The manuscript "Validation of Turbulence Intensity as Simulated by the Weather Research and Forecasting Model off the U.S. Northeast Coast" explores the turbulence intensity reproduced with WRF model offshore, comparing with observations from a lidar profiler and other instrumentation. The manuscript is worth to be published, contributing to a better knowledge of the ability of the model to resolve the main meteorological variables and the TKE, or the turbulence intensity. Results are well presented, plots are correct and illustrate well the results. However, in the text, in general, my feeling is that more detailed explanations about the obtained results are needed. In addition, 3 major points need to be addressed.

My major concerns are:

- Results with SST update are very interesting and seem to be closer to observations. It seems that a big part of the paper is about the discussion about the added value of the SST update in comparison with the baseline simulation. Have you considered focusing the manuscript in these topic, and maybe changing the title, the main focus, etc. In the present form it is confusing why the sst update is a big topic and not others, such as the PBL scheme, the surface layer scheme, initial conditions, vertical levels... etc

Response: We thank the reviewer for the great comment. We would like to note that the choices of model physic parameterizations in this study are consistent with the setup for the 20-year wind resource dataset released by the National Renewable Energy Laboratory (NREL) (Optis et al. 2020) The model configuration for the CA20 dataset was determined through a series of sensitivity studies including varying PBL schemes, reanalysis forcing, sea surface temperature forcing, and surface layer schemes.

Since the HRRR analysis, the atmospheric forcing we used for our WRF simulations, does not include SST data, we must adopt one from the existing SST products. The NASA JPL GHRSST data was chosen as it has higher resolution (0.01°) than what were used in Optis et al. (2020). A few sentences are added in Section 3.3 from Line 291 to 302 to provide more details on this.

While the discussion of sensitivity of prescribed model SST is particularly included in the manuscript, we acknowledge many other treatments in the model would interactively contribute to the uncertainties in simulated wind and turbulence at the hub height. Hence, we briefly describe the potential sources of model uncertainty in Section 3.1 from Line 139 to 141 to provide more background and hopefully it helps clarify why we didn't test the sensitivity to model physics or other factors.

- Metrics are included but not described in methodology. Please, include a section with the formula of Bias, RMSE and others if needed.

Response: We thank the reviewer for the comment. To address this, we've included descriptions for the metrics (RMSE, bias, and correlation coefficient,) in the Section 4.1 from Line 357 to 365.

-Another important point is what was the criteria to choose the physics WRF configuration. Are there previous studies that proof a suitable model physics? Apart from SST update, have you tried any other experiment varying physics? PBL? Other initial conditions? Vertical levels?

Response: We thank the reviewer for bringing up these questions. As in our response for the first comment, the model configuration was based on the results of model sensitivity experiments described by Optis et al. (2020). We acknowledge many other treatments in the model could contribute to the uncertainties in simulated wind and turbulence at the hub height. We've added a few sentences from Line 144 to 157 in Section 3.1 to address the comment:

"The choices of model physics parameterizations for this study are consistent with the setup for the 20-year wind resource dataset released by the National Renewable Energy Laboratory (NREL) (Optis et al. 2020). Optis et al. (2020) conducted a series of model sensitivity experiments with respect to surface layer and PBL parameterizations, reanalysis data, and SST forcing. The results of their model assessment indicated that the largest uncertainty is associated with the choice PBL parameterizations, and the Mellor-Yamada-Nakanishi Niino (MYNN) boundary layer parameterization (Nakanishi and Niino 2009) generally outperforms the Yonsei University (YSU, Hong et al. 2006) scheme off the east coast of North America. Hence the MYNN boundary layer parameterization, as well as other parameterizations described by Optis et al. (2020) were used for generating the CA20 dataset as well as our simulations."

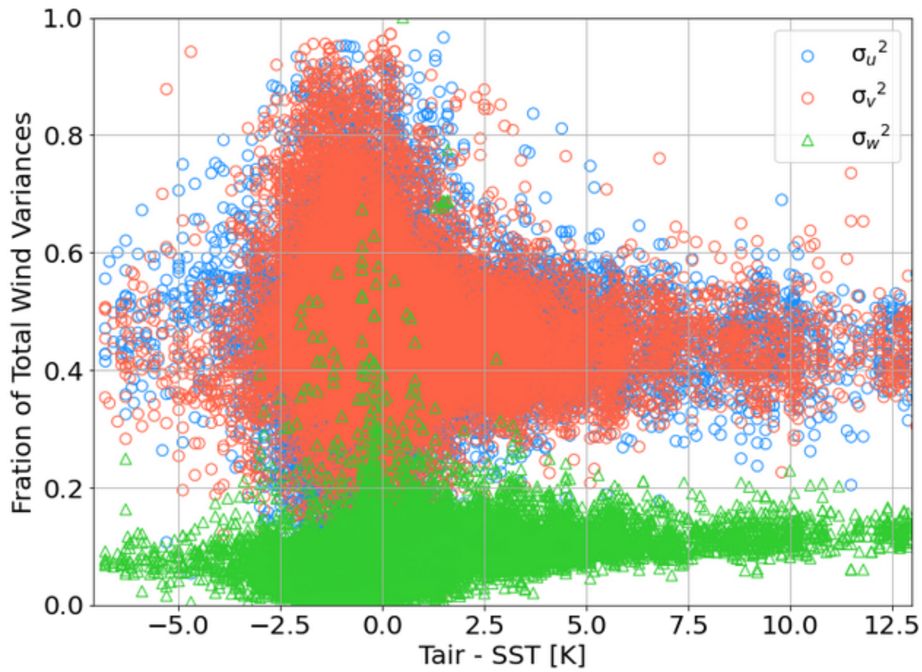
Some minor comments are indicated below:

Line 45-46: and during night?
Response: The structure of marine PBL is very dependent on both the variations of sea surface temperature and air temperature near the sea surface. We revised the sentence to clarify this point as in Line 43 and 44.

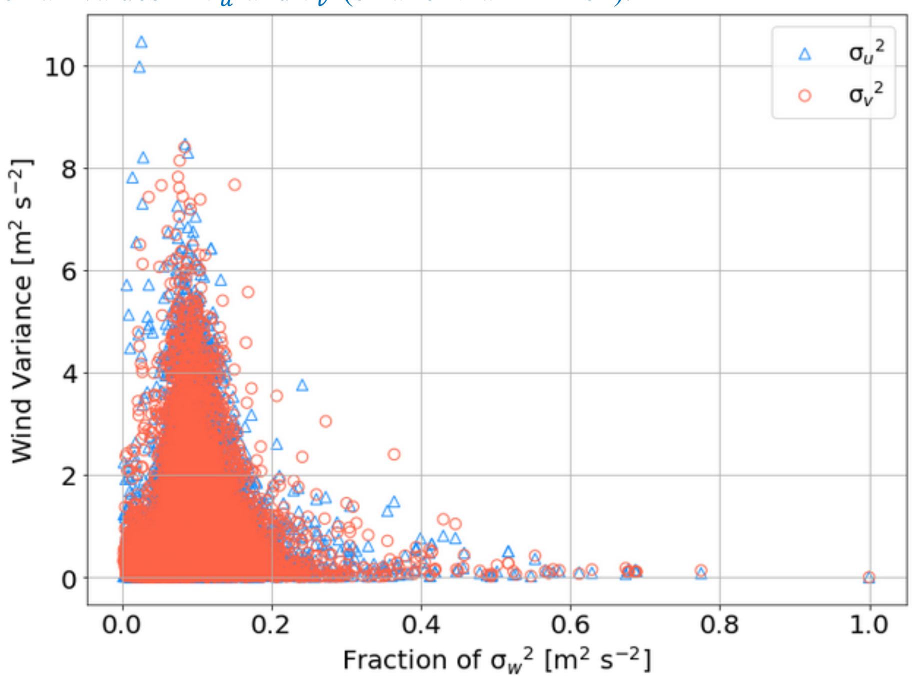
Line 109: 23 dB (include an space)
Response: This is corrected.

Line 146: w_variance is always negligible? Please, detail cases when this assumption is not correct.
Response: We reviewed the observational datasets collected at the MVCO ASIT and we used them to characterize the distribution of observed wind variances and summarized results as following:

Figure below shows the fraction of three components of wind variances normalized by the total variance ($\frac{\sigma_u^2}{\sigma_u^2+\sigma_v^2+\sigma_w^2}, \frac{\sigma_v^2}{\sigma_u^2+\sigma_v^2+\sigma_w^2}, \frac{\sigma_w^2}{\sigma_u^2+\sigma_v^2+\sigma_w^2}$) as a function of stability (using buoy-measured air temperature minus SST as its proxy). The results suggest overall, the fractions of horizontal wind variances (σ_u^2 and σ_v^2) are larger than the vertical wind variance (σ_w^2). In addition, there is no evident correlation between the fraction of σ_w^2 and stability. In most of the conditions, the fraction of σ_w^2 is no greater than 0.2. Only a few data points exceed 0.2 but they mostly occur during neutral conditions (Tair – SST is close to zero).



We further analyze what may cause those data having relatively large fraction of σ_w^2. By looking at the corresponding horizontal wind variances (σ_u^2 and σ_v^2) in function of fraction of σ_w^2 as in the figure below, we find that the relatively large fraction of σ_w^2 (larger than 0.4) is most likely due to concurrently small values in σ_u^2 and σ_v^2 (smaller than 1 m^2 s^{-2}).



To address this comment, we add a figure (Figure 2 in the revised manuscript) to illustrate the observed PDFs of three wind components of variances and their dependency on stability. Corresponding descriptions can be found in Section 3.2 from Line 202 to 206.

Table 1: Mean wind speed in 10-min window

Response: This is corrected.

Fig2 caption: observations from lidar? Please indicate
Response: The observations are from both lidar and buoy. It is now included in the caption.

Line 218: is there cancellation in the MB between negative and positive values?
Response: From the formula of bias, the negative and positive values could possibly compensate each other. However, if the sign of biases is relatively consistent throughout the data sample, MB would be still useful in identifying the overall bias.

Line 286: relative importance? Please, give details
Response: We revised the sentence now as "which can be explained by how TI is calculated in Eq. (4)."

Figure 7: green line in panel WSPD is difficult to see
Response: The green line in this figure (Figure 8 in revised manuscript) is changed to blue.

Line 322: please explain better
Response: The sentences mentioned here are revised.

Line 339: please define the buoyancy component
Response: We modified the sentences in Line 514 and 515 where the two terms buoyancy and shear components are first defined in the text.

Line 354: yes, so what about YSU?
Response: In our responses to the major comment, we clarify the appropriateness of using the MYNN parameterization for our study. In addition, since TKE is not diagnosed in the module of the YSU scheme, it is difficult to answer the question at this moment.

Line 361: this regime can be low shear and stable conditions, maybe intermittent turbulent events?
Response: It is possible that intermittent turbulent events may cause occurrence of large TI during low shear and stable conditions in addition to resolved turbulence linked to mesoscale convective events. To address this, we revised the sentence in Line 580.

Line 370: ok, but also intermittent turbulent events?
Response: Please see the response for previous comment.

Figure 10 caption: buoyancy is the numerator of Rib?
Response: The buoyancy here is defined as air-sea virtual temperature difference ($\Delta\theta_v$) as stated in Line 514. We include exact ingredients for both buoyancy and shear components in the caption to clarify.

Table 2: please, join it with table 1 or refer to it in the discussion
Response: The Table 2 is already referred in Line 604.