## RESPONSE TO RC1

We thank the reviewer for constructive comments. Our responses to all of your comments are given below.

This paper documents the validation of a relatively new 3D PBL scheme in an offshore application coupled to a wave model making it a state-of-the-science study well worth publishing. All my comments on this are relatively minor and I will list them below.

1. The main thing I would like to have seen is how the coupling is done to the wave model. This is something still not seen very often and it would be helpful to spell it out. I am aware that directional drags could also be implemented in such coupling and whether or not this is done should be mentioned. I assume the wave height and period come into the roughness length somehow. I believe this would add to the completeness of the paper.

We added a sentence to clarify this, see lines 89-92: “Every 30 minutes, the WRF model 10-m zonal and meridional velocities are passed to WW3 and the wave characteristics (significant wave height, wave period, and wavelength) are passed to the WRF model surface-layer parameterization. In the surface-layer parameterization, the time-varying surface roughness length is calculated from the wave parameters, with a specified Charnock parameter relation.”

2. Figure 6. Caption does not mention that potential temperature is also shown.

Thank you, we added a sentence indicating that the potential temperature is also shown. See new Fig. 6 caption.

3. Line 209 and Figure 10. "1800 UTC Nov 6". The observed winds don't show a maximum at that time (or any observations).

We made this sentence more specific: “In particular the observed maxima between 0600-1500 UTC Nov. 6 were underpredicted at SNS and MRY”. By maxima, we mean to say the approximate time series portion with the largest wind speeds. See L217-219 in the revised manuscript.

4. Line 229 and Figure 12. "0600 UTC Nov 6". This is not a time shown in Figure 12.

Thank you for noticing this error. We corrected the sentence to “At 0000 UTC Nov. 6, higher TKE was confined near the coastline.” See L238 in the revised manuscript.

5. Line 261. "full 3D PBL". May be helpful to add a "wherein..." here as I had to search back through the paper to find what the full option was.

We added a sentence to remind the reader what the essential aspects of the full 3D PBL parameterization are here. See L280-281 in the revised manuscript.

6. Overall while the differences were marginal, it was good to see the surface and lidar data validating the coupled model. If any simulations were carried out without coupling the wave model, it would have been interesting to see a note on whether that helped or why a wave model was coupled.

We appreciate this comment. We considered comparing simulations with and without coupling to the wave model. However, we deemed that work not essential to the main purpose of the manuscript, which is demonstrating that the 3D PBL parameterization works properly in coupled atmosphere/wave simulations and comparing it to a traditional 1D PBL parameterization in this framework. With an already long manuscript and this topic not being crucially related to the primary purpose of the manuscript, we defer this to future work.

## RESPONSE TO RC2

We thank the reviewer for constructive comments. Our responses to all of your comments are given below.

**REVIEWER 2**

This study is valuable in that it applies and evaluates the performance of a 3D PBL scheme with high potential at a time when high-resolution simulations are predominant. I provide several minor comments below.

Specific comments:

Fig. 1a: It would be beneficial to replace this with a higher-resolution figure. Improving the overall visual quality would help (e.g., adding latitude/longitude labels and making key information readily visible before reading the caption).

We added latitude and longitude lines to Fig. 1b. Unfortunately, the images in Fig. 1a are downloaded from the NOAA Weather Prediction Center (WPC) archive at https://www.wpc.ncep.noaa.gov/archives/web_pages/sfc/sfc_archive.php. We downloaded the high resolution images available and also zoomed into the area of interest. The key features of highs, lows, fronts, and pressure contours can be discerned. Unfortunately, NOAA WPC does not provide latitude and longitude lines on these maps, perhaps because it would distract from the other weather map information. We hope that adding the latitude and longitude lines to the satellite images helps geographically orient the reader. We thank the reviewer for this comment.

Fig. 2: Axis titles and units for the color bar should be clearly indicated. The presence of an outer map may lead to a misunderstanding that domains 1 and 2 are configured in a particular way. It might be clearer to present the full domains (d01 and d02) alongside a zoomed-in view of domain 2 in parallel.

We added a title to the figure so it clearly indicates terrain height in meters. To save space we did not show the axis titles of latitude and longitude for the y- and x- axes, respectively. However, to address this comment, we added a mention of this in the caption: “In this figure and subsequent two-dimensional plan-view figures over geographic regions, the y-axis is latitude in degrees north and the x-axis is longitude in degrees east.” The purpose of this figure is to show the zoomed-in “gray zone” d02 with the observations. This is a good point however, and a zoomed out view containing the WRF model d01 with an outline of the embedded WRF model d02 is shown in Figure 3 where the focus is on the large-scale synoptic evolution.

L121: Please clarify under what assumptions the approximated 3D PBL parameterization can be applied.

We added a sentence describing the broad assumptions when the 3D PBL parameterization can be applied: “The 3D PBL parameterization can be applied in simulations with horizontal grid spacings of O(100-1000 m) (the “gray zone”) where 3D effects are important since the most energetic turbulent eddies are neither fully parameterized nor fully resolved.” See L124-126 in our revised manuscript.

L137: The phrase “region of interest near the WRF model d02” is somewhat ambiguous. Is domain 2 not the primary region of interest? A more explicit description or a corresponding map would improve clarity.

Thank you, we removed “region of interest near the WRF model d02” and replaced it with “within the WRF model d02”. See L142 in the revised manuscript.

L149: Please revise to explicitly state “less westerly and stronger over the ocean *than the 3D PBL.*”

We corrected this as suggested: “…less westerly and stronger over the ocean than the COAWST 3D PBL simulation.” See L156 in the revised manuscript.

L149: The hub height of 206.5 m—what is the basis for this value, and how is it derived within the model?

Thank you for allowing us to clarify this oversight. We changed this sentence to: “Inspection of 24-hour time means before and after the frontal passage on the WRF model d02 at z = 10 m and z = 206.5 m, the latter of which is one of the WRF model vertical levels that is within the typical range of hub heights for offshore wind turbines (z = 75 − 225 m) (Sheridan et al., 2022), show...” See L152-154 in the revised manuscript.

Sheridan, L. M., and Coauthors, 2022: Offshore reanalysis wind speed assessment across the wind turbine rotor layer off the United States Pacific Coast. Wind Energ. Sci, 7, https://doi.org/66410.5194/wes-7-2059-2022

Fig. 6: The repeated references to time in the caption could be streamlined. Additionally, please clarify the meaning of each line shown in the figure.

We removed the repeated references to time and cleaned up the caption. See new caption of Fig. 6 in the revised manuscript.

Figs. 7, 8, 9: It would be preferable to place labels on the first panel rather than the middle. Also, repeating the terms “Bias/MAE” on every panel seems unnecessary.

We thank the reviewer for this comment, but we wish to keep the figures as they are. The reason the labels are not on the first panel is because the first panel (buoy 46013) is not within

the WRF model d02. Labels are listed in the panels only in which two or more WRF model simulations are compared to observations.

Fig. 7: Is it appropriate to directly compare 10-m wind with wind at buoy measurement height?

We thank the reviewer for noticing this error in the description. We forgot to mention that we reduced the 10-m winds to the lower buoy height using a log-layer assumption. We added this sentence to the figure caption: "The 10-m wind speeds in the simulations are extrapolated to the buoy height using a log-layer assumption with an ocean surface roughness length of $z_0=0.0002$ m." Please see the new Fig. 7 caption.

L209–210: Is this analysis valid without observational data? It is unclear whether winds in domain 1 are consistently underestimated beyond the MRY site.

The missing observational data certainly does not allow for us to make any conclusive statements beyond 1500 UTC November 6. At the over-land stations farther north (MRY, SNS, and WVI), the peak simulated wind speeds were lower than observed (Fig. 10). In particular, the observed maxima between 0600-1500 UTC Nov. 6 were underpredicted at SNS and MRY. However, the simulations cannot be evaluated beyond 1500 UTC Nov. 6 due to missing observational data.

L210, Fig. 10: The SPB site was previously abbreviated as SBP; please ensure consistency.

The figure has been corrected with 'SBP' in the title. Thank you for noticing this error.

Sections 3.4–3.5: The comparison with buoy and Doppler lidar observations lacks a clear emphasis or key takeaway, and currently reads as largely descriptive.

We appreciate this comment and agree key takeaways are necessary. Therefore we created a new subsection 3.6 that summarizes the key takeaways from the observational evaluation. Please see L258-264 in the revised manuscript.

L271: Please clearly denote this as the "approximated 3D PBL."

We added "approximated" before 3D PBL. Please see L290 in the revised manuscript.

L301: It appears that the 3D PBL code is not included in commit #843f8be. Please update this to reflect the most recent commit hash.

We updated this to the most recent commit hash #b60ddac.

I would like to offer a few additional comments.

L87: Please specify the version of the model used in this study.

The versions of models used in COAWST are listed in the ‘Code and data availability section.’

L92: Since ERA5 has a horizontal resolution of 0.25°, it would be helpful to clarify whether it is appropriate to run the experiment at such a high resolution (2 km) directly in d01.

This is a good point and open to debate. While HRRR is higher resolution than ERA5, it has not undergone the more complete data assimilation processes of a true reanalysis, and that it also covers a more limited offshore extent than ERA5. However, even though ERA5 is at 0.25 deg., the initial and lateral boundary conditions are downscaled to 2-km on the WRF model d01 during the model integration.

L94: Please provide the model top (upper bound) value.

The model top is 50 hPa. Please see L98.

Figure 3: Please indicate the reference vector in the figure.

There is no reference vector in the figure. There are some inland lakes where vectors were added, which may have appeared to be a reference vector.

Overall: If the differences among the experiments are explained solely in terms of non-local mixing term, the justification for using the 3D PBL parameterization may appear somewhat limited. It may therefore be beneficial to further interpret the results from the perspective of horizontal turbulence as well.

We appreciate this comment. In section 3.7 and Fig. 15, the differences in horizontal turbulence can be assessed since the vertical mixing is the same. As shown by Fig. 15 and discussed in section 3.7. While the differences between the 1D and 3D PBL parameterizations (without non-local mixing) are more minor, clear differences in the horizontal structure of turbulence are evident. We note in this section that the full 3D PBL parameterization would likely show more significant differences due to its more realistic treatment of horizontal mixing and we defer this to future work.

We wish to clarify that the differences are mostly, but not solely, due to very different treatment of vertical mixing between the YSU and MYJ PBL parameterizations (including the nonlocal mixing term in the YSU PBL parameterization). As noted above there are clear differences in the horizontal turbulence structures between the 3D and 1D PBL parameterizations due to the differences in horizontal mixing treatment.