Review Comments 2:

The manuscript presents a set of experiments aimed at demonstrating the impact of 1) coupling between the atmosphere/ocean, 2) further coupling the atmosphere/ocean model to a wave model and 3) various parameterizations of Langmuir turbulence using tropical cyclone Mekunu as a case study. The paper is clear, well written and provides a great level of detail on the various model formulation settings which is very useful for readers who want to explore similar experimentation.

The authors demonstrate significant improvement to the mean and RMSE values of cyclone central pressure, wind speed and latent heat fluxes through coupling. Though as the authors point out, the CPL.AOW model does not outperform the CPL.AO model. It would have been nice to see some discussion on why this might be. There are additional figures comparing the evolution of SST and MLD in the CPL.AOW simulation and that in the HYCOM analysis; however, there is little discussion of these figures. For instance, I'm surprised the HYCOM analysis has a great decrease in SST but smaller decrease in MLD.

The second section of "Results" (Section 5 for some reason) examines the impact different parameterizations of Langmuir turbulence have on SST and MLD. The authors demonstrate that the LF17 and LF17-ST experiments produce more accurate changes in SST with greater cooling and greater decreases in MLD relative to an experiment without Langmuir turbulence included. Interestingly, they also find that the VR12-MA experiment produces worse results than running without Langmuir turbulence. They attribute this fact to a reduction in turbulent shear and ocean mixing by the VR12-MA scheme.

Overall, the manuscript is straightforward, easy to follow and presents some interesting results that I feel many in the scientific community will find useful. I feel the manuscript only requires minor revision with perhaps a bit more discussion in the Results section and addressing the comments below.

We thank the reviewer for the comments that acknowledge our manuscript. Because this manuscript aims to demonstrate the development of the coupled model, we tried to focus on the technical development and demonstrate the capabilities of the coupled model. Now we have added more discussions on the physical insights and the SST trend in the sensitivity analysis in Sections 4 and 5.

Specific Comments:

Sort of a general note, but I'm surprised the first mention of using an ensemble comes on Line 195. I would think this would have been mentioned in the abstract or perhaps further up in the Methodology section.

Because we added random noise to each grid point, we put the setup of the 20-member ensemble simulation in Section 3.2 on model setups. The random noise is added simply to avoid digital precision error due to model internal variability. Now we have added the description of the ensemble experiment in the abstract:

Because of the chaotic nature of the atmosphere, we ran an ensemble of 20 members for each coupled and uncoupled experiment.

Section 2.3: It might be useful to have some description on why these three Langmuir parameterizations were selected. Especially since the Results section shows that the LF17 formulations are quite similar in their impact and the VR12-MA simulations are substantially different.

We tested these options because they are used in many global and regional models that reduce the error in simulating the mixing layer depth and SST (Li et al., 2016, 2017). Now we have added the description of the Langmuir turbulence parameterizations used in Section 2:

The three Langmuir turbulence parameterizations are selected because VR12-MA and LF17 have been shown to substantially improve shallow biases of mixed layer depth (Li et al., 2016, 2017, and 2019).

Line 196: “small random perturbations to the initial SST (<0.01 degC) at every grid point in the coupled model.” Are they actually random or is there some amount of spatial/temporal correlation?

Now we have added the following sentence to the manuscript:

The random perturbations are added without any spatial or temporal correlation, aming to demonstrate the internal variability of the model.

Line 234-235: The language here does not reflect that there are three model being compared. Unless the purpose it to only discuss the coupled model simulations.

Now we have revised this sentence:

First, we examine the characteristics of cyclone Mekunu obtained from CPL.AOW, CPL.AO, and ATM.DYN to demonstrate the capability of the coupled model. The tracks of the tropical cyclone, defined by the positions of the low pressure center, are presented in Fig. 2, where it can be seen that all models can qualitatively match the observed evolution and track. Although the translation speed of the tropical cyclone from CPL.AOW is somewhat slower (CPL.AOW: 236 km/day; IBTrACS: 254 km/day), the distances between the cyclone centers for all model runs and IBTrACS data are less than 250 km until May 26, shown in Fig. 3(a).

Figure 3: Interesting that the models are somewhat indiscernible until ~day 3. Suppose this illustrates the time necessary for the ocean/waves to begin to have a meaningful influence on these metrics.

We started the simulations on May 20 and the model outputs are indiscernible for 5 days. In Figure 3 we did not show the indiscernible results from May 20 to 22. This is because the SST does not change significantly in the first few days. This is also consistent with our previous study in Sun et al., 2020 for atmospheric river events. Now we have added one sentence in the caption of the figure:

The simulations start on May 20, but the results are not presented before the pressure starts to drop on May 22.

Lines 261-263: Not sure what's being said here.

Now we have removed this sentence that causes confusion.

Line 288: Please describe why this figure shows that the wave height is sensitive to the wind speed.

Now we have added the contours of wind speed in this figure. The standard deviation of the wave height Hs is larger when the wind is strong. We have also revised our manuscript to clarify this:

Near the eye wall of the tropical cyclone, the standard deviation of Hs from the ensembles is approximately 3 m, showing greater variance near the eye wall (Fig. 7b),

Line 298: Would be useful to provide more description on why spectral nudging is necessary.

Now we have added more description on the spectral nudging:

To evaluate the effect of Langmuir turbulence on the ocean, we also performed the simulations using spectral nudging in WRF in addition to the ``free runs" (simulations without spectral nudging). The spectral nudging is performed because of the uncertainties of the atmosphere model, especially for the tracks of the cyclones. By restraining the uncertainty of the atmosphere using spectral nudging, we are able to highlight the impact of the Langmuir turbulence on the ocean.

Figure 9/10: Perhaps I'm missing something, but in the figures with spectral nudging the red dot isn't centered on the largest SST/MLD differences. Should it be?

Yes. The largest changes are not centered on the red dots. We hypothesize that it is because (1) SST/MLD changes need some time to develop and (2) the winds on the right-front quadrant of the cyclone are strongest. Now we have added this to the manuscript:

It is noted that the largest SST and MLD changes are not centered on the location of the tropical cyclone. We hypothesize that this is because (1) the SST and MLD changes need some time to develop and (2) the winds on the right-front quadrant of the cyclone are strongest (Moon et al., 2014; Fan et al., 2009).

Lines 339-342: Having trouble connecting this phrase to figure 11. Perhaps demonstrate explicitly if it is seen in the figure

We have replotted Fig. 11 and improved its quality. We have also revised our discussions to the results shown in Fig. 11.

Technical Comments:

Line 41: "the Antarctica"?

We have replaced “the Antarctica” using “the Southern Ocean”.

Line 167: Maybe just personal preference, but I would rephrase to “The ocean-atmosphere model is not coupled to the wave model”. Same for #3 (Line 169).

Now we have rephrased it:

The ocean–atmosphere model is not coupled to the wave model, aiming to demonstrate the impact of the wave model on the simulation results.

The atmosphere model is not coupled to the wave or ocean models.

Line 176: Perhaps linearly interpolating “to” not “between”?

In MITgcm, we used the daily temperature, salinity, and current velocity to drive the coupled model. At each time step, the boundary conditions are interpolated between the HYCOM data before and after it. Now we have rephrased it:

At each time step, the boundary conditions for the ocean are updated by linearly interpolating between the daily HYCOM/NCODA analysis data.

Line 249: “Despite CPL.AOW better simulates…” Believe some words missing here.

Now we have removed this sentence to avoid confusion.

Line 267: Figures 5b, c. Not 5a.

Line 271: Fig. 5c. Not 5b.

Line 277: Fig. 6b, c. Not just Fig. 6b

Figure 7 Captions don’t match the figure.

Thanks. Now we have fixed the typos with these figures. We have also gone through the manuscript carefully to check the figures.