Review Comments 1

General comments

In this manuscript the authors assess the performance and sensitivity to different parameterizations of a regional atmosphere-ocean-wave coupled model in simulating cyclone Mekunu in Arabian Sea. The authors first compare the performance of an atmosphere-ocean-wave fully coupled simulation, an atmosphere-ocean coupled simulation, and a standalone atmosphere simulation. They conclude that both versions of the coupled simulation give better results than the standalone atmosphere simulation. They further examine the sensitivity of the coupled simulation to different options of Langmuir turbulence parameterization and found that the simulation results including the mixed layer depth and sea surface temperature are sensitive to the choice of Langmuir turbulence parameterization. The authors also report the sensitivity of the simulation results to different choices of ocean surface roughness parameterization in the appendices.

In general this is an interesting study. The results are helpful for improving our understanding of the atmosphere-ocean-wave coupling during cyclones, and are useful for the development of regional atmosphere-ocean-wave coupled models. While the manuscript is easy to read, I think it can be significantly improved by a careful revision.

We thank the reviewer for acknowledging the usefulness of our work and providing comments that helped improve this manuscript.

One of my major concerns is that the focus of this study doesn't seem clear to me. If I understand it correctly, the focus of this study is to assess the effects of ocean surface waves by incorporating a wave model WaveWatch III into a regional atmosphere-ocean coupled model SKRIPS, using cyclone Mekunu as an example. If this is the case, the comparison with a standalone atmosphere model WRF seems to distract the readers from the focus. Also, coupled model has more skill in simulating cyclones than standalone atmosphere model may not be entirely new. I'd suggest the authors focus more on the impact of ocean surface waves by the coupling with a wave model. In this sense it would be better to examine in more detail what are the impact of including the effects of Stokes forces, Langmuir turbulence, wave modulated wind stress and ocean surface roughness seen by the atmosphere as introduced in Section 2 on simulating cyclone Mekunu. The presentation of the results in Section 4 is very brief and is not focusing on the effects of waves in my opinion, whereas Section 5 only discusses the impact of different options of Langmuir turbulence parameterization, which is only one of the wave effects included in this coupled model. So the section title of both sections are very confusing. In addition, the results of different sea state dependent surface roughness closures are presented only briefly in the appendices, which is also confusing to me why the authors choose to present these materials there.

Reply:

The goal of the paper is twofold: (1) demonstrate the implementation of the coupled ocean-wave-atmosphere model; (2) evaluate the impact of the surface waves to the coupled system. We have edited Section 1 to highlight our goals:

The goal of this work is twofold. First, we demonstrate the integration of the wave model WAVEWATCH III to the Scripps–KAUST Regional Integrated Prediction System (SKRIPS, Sun et al., 2019), which is a regional coupled ocean–atmosphere model that has been used to investigate extreme heat wave events on the shore of the Red Sea (Sun et al., 2019), North Pacific atmospheric rivers (Sun et al., 2021), and sea-ice evolution in the Southern Ocean (Cerovevcki et al., 2022). The second goal is to evaluate the implementations of ocean surface waves in the coupled system, especially for Langmuir turbulence that increases ocean mixing and cools down the SST during the simulation. Here, we perform a series of coupled and uncoupled numerical simulations of tropical cyclone Mekunu in the Arabian Sea.

We have also clarified the comparison with stand-alone WRF model in Section 4. By comparing with the uncoupled runs, we can highlight the changes due to air–sea coupling and the effect of the waves:

In this section, the ensemble coupled simulation results (i.e., CPL.AOW and CPL.AO) are compared with the results from the uncoupled runs (i.e., ATM.DYN) to assess the performance of the models, the impact of coupled feedbacks, and the effect of the waves.

Section 4 provides the comparison of simulations with and without all the effects of waves (e.g., Stokes forces, Langmuir turbulence, wind stress, and ocean surface roughness). In this section we showed that the effect of the surface waves does not signifitanctly impact the characteristics of the tropical cyclone in the simulation.

Furthermore, section 5 details the sensitivity analysis of Langmuir turbulence; Appendix C presents the sensitivity analysis of other effects of the waves. We did not present each component of the wave effects because their impact on the tropical cyclone characteristics is not significant, as shown in Fig. 3. The literature also suggests that the effect of Stokes forces could cancel each other in the coupled simulation (Suzuki and Fox-Kemper, 2016). Due to the chaotic nature of the atmosphere during the tropical cyclone event, the other effects of the wave model are not significant and thus we did not examine all the ocean/atmosphere variables obtained in the simulations.

Now we have revised the name of Section 4 to "Comparing coupled and uncoupled models". We have also added the comparison of the coupled simulation with/without the Stokes forces and the wind stress terms in Appendix C.

Another major comment is on the result of VR12-MA, one of the Langmuir turbulence parameterizations tested in this study. The authors found that using VR12-MA makes the simulated mixed layer depth shallower and sea surface temperature warmer in the cyclone wake than the simulation without Langmuir turbulence parameterization. This result is not intuitive as it is expected that Langmuir turbulence enhances the vertical mixing and deepens the mixed layer. The authors provide a possible explanation in Section 5.2 by examining the regionally averaged vertical profiles, which is very interesting. This may highlight a deficiency of KPP which uses a bulk Richardson number to determine the boundary layer depth, which might be sensitive to the structure of the velocity and buoyancy profiles. I'd suggest the authors to look closer to this issue, perhaps by plotting the time evolution of these profiles in Figure 11 at a point on the cyclone track and check how these profiles change as the cyclone passes by. I guess VR12-MA would still give stronger deepening of the mixed layer depth during the cyclone, but the mixed layer depth may be shallower after the cyclone for reasons suggested by the authors.

Reply: Now we have attached the temporal evolution of these profiles (Fig 1 and 2 below). The vertical profiles of the Richarson number and velocity are similar throughout the tropical cyclone event and thus we only present the results on day 4 in our manuscript.

We have modified the manuscript to clarify the seemingly counterintuitive results from the VR12-MA simulations. When the Langmuir turbulence effects are considered in VR12-MA, the turbulent velocity scale w_s increases and initially strengthens the parameterized diffusion of temperature and salinity. However it also increases the parameterized vertical viscosity and reduces the velocity shear. This reduces the (Ur–U) term in Eq. (5), enlarges the Richardon number (which determines the parameterized mixing layer), and lessens the mixing and MLD in the model. These results show the importance of assessing the full parameterization when one aspect is changed, and this includes assessing how the shear is determined. Although using VR12-MA can initially improve the simulation of mixing layer depth and SST, it is also showing counterintuitive results may occur via feedbacks from the reduction in the shear (Li et al., 2016).

To illustrate the impact of the changes of velocity shear, we performed another experiment by not enhancing the diffusion of the velocity in KPP. This new option is shown in the figure below as VR12-MA-NoU. Still, the temperature and salinity diffusion are enhanced in this experiment. It can be seen in Fig. 3 below that the MLD deepens by 20 m and SST cools down by about 0.5 degree along the wake of the tropical cyclone. This is indicating that, when enhancing the diffusion of velocity in VR12-MA, it shoals the boundary layer and reduces the vertical mixing. Now we have revised our Section 5.2 to clarify our findings using VR12 and added Fig. 3 to Appendix D.

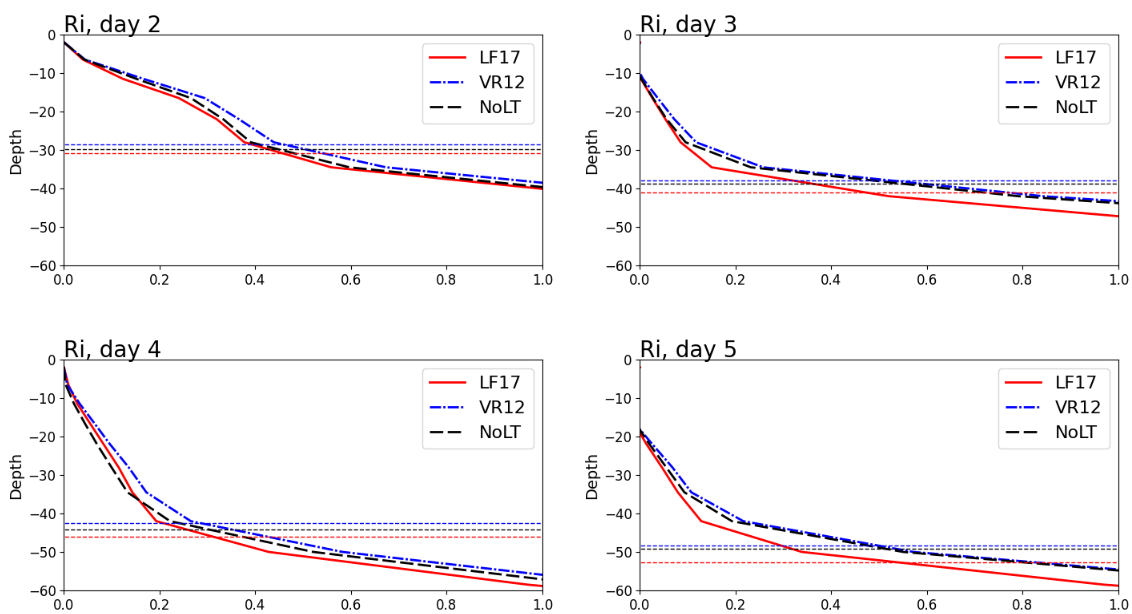


Fig. 1. The temporal evolution of the Richardson number profiles. The profiles on day 2 to day 5 are presented. The horizontal dashed lines are indicating the boundary layer depth in KPP.

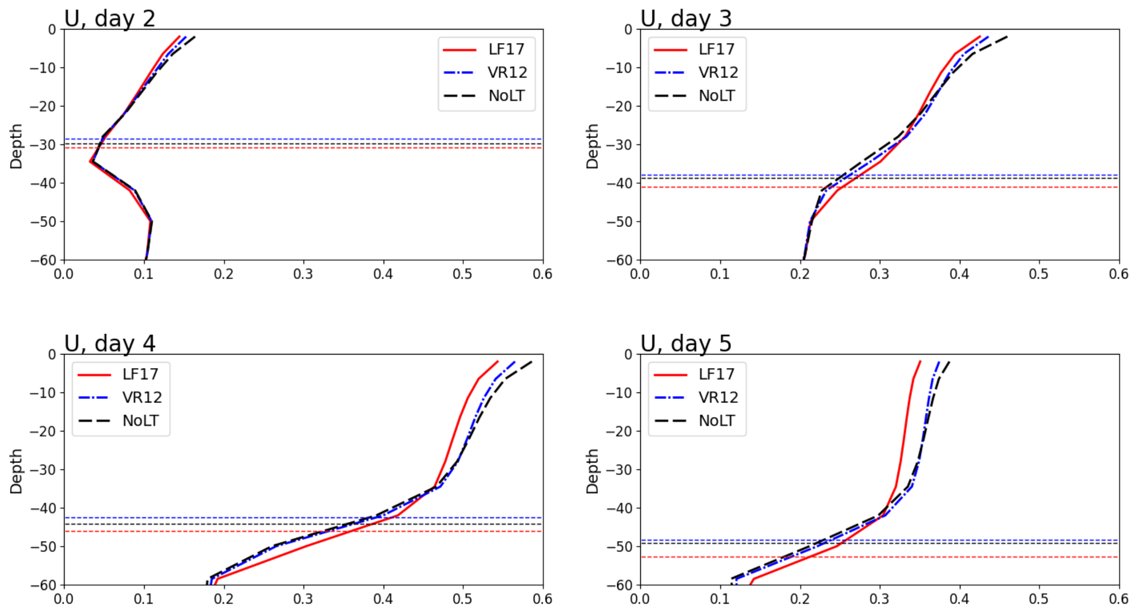
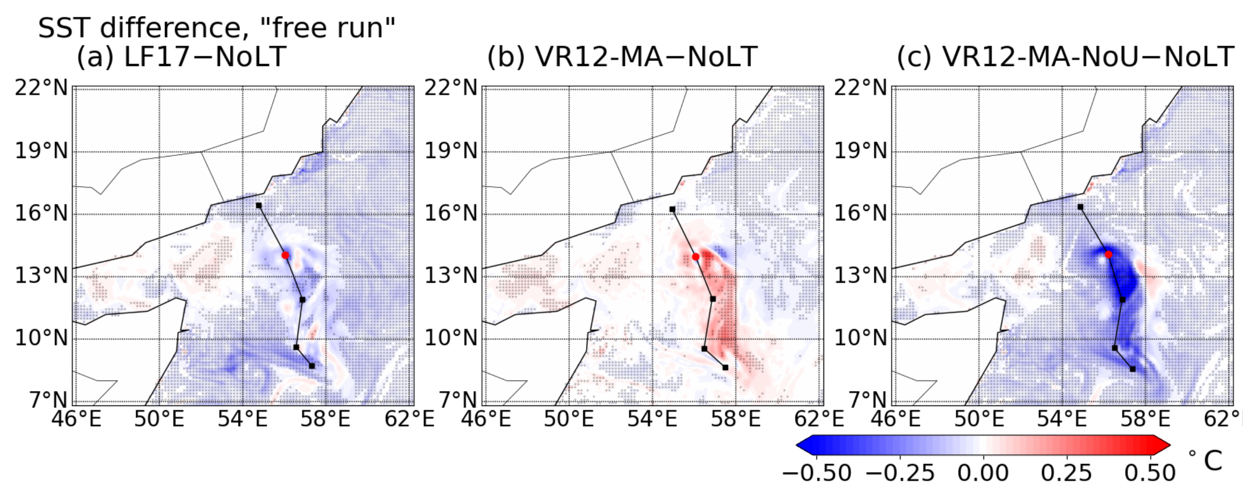


Fig. 2. The temporal evolution of the horizontal current velocity profiles. The profiles on day 2 to day 5 are presented. The horizontal dashed lines are indicating the boundary layer depth in KPP.



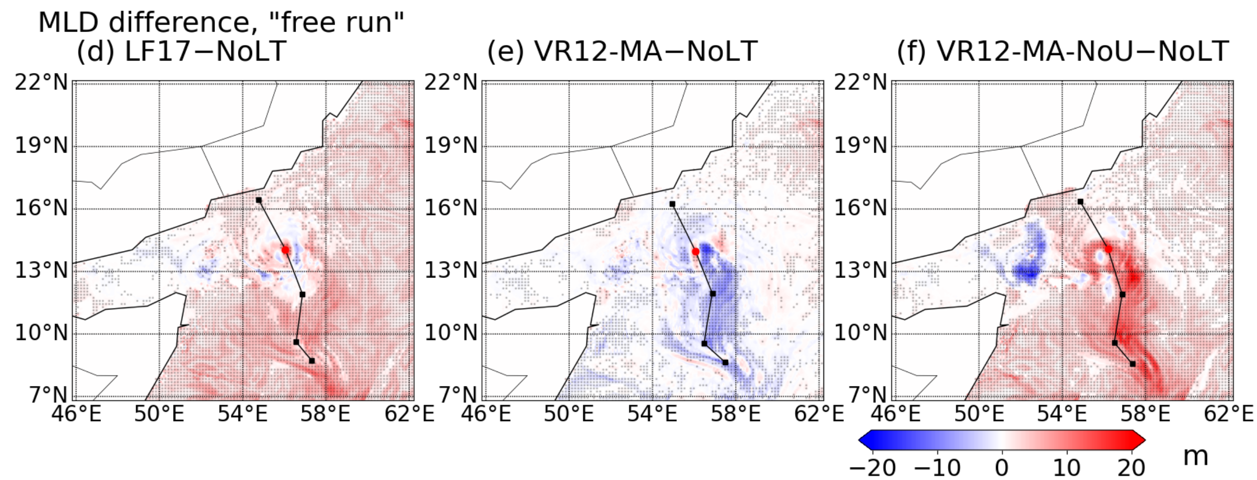


Fig. 3. The snapshot of the ensemble averaged SST and MLD differences. Panels (a-c) show the SST difference between the simulations with Langmuir turbulence (CPL.LF17, CPL.VR12-MA, and CPL.VR12-MA-NoU) and without Langmuir turbulence (CPL.NoLT). Panels (d-e) show the differences in MLD. The markers indicate the regions where the SST difference is significant ($P < 0.05$).

I'd also appreciate it if the authors could provide more detailed discussion on the results. My impression is that the authors presented a lot of figures showing the results, but the corresponding description and discussion in the text are rather brief.

Thanks. Now we have revised our paper according to the reviewer's comments and added more discussions to Sections 4 and 5.

Specific comments

L5: Why comparing with a standalone atmosphere model? The difference would be dominated by the effect of including an active ocean model? Why not comparing with the coupled model without the wave component?

We compare with a standalone atmosphere model to demonstrate the improvement of the coupled model over uncoupled models. We also compared the effects of the waves in the coupled system. Now we have revised this sentence in the abstract to avoid confusion:

We examined the model skill in these simulations and further investigated the impact of Langmuir turbulence in the coupled system.

L9: Is Langmuir turbulence the only way through which the effects of waves are included? It might be helpful to mention what wave effects are included in the coupled model.

The Langmuir turbulence is not the only thing that we included. Now we added the other effects of ocean surface waves to the abstract:

In our implementations, we considered the effect of Stokes drift, Langmuir turbulence, sea surface roughness, and wave-induced momentum flux.

L22: "Intensity" -> "Intensity of TCs"?

This text has been corrected.

L37-38: Is Langmuir turbulence the only way impact of surface waves is implemented in this study? I know this becomes clear in section 2. But it would still be helpful to discuss at least why Langmuir turbulence is emphasized here.

We focused on Langmuir turbulence because the bias of the coupled model is alleviated when it is considered. According to the literature (Li et al., 2016; Reichl et al., 2016), in the coupled ocean–atmosphere model the SST and mixing layer depth are also sensitive to Langmuir turbulence. When the effect of Langmuir turbulence is considered, the mixing layer gets deeper and the SST cooling could be stronger by 0.5 to 0.7 degrees. Now we have revised this sentence in L37-38 and emphasized the Langmuir turbulence:

The second goal is to evaluate the implementations of ocean surface waves in the coupled system, especially for Langmuir turbulence that alleviates the model bias (Li et al., 2016). The coupled model is also sensitive to the implementation of Langmuir turbulence because it increases ocean mixing and cools down the SST during the simulation.

L68: What "surface boundary fields" are exchanged here?

The exchanged surface boundary fields are detailed in L76-86. Now we have re-organized Section 2 to avoid confusion:

The schematic description of the coupled model is shown in Fig. 1. We separated the WW3 main program into three subroutines that handle initialization, execution, and finalization. These subroutines are used by the ESMF/NUOPC coupler that controls the wave component in the coupled run. During the simulation, the surface boundary fields are exchanged via subroutine calls by the WW3–ESMF interface, shown in Fig. 1.

L70-71: Not sure what do the authors mean here... Why online regridding is not needed? If online regridding is not needed, why implementing it?

The online regridding process is implemented based on ESMF coupler. It is one of the advantages of this coupling framework. In this work, we aim to present the implementation of the wave model and thus not evaluate the online regridding process. Now we have revised this sentence in Section 2:

It is noted that ESMF online re-gridding options are also implemented for the wave component when exchanging boundary fields, but it is not used in this work because we aim to present the implementation of wave components.

L73-74: Might be helpful to be specific on what inputs and outputs are included…

Now we have reorganized this section and presented the input/outputs first. Please see our revisions in Section 2.1.

L81: By "Langmuir turbulence parameters" do the authors mean "Langmuir number"?

The Langmuir turbulence parameters include the Langmuir number and the turbulence enhancement factor, which are explained in Section 2.3. Now we have added the parameters that we used to parameterize Langmuir turbulence:

Langmuir turbulence parameters (i.e., Langmuir number and enhancement factor)

L99-100: So in addition to the surface Stokes drift mentioned on L80, the integrated Stokes transport is also needed to approximate the Stokes drift profile, right? Also, the same authors (Breivik et al) have an updated method to approximate the Stokes drift profile (Breivik et al., 2016), essentially requiring the same information from WW3. Any comments on why not using this newer method?

In this paper, we aim to demonstrate our implementations of the coupled system. To this end, we used the equations proposed by Breivik to approximate the profiles of Stokes velocity. We agree with the reviewer that there are other methods (e.g., Breivik et al., 2016 and Romero et al., 2021) that can better represent the Stokes velocity. Discussing the impact of Stokes drift profiles is out of the scope of this work, but we will test the other options for approximating the Stokes profiles in future work:

There are also other options to better approximate the Stokes velocity profiles (Breivik et al., 2016 and Romero et al., 2021) that remain to be tested in future work.

L117: Might be helpful to write out the equation here rather than referring the readers to an equation in Li et al., 2019?

Now we have added the missing equations to Appendix A.

L117: "using" -> "uses"?

This text has been corrected.

L119: The entrainment flux is also affected by the enhanced turbulent velocity scale, right?

Yes. We agree with the reviewer that the entrainment flux is also affected although it is not directly parameterized in VR12-MA. Now we have revised this sentence to avoid confusion:

Different from VR12-MA, LF17 parameterized the entrainment fluxes due to Langmuir turbulence by revising the definition of the bulk Richardson number:

L126: Same as above, might be helpful to write out the equation here?

Now we have added the equations to Appendix A.

L147-148: Which other models are used in this study? Was there a comparison of different options?

Yes. We have tested all these models for this case study, but the characteristics of the tropical cyclone do not change significantly. The surface variables (e.g., 10-m wind speed and latent heat fluxes) are not sensitive to these options either. We have presented the equations used in the surface roughness parameterization options in Appendix B and the sensitivity analysis in Appendix C. The sensitivity analysis of other effects of surface waves (e.g., Stokes-Advection, Stokes-Corolis, and wave-induced momentum flux) are also included in Appendix C.

L173-L185: Are the boundary conditions of atmosphere, ocean and waves consistent with each other?

In this case, we did not downscale from a global coupled model. In this sense, the boundary conditions for the atmosphere, ocean, and wave are not consistent with each other. This is done because we want to initialize our regional model using the reanalysis data to study the physical processes of air–sea interactions in a setting closest to the observed world. In addition, it is not straightforward to initialize the WAVEWATCH III model from the bulk wave parameters of a global coupled model. However, given that the reanalyses represent the observed world, they should be reasonably consistent with each other and in balance.

L193: What do the authors mean by "derive skill from boundary conditions"

In this work, we are performing downscale analysis and downscaling the ocean, wave, and atmosphere from the global models. In each ensemble, the lateral boundary conditions are provided by the same global products that allow us to investigate the air–sea interactions within the computational domain. Now we have revised this sentence to avoid confusion:

We performed downscaled hindcasts in this work, which allows us to focus on the impacts of air–sea interactions during the tropical cyclone event by minimizing the boundary errors.

L208-209: I didn't follow this sentence.

Now we have revised this paragraph in Section 3.2:

When the effects of surface waves are considered in CPL.AOW, the model setup is as follows. The Stokes-Coriolis and the Stokes-Advection in Eq. (1) are considered; the impact of Langmuir turbulence is parameterized in the same way as Li et al., (2017); the ocean surface roughness is determined using the Charnock coefficient (CHNK) from WW3; the wind stress in the ocean model is treated as mentioned in Eq. (7). We have compared the coupled model with and without wave effects in Section 4, then we further illustrate the sensitivity of the coupled model to the wave effects in Section 5 and the Appendix.

Section 4: The purpose of this section is a bit confused. If the purpose is to validate the simulation results of the coupled model (which seems to be suggested by the section title "Results"), more details and discussions on the comparison among the three sets of simulations (CPL.AOW, CPL.AO, and ATM.DYN) seem appropriate. If the purpose is to provide a background information for the discussion on the wave effects, the authors might need to be explicit on that.

Thanks. Now we have changed the title of this section to “comparing coupled and uncoupled models”. We have also revised the first paragraph in Section 4 to clarify this:

In this section, the ensemble coupled simulation results (i.e., CPL.AOW and CPL.AO) are compared with the results from the uncoupled runs (i.e., ATM.DYN) to assess the performance of the models, the impact of coupled feedbacks, and the effect of the waves.

L250-251: Might be helpful to be specific on what is better and what is worse in CPL.AOW than CPL.AO.

Thanks. Now we have added more specific discussions to Section 4.1:

In summary, both CPL.AOW and CPL.AO runs better simulate the tropical cyclone characteristic than ATM.DYN in comparison with the IBTrACS data. CPL.AOW better simulates the minimum pressure and maximum wind speed than CPL.AO, but is outperformed by CPL.AO for the RMSEs throughout the event. CPL.AO also better simulates the track of the tropical cyclone.

L271: "Fig 5(b)" -> "Fig 5(c)"?

This text has been corrected.

Figure 4 caption: What do the black and red dots mean?

Now we have added the meaning of these dots in the caption:

The red dots indicate the ensemble-averaged locations of the center of tropical cyclones at the snapshot; the black dots indicate the ensemble-averaged locations of the center of tropical cyclones each day at 00 UTC.

L297: Switch the order of "cool the SST" and "deepen the MLD"?

Now we have switched the order of “cool the SST” and “deepen the MLD”.

L299: Nudging to what?

We have added this sentence in the first paragraph of Section 5:

By using spectral nudging, WRF nudges the model fields to NCEP FNL data.

L300-301: Perhaps more reasoning of why nudging is necessary here deserves more clarification.

We used spectral nudging because of the uncertainties of the atmosphere model. Although we performed a series of 20-member ensemble simulations, the standard error of the SST and MLD is still very large due to the uncertainties of the cyclone tracks. By restraining the larger scale features of the atmosphere, we are able to reduce the uncertainty of the atmosphere model and can highlight the impact of the Langmuir turbulence on the ocean. Now we have added the discussion of this to Section 5:

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 Langmuir turbulence on the ocean.

L318-321 and L325-326: Do the authors mean that the vertical mixing of momentum is too much in VR12-MA, which reduces the vertical gradient of ocean current and reduces vertical mixing of tracers like temperature? It is not clear to me why an enhanced vertical mixing of momentum coexists with a reduction in vertical mixing of temperature. It might be helpful to elaborate on why this is the case.

When VR12-MA is applied in the coupled model, the KPP diffusion coefficient increases (multiplied by the enhancement factor in Eq. (3)). Then the horizontal diffusion increases and reduces the vertical gradient of ocean current. Because vertical current velocity is used in the (Ur – U) term Eq. (5) to determine the Richardson number, a smaller velocity gradient increases the Richardson number and shallows the ocean mixing layer in the model. When the ocean mixing layer is shallower, the vertical mixing of the ocean is reduced in VR12-MA and SST cooling becomes weaker. The vertical profiles shown in Fig. 11 aims to demonstrate the velocity profiles and current velocity to help explain this.

For the temperature differences shown in Figs. 9 and 10, we hypothesize that in this case ocean mixing layer gets shallower and thus reduces the vertical mixing of temperature. Now we have revised our manuscript and added more discussions on this:

When VR12-MA is applied, the Langmuir enhancement factor (see Eq. (3)) is used to amplify the KPP diffusivity term (see Eq. (5)). This reduces the vertical gradient of horizontal current, shown in Fig. 11(d). When the velocity gradient is reduced in VR12-MA, the |Ur – U| term in Eq. (5) decreases, and thus the Richardson number increases. This Richardson number increase results in a decrease in estimated MLD, and thus the SST cooling is weaker than the simulation without Langmuir turbulence (NoLT).

Figure 9, 10: The results of VR12-MA are not intuitive to me. According to Section 2.3, VR12-MA also includes the effects of Langmuir turbulence on enhancing the vertical mixing. Then why the MLD gets shallower and SST gets warmer along the cyclone track than the case without Langmuir turbulence? Are the snapshot plotted at the time indicated by the red dot?

Yes. These results are plotted at the time indicated by the red dot. While we agree this is non-intuitive, we have diagnosed the processes taking place with the implementation of VR12-MA and gained understanding. Now we have revised the manuscript and clarified the cause:

To analyze the impact of different Langmuir turbulence options, in Fig. 11(a-d) we plotted the domain-averaged Richardson number, buoyancy difference, vertical density gradient, and velocity, which are the dominant terms in Eq. (6). The Richardson number is plotted because it is used in the MITgcm KPP scheme to determine the boundary layer depth, which is crucial to parameterize vertical mixing. The threshold of critical Richardson number is 0.3, meaning the ocean is assumed dynamically unstable and turbulent when $Ri<0.3$.

It can be seen that when VR12-MA is applied, the Richardson number increases compared with NoLT (no Langmuir turbulence), indicating the parameterized turbulence is getting weaker. Examining each component of the Richardson number in Eq. (6), it can be seen that buoyancy and vertical density gradient terms do not change significantly, shown in Fig. 11(b) and 11(c); while the changes of horizontal current speed can be seen in Fig. 11(d) near the surface.

When VR12-MA is applied, the Langmuir enhancement factor (see Eq. (3)) is used to amplify the KPP diffusivity term (see Eq. (5)). This reduces the vertical gradient of horizontal current, shown in Fig. 11(d). When the velocity gradient is reduced in VR12-MA, the $|Ur – U|$ term in Eq. (5) decreases, and thus the Richardson number increases. This Richardson number increase results in a decrease in estimated MLD, and thus the SST cooling is weaker than the simulation without Langmuir turbulence (NoLT).

As we mentioned in our previous reply, to illustrate the impact of the changes of velocity shear, we performed another experiment by not enhancing the diffusion of the velocity in KPP (i.e. leaving epsilon constant in Eq. 3). This new simulation is shown in the figure below as VR12-MA-NoU. In Fig. 1 of this reply, the MLD deepens by about 20 m and SST cools down by about 0.5 degree along the wake of the tropical cyclone. This is indicating that, when enhancing the diffusion of velocity in VR12-MA, it shoals the boundary layer and reduces the vertical mixing. We have added this figure and the discussions to Appendix D:

To verify this, we run an ensemble of simulations (CPL.VR12-MA-NoU) that do not enhance KPP diffusivity for horizontal currents, then we observed cooler SST due to enhanced mixing. The simulation results of this verification test are detailed in Appendix D.

Figure 11: Why the mixed layer depth (dashed lines) is different in panel (d) from other panels?

Now we have re-plotted Figure 11 and improved its quality.

References

Breivik, Ø., J.-R. Bidlot, and P. A. E. M. Janssen, 2016: A Stokes drift approximation based on the Phillips spectrum. Ocean Modelling, 100, 49–56, https://doi.org/10.1016/j.ocemod.2016.01.005.