact of SST-driven changes. Air-sea CO_2 flux is nonlinearly controlled by multiple factors, including wind speed, temperature, and ΔpCO_2, making it difficult to disentangle the effect of temperature alone when using flux.

Although seasonal variability is large, the mean ΔpCO_2 still captures the integrated effect of these seasonal changes on the air-sea gradient. Using annual CO_2 flux would incorporate variability from wind speed and gas transfer velocity, which may obscure the specific processes we aim to highlight here.

C-12: Line 196: Authors write: "During the Blub, both the mooring observations and model show pronounced decline in oceanic pCO_2." Typo: Blub or Blob?

We appreciate careful review and it was a typo. We will correct it to Blob.

C-13: Line 213: Authors write: "Observations show that oceanic pCO_2 increases due to SST changes during the Blob by about +20 µatm…". Please specify which observations? OSP time-series? Other observations?

It is for the OSP time series. For clarity, we have added an explanation in the manuscript as "The mooring observations show that oceanic pCO_2 increases due to SST changes during the Blob by about +20 µatm, …"

C-14: Line 216: Authors write:"… the net oceanic pCO_2 change of around −10 µatm is primarily driven by the DIC, explaining the oceanic pCO2 decreases during the Blob." It would be useful to show the time-series of DIC result (e.g. to add in Figure 3). Is the model result coherent with other data-products such as CMEMS (Chau et al, 2024b).

We have added the DIC time series as Fig. S2 (Fig. R2) including CMEMS_FFNN and in situ data from Franco et al. (2021). Both show a DIC decrease during the Blob based on in situ observations. The model reproduces the observed decrease in DIC during the Blob.

C-15 Line 310: Betten et al., (2022). Check name for reference Batten et al., (2022).

We appreciate careful review. It was a typo. We will correct it.

C-16: In Supp. Mat.: “Figure S1: Climatology of DIC from the model. The star indicates the location of OSP”. Please, specify what is “climatology”: average for all seasons and for a period 2000-2017, or for a specific year?

DIC climatology was calculated as the average between 2010–2017. We have changed the Figure S1 and rewrote the caption as “Figure S1: Comparison of surface distributions of (a, c) DIC and (b, d) TAlk, averaged over 2010–2017, between the model and CMEMS_FFNN, one of the SeaFlux products. The star in each panel indicates the location of OSP.”

C-17: Why one calls this warming event a “Blob”? Acronym for « Bizarre Large Ocean Bubble”?

The term “Blob” is not an acronym (e.g., it does not stand for “Bizarre Large Ocean Bubble”). It is simply a nickname referring to a large, amorphous patch of anomalously warm water in the NE Pacific occurring in 2014 through 2015. The name was coined by Nicholas Bond in the 3 June 2014 newsletter for the Office of the Washington State Climatologist, and used in general.

References

Franco, A. C., Ianson, D., Ross, T., Hamme, R. C., Monahan, A. H., Christian, J. R., et al. 2021. Anthropogenic and climatic contributions to observed carbon system trends in the northeast Pacific. Global Biogeochemical Cycles, 35, e2020GB006829. doi: 10.1029/2020GB006829

Ito, T., Timmerman, H. V. A., Bjorklund, A., Stanley, S. I., Abe, Y., Reinhard, C. T., and Montoya, J.: Eddy-induced iron transport sustains the biological productivity in the Gulf of Alaska. 2026. JGR Oceans, Volume 131, Issue 2, doi: 10.1029/2025JC022996.

Responses to Reviewer 2 (original are in black and our responses are in blue)

In this manuscript, the authors explore the impacts of a regional MHW (in the Northeast Pacific) on CO_2 using a novel regional model configured for the Northeast Pacific that is validated, and compared, to observational products. Results indicate that changes in pCO2 are primarily driven by the compensation between increased temperatures (which raises pCO2) and lowered Dissolved Inorganic Carbon (DIC; which lowers pCO2) in the surface ocean. Results suggest that DIC is the dominant driver of changes in pCO2 and is decomposed to demonstrate that changes in DIC stem primarily from changes in transport (both vertical and horizontal). While I find the analysis robust, I believe that the authors could expand their work to strengthen their arguments and contribute to fundamental understandings of the impacts of MHW on regional marine carbon cycling.

First, I think it is important that the authors directly compare the physical manifestations of the Blob across model and observational products to demonstrate that they do show the same spatial and temporal extent. I would be curious to see how SST anomalies during the Blob compare across these data, and if differences in their representation might have any impact on interpretation of results.

We sincerely appreciate the time and effort that the reviewer dedicated to evaluating our work and to give insightful comments.

In accordance with the reviewer's suggestion, we have compared SST anomalies between the model and two observational products (Fig. R1). This figure will be included in the revision if we are invited to revise and resubmit the manuscript. The modeled SST anomalies show good agreement with both OISST and HadISST in terms of their spatial patterns and overall amplitudes. Given this consistency, the model robustly represents the observed SST anomalies, and they are likely not influencing the interpretation of the SST-driven component of ΔpCO_2.

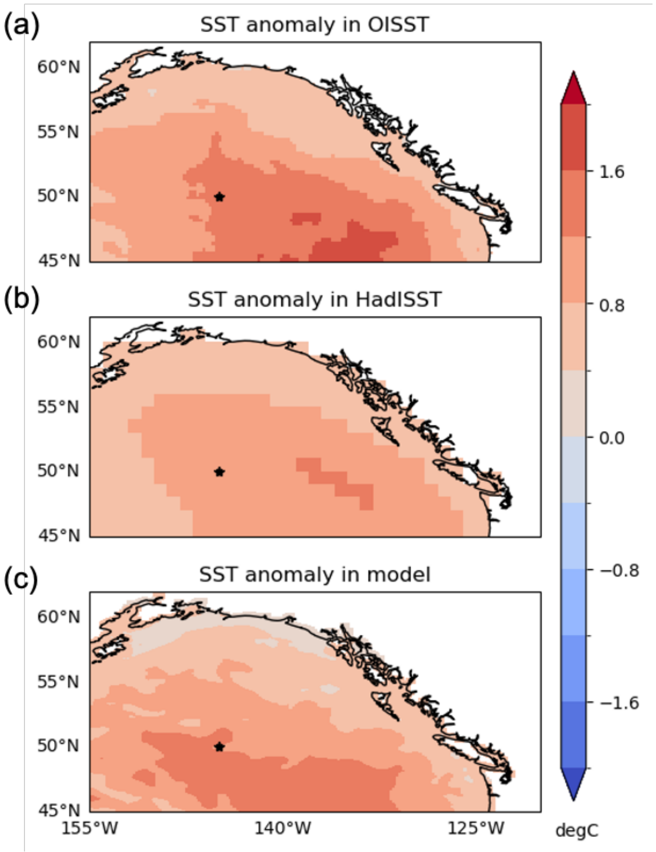

Figure R1. Annual SST anomaly during Blob (2014–2015) relative to 2010–2017 from (a) OISST, (b) HadISST, and (c) the model. The star indicates the location of OSP.

If possible, I also recommend the expansion of this analysis to other manifestations of MHWs in the region (as described by Amaya 2020 and examined by Kohlman 2025) to demonstrate the robustness of these results across discrete warming events. I know that the 2019 MHW is outside of the integration for the model used in this study, but could you examine prior MHW? I am curious to see if the responses of pCO2 is identical or shows discrete differences for different MHW, as demonstrated by Kohlman 2025. This would also connect to the broader implications of this work – can the response demonstrated be considered the expected response in the Northeast Pacific? Or, is the response event dependent.

We agree that evaluating additional MHW events would strengthen the broader implications of this study, however, more recent MHW events are outside the scope of this manuscript. We have added a paragraph discussing this point and clarified that extending the simulation period to include recent MHWs remains an important direction for future work. While recent large events (e.g., 2019, 2023) fall outside the integration period of our model, the 2005 warm condition can be analyzed. (Fig. R2; reproduced from Ito et al. 2026, Supplementary Fig. S1).

The decomposition analysis of pCO_2 changes for 2005 shows a similar response to the Blob. The DIC-driven decrease in pCO_2 is larger than the SST-driven increase, resulting in a net pCO_2 reduction (Fig. R3). This DIC decrease is primarily associated with changes in transport, particularly weakened vertical transport, which is consistent with the mechanism identified for the Blob (Fig. R4).

These findings suggest that the pCO_2 response is not unique to the Blob but may represent a characteristic behavior of the NE Pacific during strong warming events. These results are presented here for completeness in response to the reviewer's comment and are not included in the revised manuscript.

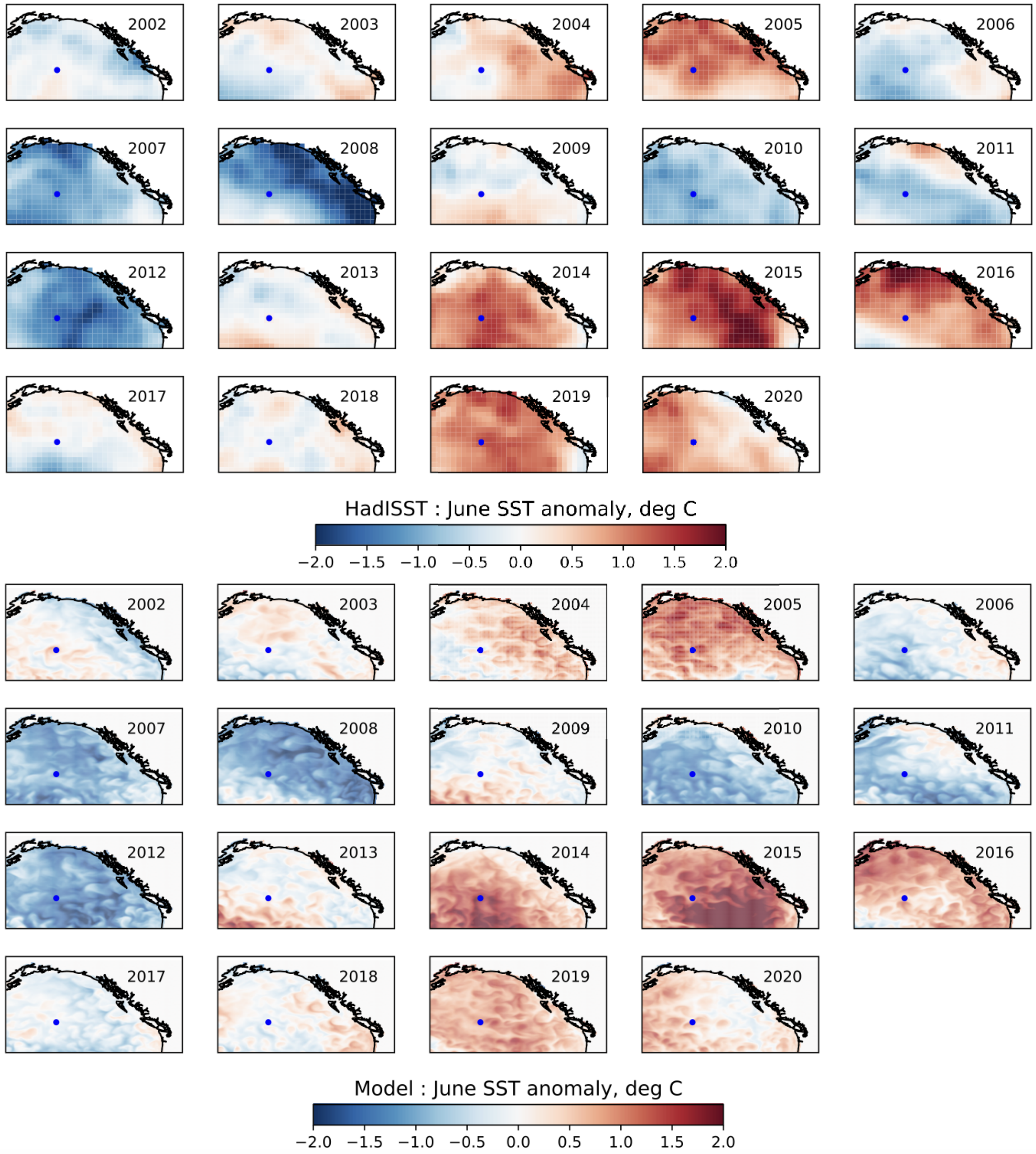

Figure R2. Monthly mean SST anomaly fields from HadISST (top) and the model (bottom) The anomalies are calculated as the deviation from the climatology for 2002–2020. The blue dot indicates the location of OSP (From Ito et al. 2026).

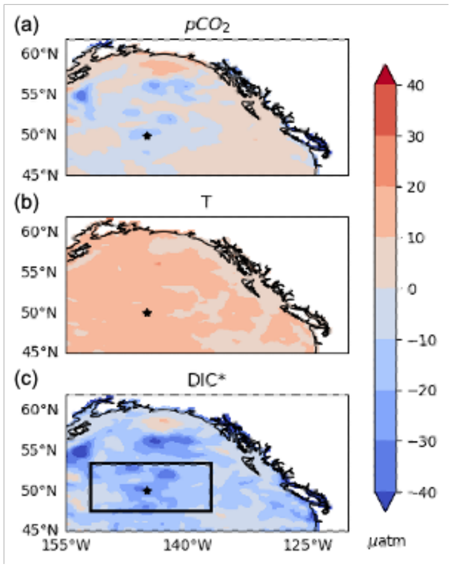

Figure R3. Spatial patterns of (a) oceanic pCO_2 changes and the contributions to oceanic pCO_2 changes from (b) SST, and (c) DIC* in 2005. Blue shows ocean CO_2 uptake, and red shows CO_2 outgassing from the ocean. The star in each panel marks the location of OSP. The black box (47.5–53.5°N, 208–223°E) in panel (c) indicates the central GOA area used for the budget analysis.

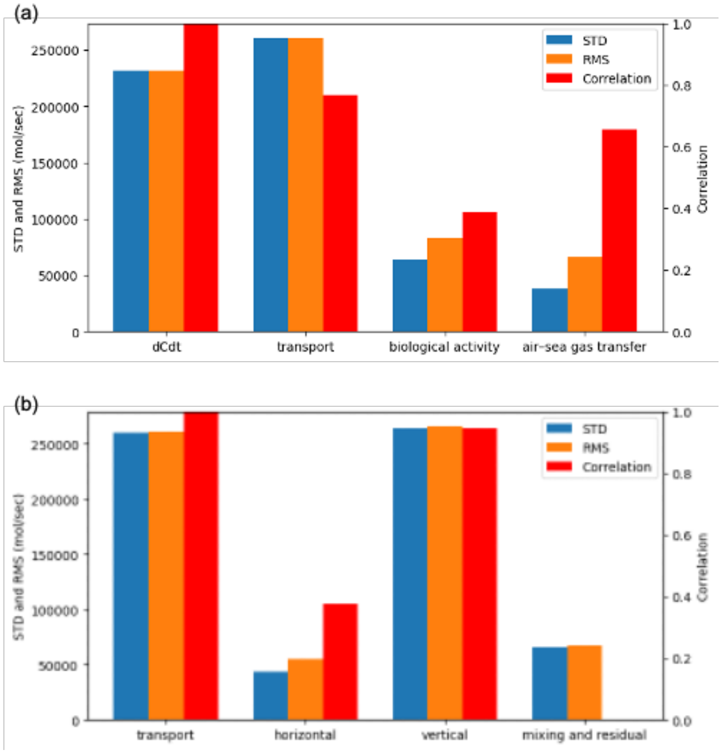

Figure R4. Bar graphs showing the variability of each component of (a) the carbon budget and (b) the transport budget in the central GOA in 2005. Blue bars represent the annual standard deviation (STD) and orange bars show root-mean-square (RMS) of each term. Red bars, plotted the secondary y-axis, indicate the correlation coefficients between dC/dt (panel a) or the transport term (panel b) and each component.

I would also recommend a thorough read through to ensure consistency in the tense used throughout the manuscript.

We appreciate the reviewer's careful reading of the manuscript and helpful suggestions for improving clarity. We will proofread the entire manuscript if we are given the opportunity to revise and resubmit the manuscript.

Aside from the major comments above, I noted a few explicit instances in the text that could be clarified:

Line 17: also 'is' to 'would be'

We will fix this typo.

Line 34-35: I would suggest including an expanded definition of MHWs here to better orient the reader, i.e. "short-term statistical extremes in sea surface temperature, called marine heatwaves"

In accordance with the reviewer's comment, we will expand the definition of marine heatwaves at the relevant location in the manuscript to better orient the reader and improve clarity.

Line 168: 'is at odds against' could be simplified

We will revise this sentence.

References

Ito, T., Timmerman, H. V. A., Bjorklund, A., Stanley, S. I., Abe, Y., Reinhard, C. T., and Montoya, J.: Eddy-induced iron transport sustains the biological productivity in the Gulf of Alaska. JGR Oceans, Volume 131, Issue 2, doi: 10.1029/2025JC022996.

Responses to Reviewer 3 (original are in black and our responses are in blue)

The manuscript is generally well written. The authors present their ideas clearly, making the text easy to read and understand. They address an interesting question and use an appropriate tool i.e., the high-resolution regional MITgcm simulation to investigate the CO_2 response to the 20142015 Blob. That said, the attribution and mechanistic explanation (lines 266–287) of the main findings need to be strengthened. First, the explanation of the respective roles of horizontal and vertical transport is brief and unclear. As this is a key point of the study, it deserves a clearer discussion.

We sincerely appreciate the reviewer's thoughtful and constructive comments, which have helped us strengthen the mechanistic interpretation of our results. In response to the request for clearer attribution of the respective roles of horizontal and vertical transport, we have performed additional analyses to clarify the respective roles of vertical and horizontal transport.

In the GOA, upwelling supplies DIC-enriched subsurface waters to the surface. DIC concentrations are also higher in the northern part of the domain than in the southern (Fig. R1a). The westerlies drive a north-to-south Ekman transport of DIC-rich waters. During winter 2013, the supply of DIC by vertical transport decreased in the central GOA. At OSP, in addition to the reduced vertical supply, a weakening of the north-to-south horizontal Ekman transport also contributed to the decrease in surface DIC.

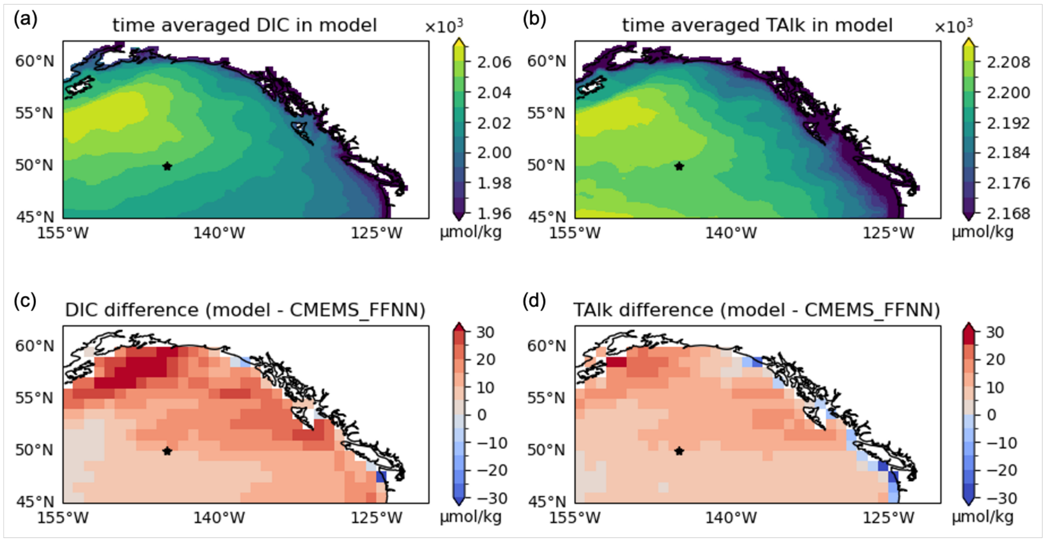

Figure R1. Comparison of the surface distributions of DIC and TAlk between the model and CMEMS_FFNN, one of the SeaFlux datasets. The star in each panel indicates the location of OSP.

We generated new figures illustrating the potential density and wind stress curl (Figs. R2 and R4). These figures clarify the stratification changes and their impact on the vertical transport.

The influence of horizontal transport is highly local (at OSP) and does not contribute to the domain-wide DIC decrease in the same way that vertical transport does.

Second, the main finding also relies heavily on stratification changes due to warming. Although the effect of warming on stratification is somewhat obvious, it would be helpful to include an explicit analysis, perhaps in the supplementary material showing the upper-ocean density gradient or a related metric. This may not require much additional effort but would enhance the mechanistic understanding and make the manuscript more self-contained regarding attribution.

As discussed in the response to the previous comment, we generated the zonal cross sections of density (Fig. R2) to document the enhanced stratification. Comparing the winter of 2013 with the DJF climatology, the potential density in 2013 is substantially lower at the surface between 160°W and 140°W. The enhanced stratification weakened the vertical transport of DIC, thereby reducing the supply of DIC to the surface.

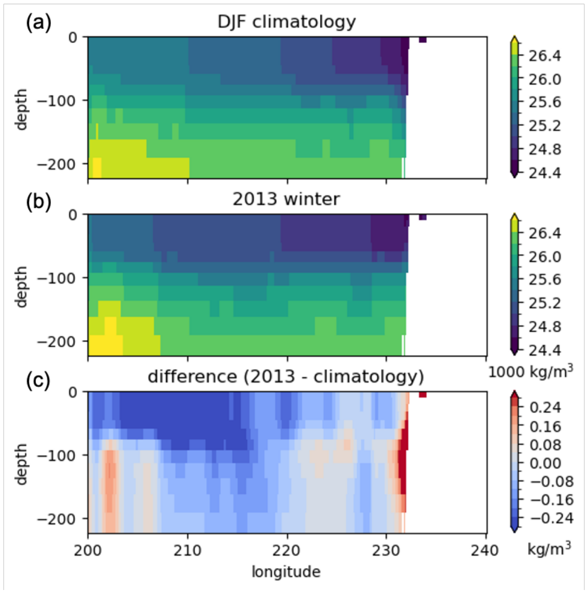

Figure R2. Vertical cross-sections of potential density along 50.5°N for (a) the DJF-mean climatology over 2010–2017, (b) 2013, and (c) their difference (2013 minus climatology).

Third, Supplementary Figure S2 appears to be central to the study, as it shows changes in surface transport. To make the analysis more complete, I suggest reducing the number of panels and moving a modified version of this figure into the main text. If there are restrictions on the number of figures, Figures 6 and 7 could be combined, this might also be visually more effective and the freed space could then be used for Figure S2. This is, of course, only a suggestion.

Regarding the maps of the horizontal advection (previous Fig. S2), we agree that these maps are informative. However, because the influence of horizontal advection is primarily localized around OSP, we believe that these results are most effectively presented in the supplementary material. We also improve the clarity of this figure (Figure R3) by focusing on the region near the OSP. In addition, we will revise the corresponding text to better explain the localized influence of horizontal advection around OSP.

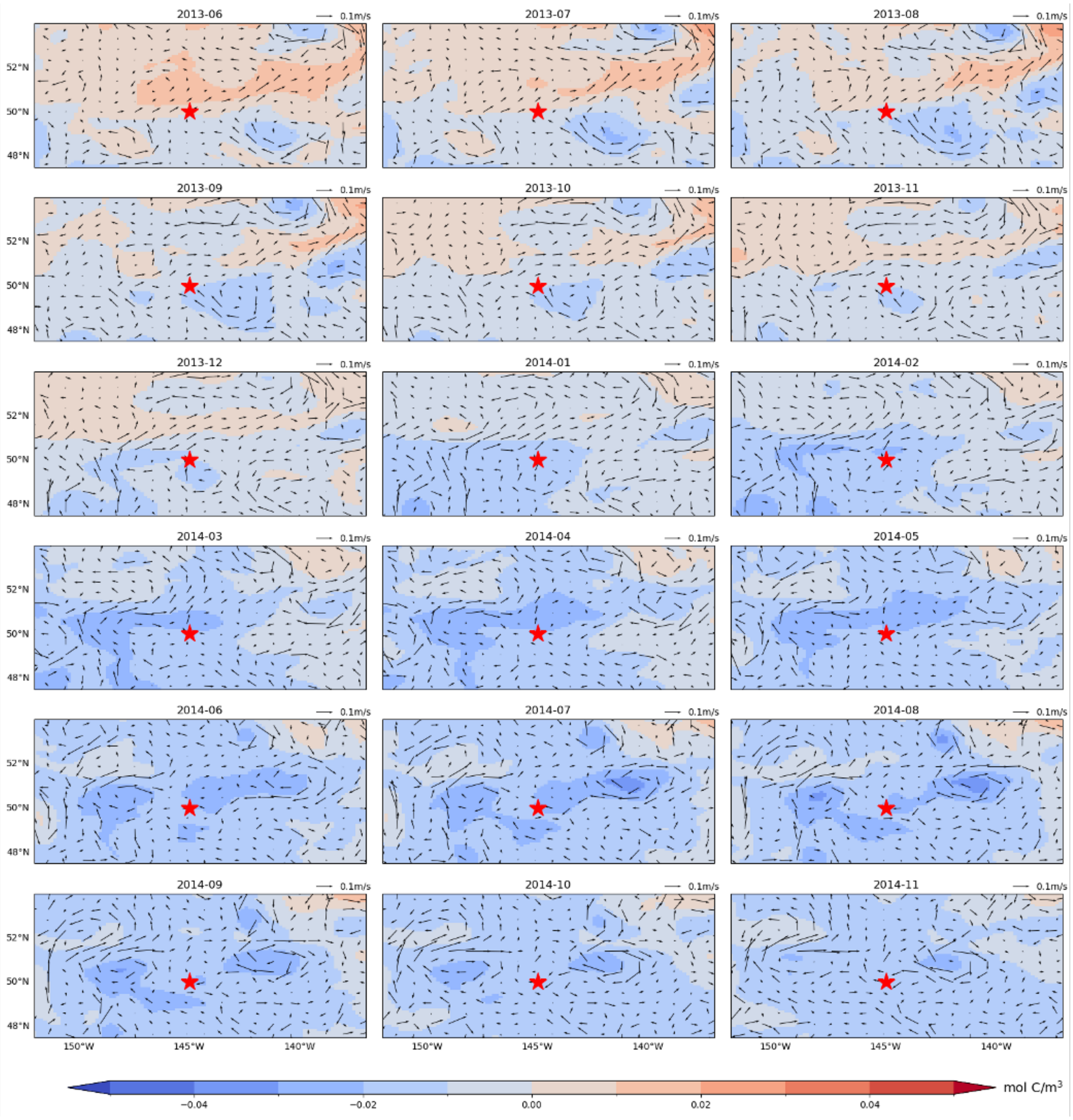

Figure R3. Surface velocity (vectors) and DIC (colored shading) anomalies relative to 2010–2017, from June 2013 to November 2014. All variables have been detrended and deseasonalized. Velocities are smoothed using a 3-month moving average. The red star indicates the location of OSP.

Finally, it would be helpful to strengthen the link to Ekman dynamics and entrainment discussed at the beginning of the Discussion section. The authors could support this point with additional literature or include a supplementary plot of wind stress curl to clarify the connection. Overall, this is an interesting and valuable study. The authors have most of the essential analyses in place but would benefit from refining the mechanistic explanations and reinforcing the main argument.

We agree to the reviewer's suggestions. We generated a new figure of the wind stress curl (curlτ) (Fig. R4). The comparison between the DJF climatology of the wind stress curl and the conditions in winter 2013 shows that, although the GOA is typically an Ekman upwelling region, the period immediately preceding the onset of the Blob was characterized by Ekman downwelling. As a result, vertical supply of DIC to the surface was suppressed, leading to a reduction in surface DIC. The reduced wind stress curl prior to the Blob provides clear evidence for the physical mechanisms underlying the DIC-driven pCO_2 response. We will be able to expand the corresponding discussion to clarify the link between Ekman dynamics, entrainment, and the surface DIC decrease.

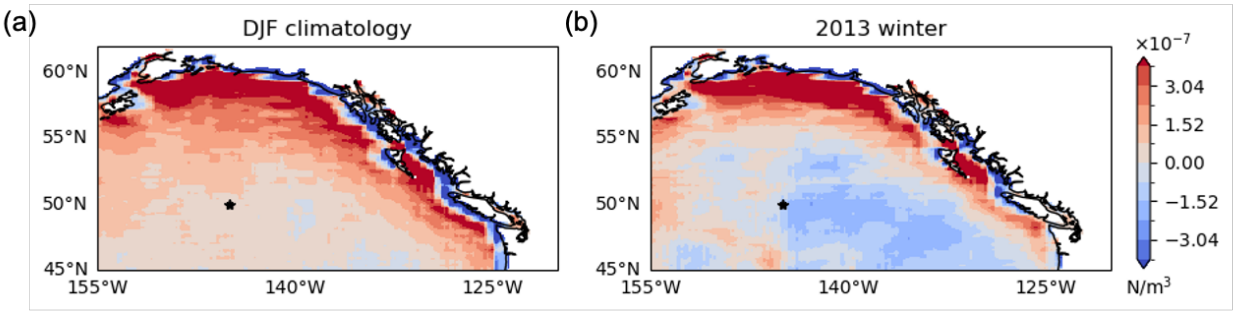

Figure R4. Spatial patterns of wind stress curl (curlτ) for (a) the DJF-mean climatology over 2010–2017 and (b) 2013. Positive values indicate cyclonic flow associated with Ekman upwelling. The star in each panel marks the location of OSP.