Reviewer 1:

General comment:
The authors addressed well my comments in the first round of revisions. The text in the manuscript have also been improved. I still believe that this paper would be a good fit for GMD. I only have one minor concern that rose from one of the author's responses.

Minor comments:
You mentioned the different switches used to compile WW3, one of them concerns the correction of the wind using the ocean current (RWND). I assume the wind in WRF to be the relative wind since ocean currents are prescribed from the ocean model, so the WRF output should be relative wind as well. Thus, the wind sent to WW3 should already be the relative wind, yet by using this switch in WW3 you would correct the wind again, wouldn't that be double counting the ocean current to correct the wind, first in WRF and a second time in WW3? This remark could apply also to the MITgcm as at line 97 you mentioned the wind is corrected in the ocean model as well. Could you clarify this point?

Reply 1: We thank the reviewer for pointing out this issue in our work. We have carefully examined the WRF code and literature, then we found that WRF outputs the relative 10-m wind speed. We have re-compiled the code (removed the RWND switch in WW3) and re-ran all the simulations. Now we have revised the manuscript to clarify this:

The wind speed sent to WW3 and MITgcm is the relative 10-m wind speed from WRF based on the Monin-Obukhov similarity theory (Monin and Obukhov, 1954; Renault et al., 2020), then WW3 and MITgcm use the relative 10-m wind speed without correcting the current velocity in the simulations.

Due to the minor difference between the old and new simulation results, we have also updated all figures and some texts describing the simulation results. For example, we moved the comparison with drifter data to the appendix because the SST differences are not significantly different at the location of the drifters (and they were not significantly different from our previous version). Please refer to our annotated manuscript.

Reviewer 2:

I think this revised version of the manuscript has been improved in clarity as compared to the original version. However, I'm still not quite satisfied with the authors' response to my two major concerns in my previous review.

1. I still feel that the inclusion of the uncoupled WRF run in the comparison is a bit confusing and distracting. Since the focus is on the effects of including waves in SKRIPS (based on the title, abstract and the two goals described in the introduction), it may make more sense to start from the atmosphere-ocean coupled version of SKRIPS. The standalone WRF simulation may be used as a reference. But it would be good to focus on the comparison between CPL.AO and CPL.AOW in the main results and conclusions. If the authors also want to highlight the changes due to air-sea coupling (as mentioned in their response to my previous comments), it might be helpful to discuss in more detail what is the role of air-sea coupling here. But I think such discussion may be quite distracting.

Reply 2: We thank the reviewer for pointing this out. In our work, we implemented the coupled model with wave components. Natural experiments to examine the newly implemented model are to compare it both with coupled models without waves and also to uncoupled models. This is typical in other studies demonstrating coupled models with newly implemented wave components (e.g., Warner et al., 2010; Chen et al., 2013). The uncoupled model serves as a useful benchmark. We have revised our manuscript to clarify this, as detailed in our Reply 5.

2. The added explanation of the unintuitive results of VR12-MA in Section 5.2 is not satisfactory. In fact, the argument of reduced velocity shear due to enhanced diffusivity of momentum also applies to LF17 -- the same Langmuir enhancement factor in VR12-MA is applied in LF17 as well. So the increased bulk Richardson number due to reduced |u_r-u|^2 should also occur in LF17 -- indeed in Fig 11(d) the same reduction in surface velocity shear can be seen for LF17 too.

Reply 3: We agree with the reviewer that the same arguments apply to LF17 for the |Ur – U| term. However, in Eqs. (7) and (8), the Richardson number in LF17 included the $V_{tl}(z)$ term to account for the effects of entrainment flux. When the entrainment flux is considered in LF17, it enhances the mixing and reduces the Richardson number, shown in Fig. 11(a). Although the near-surface velocity is also reduced in LF17, the mixing layer deepens and SST gets colder than in VR12-MA and NoLT. Now we have revised our discussion in Section 5 to clarify this:

On the other hand, when using LF17 in the simulations, the same enhancement factor as VR12-MA is added, but the term $V_{tl}(z)$ is used in Eq. (8) for parameterizing the Richardson number. Although the velocity gradient |Ur – U| is also smaller, shown in Fig. 11(d), the entrainment flux $V_{tl}(z)$ decreases the Richardson number. This implies stronger vertical mixing due to the Langmuir entrainment by the tropical cyclone. Hence the SST cooling in the near wake region of the tropical cyclone is stronger when LF17 is used than VR12-MA. This shows parameterizing the Langmuir turbulence using LF17 gives more realistic results than VR12-MA.

I think the suggestion from the Community Comments 1 about the inconsistency of the Lagrangian versus Eulerian currents in the momentum equation and Langmuir turbulence parameterization may explain this unintuitive result of VR12-MA. In both Li et al., 2016 and Li et al., 2019, VR12-MA (also LF17) was not used together with Stokes Coriolis. Their assumption (see the notes in Table 1 of Li et al., 2019) was that the simulated velocity is Lagrangian so implicitly it is the Lagrangian shear that is used to compute |u_r-u|^2 in KPP. Since the vertical shear of Stokes drift is strongest near the surface and often roughly aligns with the wind, the Eulerian velocity shear is much reduced when Stokes-Coriolis force is included in the momentum equation. This is consistent with what the authors saw in their simulations in Fig. 11(d) with reduced surface velocity shear in both VR12-MA and LF17 as compared to NoLT. I'd suggest the authors run two more simulations without Stokes-Coriolis and Stokes-advection terms (i.e., only Langmuir turbulence parameterization of VR12-MA and LF17). I guess one may see deeper MLD and cooler SST along the cyclone track in both cases, but more MLD deepening and SST cooling in LF17 than shown here.

Reply 4: Now we have added the experiments using NoLT, VR12-MA, LF17, and LF17-ST without using Stokes-Coriolis and Stokes-advection in Appendix C. This is consistent with the implicit option when simulating coupled ocean-wave-atmosphere interactions in Li et al., 2016 paper. We have found that the simulation results do not change significantly, as shown in Fig. 1 below. It can be seen that the SST changes are similar to what is shown in Fig. 8 of the manuscript, except for the region near the track of the tropical cyclone where the uncertainty is large. For Panels (d-f) when spectral nudging is applied, similar patterns of SST cooling and warming are captured when the Stokes forces are implicitly considered.

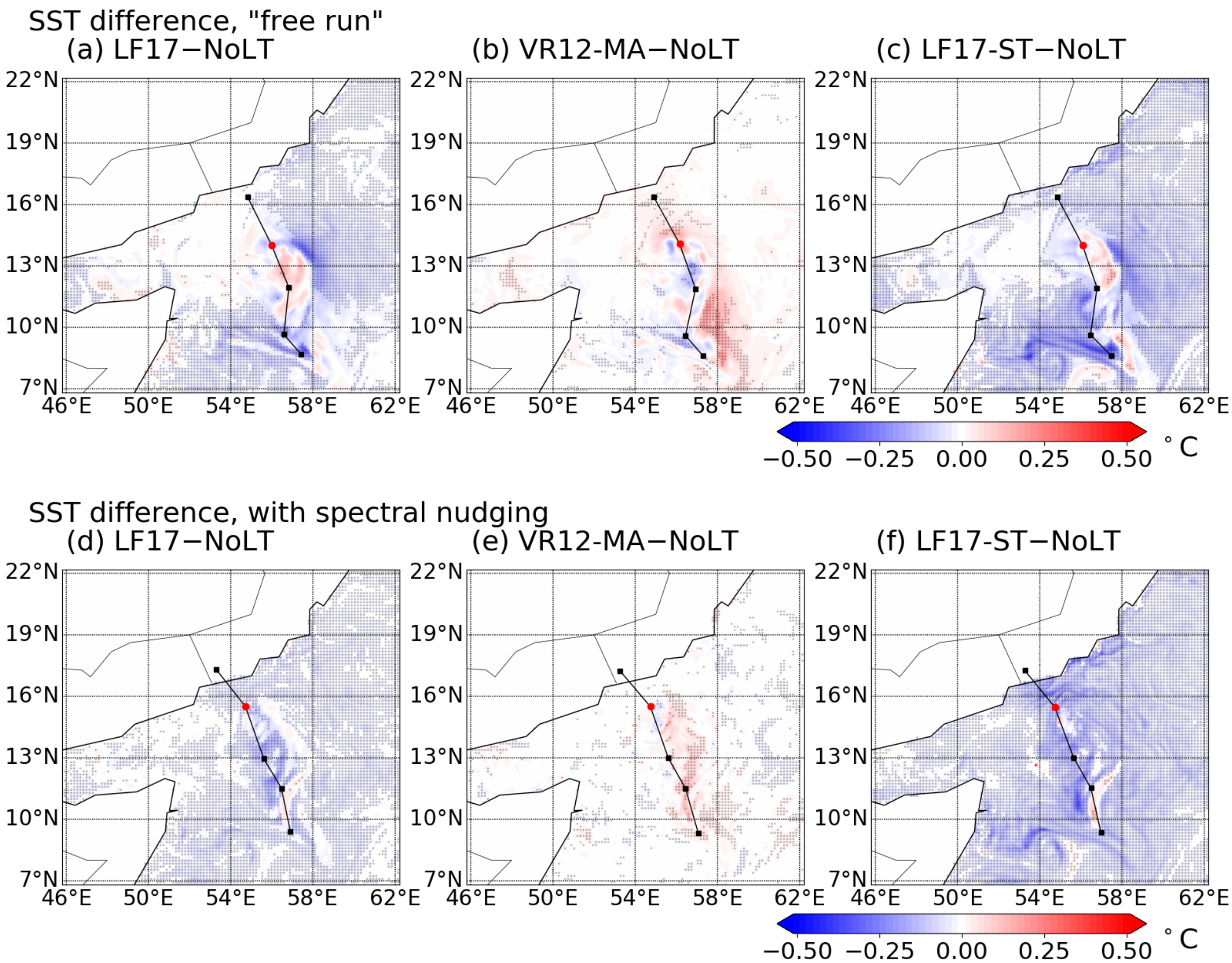

Figure 1. The snapshot of the ensemble-averaged SST and MLD difference. Panels (a-c) show the SST difference between the simulations with Langmuir turbulence (CPL.LF17.IMP, CPL.VR12-MA.IMP, CPL.LF17-ST.IMP) and without Langmuir turbulence (CPL.NoLT.IMP). Panels (d-e) show the SST difference for the simulations with spectral nudging. The markers indicate the regions where the SST difference is significant ($P < 0.05$).

However, the investigation of potential inconsistencies between Eulerian and Lagrangian implementations of the numerics is beyond the scope of this paper. This paper aims to present the implementations of the coupled model and we offer two options: (1) explicit scheme for Stokes forces (similar to Wu et al., 2019); and (2) implicit scheme (similar to Li et al., 2016). We summarized the simulation results using the implicit option in Appendix C and added the following sentence in our manuscript in Section 3.2:

It is noted that when the Stokes-Coriolis and the Stokes-Advection are not explicitly considered in the experiments, the model setups are consistent with Li et al. (2016), assuming the simulated velocity is Lagrangian.

Specific comments:

L8-10: Given that the focus of this study is on including waves in SKRIPS (suggested by the title), I still feel it is not necessary and is really confusing to put any emphasize on the difference between coupled model and standalone WRF. Perhaps rephrase to use the difference between the stand-alone WRF and ocean-atmosphere coupled simulations as a reference to describe how large the impact of including surface waves in these experiment is?

Reply 5: In this work, we developed the coupled model with wave components, then we evaluate the model skill of the coupled model without wave and the uncoupled model. Now we have revised this sentence in the abstract by emphasizing the uncoupled model is used as a benchmark:

We found that the characteristics of the tropical cyclone are not significantly different due to the effect of surface waves when using different parameterizations, but the coupled models better capture the minimum pressure and maximum wind speed compared with the benchmark stand-alone WRF model.

We have also revised the description of the stand-alone simulation in Section 3.2:

Compared with CPL.AO and CPL.AOW, this run serves as a benchmark that aims to demonstrate the impact of waves and coupled air–sea interactions on the simulation results.

L49-54: These discussions seem to divert the purpose of this manuscript to test the coupled model? I think it is already widely accepted that accurately modeling tropical cyclones is important but challenging.

Reply 6: These discussions aim to explain why we select the Arabian Sea in our case. Now we have revised it:

The Arabian Sea is investigated in this work because of its rich and diverse ecosystem, its economic impact on the surrounding countries, and its important role in international trade. Continued climate warming is expected to further amplify the risk of cyclones in the Arabian Sea (Dube et al., 1997; Evan et al., 2011; Evan and Camargo, 2011) and increase socio-economic implications for coastal communities in that region (Henderson-Sellers et al., 1998; Murakami et al., 2017; Bhatia et al., 2018).

L80-84: I understand that the added text here is to address one of the reviewer's comments. But it seems a bit confusing and distracting. Perhaps rephrase? For example, by "overestimate the strength of surface currents" on L81, do the authors mean overestimating the currents that are dynamically important for waves in WW3, which shouldn't be the surface currents?

Reply 7: Our implementations of the currents are consistent with the COAWST model (Warner et al., 2008, 2010), but not consistent with the coupled model used by Fan et al. (2009). Therefore, we tried to clarify this in our manuscript. Now we have revised this sentence:

We used the surface current based on previous literature (Warner et al., 2008, 2010, Couvelard et al., 2020), but this may overestimate the strength of surface currents impacting the wave model, as suggested by Fan et al., (2009), who used the current velocity at L/4\pi (L is the mean wavelength).

L89-90: What do the authors mean by "surface Stokes drift forces"?

Reply 8: Now we have fixed the typo in this sentence:

The Stokes forces, the Langmuir turbulence parameters, and the momentum fluxes are detailed in Sections 2.2, 2.3, and 2.4, respectively.

L95: What do the authors mean by "surface boundary fields"?

Reply 9: The surface boundary fields are mentioned in the first and second paragraphs in the same section (Line 73-80). Fig 1 also shows the surface boundary fields sent/received by WW3. Now we revised this sentence:

During the simulation, WW3 receives and sends boundary fields via subroutine calls by the WW3--ESMF interface, shown in Fig. 1.

L100-102: So the three components are all on the same grid? Perhaps mention briefly the possible uses in the future?

Reply 10: Yes. We are currently working on a higher-resolution ocean model with our collaborators. The possible uses in the future are added:
The online re-gridding option will be used when using a higher resolution ocean model for the Arabian Sea operational model.

L116: By using Breivik et al., 2014, it means the total Stokes transport (vertically integrated Stokes drift) is also needed to be passed from WW3 to MITgcm to reconstruct the Stokes drift profile, not only the surface Stokes drift as mentioned on L77?

Reply 11: In the simulations we only used the surface Stokes drift. The total Stokes transport is not sent to MITgcm because we sent the wavenumber k_m in Eq. (8) to compute the Stoke drift profiles.

L115-121: It is not entirely clear from this discussion why approximation is necessary for the Stokes drift profile as it can be computed in WW3. The problem is passing the Stokes drift profile from WW3 to MITgcm, right?

Reply 12: The Stokes drift profile can be computed in WW3, but it is not the standard output. WW3 only outputs TUS (Stokes volume transport) and USS (surface Stokes drift) and therefore we used the Breivik et al. (2014) to approximate the Stokes drift in the coupled simulation. This is similar to the recent work performed by Couvelard et al. (2020).

Theoretically, ESMF can send 3D Stokes drift profiles from WW3 to MITgcm, but it may also significantly increase the computational cost in the coupling process.

L127-132: I didn't follow the reasoning here…

Reply 13: In this paragraph, we tried to explain why we revised KPP in our work. KPP does not explicitly consider the effect of Stokes shear force in Eq. (1), but it is tuned from observation data and may implicity incorporate some effects of Langmuir turbulence. Because of this, Li et al. (2016) and many others tries to modify the KPP parameterization to reduce the error due to the effect of Langmuir turbulence.

Now we have revised this paragraph to clarify this:

Considering the effect of the surface waves, the Stokes drift provides a source of the turbulent kinetic energy (TKE) through the vortex force and modified pressure (Craik and Leibovich, 1976), or more cleanly the Stokes shear force (Suzuki and Fox-Kemper, 2016) as mentioned in Eq. (1). Evidence of this enhanced vertical mixing has been documented from observations and large-eddy simulations (McWilliams et al., 1998; D'Asaro, 2001; Van Roekel et al., 2012). In this work, we aim to implement the Stokes shear force in the coupled model and investigate its effect on the coupled system. Although it is not explicitly accounted for in KPP (K-profile parameterization, Large et al., 1994), KPP might have implicitly incorporated some effects by tuning the parameters to ocean observations (Reichl et al., 2016). Our implemented model is about 8 km resolution (0.075 deg), and the horizontal gradients of the Stokes drift are several orders of magnitude smaller than vertical gradients. Following Suzuki and Fox-Kemper (2016) we only consider the effects of Stokes shear force due to Langmuir turbulence because of this scale separation.

L133-134: Didn't follow this sentence as well…

Reply 14: This sentence is revised:

Although there are many unknowns about the role of Langmuir mixing in ocean modeling, there exists many parameterizations that aim to represent these processes and alleviate model bias.

L137: It is not very clear what "based on the waves" refer to?

Reply 15: We add this sentence to demonstrate the differences between VR12-MA/LF17 and LF17-ST. The first two options (VR12-MA/LF17) are parameterized based on wave state; the last option (LF17-ST) is parameterized based on the surface winds. The specific parameters used to parameterize the Langmuir turbulence are detailed in the latter paragraphs.

Now we have revised this paragraph to clarify the differences between VR12-MA/LF17 and LF17-ST:

Both VR12-MA and LF17 parameterize the Langmuir turbulence based on the parameters computed from WW3: in VR12-MA the KPP turbulent velocity scale is multiplied by an enhancement factor; in LF17 the KPP turbulent velocity scale is treated in the same way as VR12-MA, and the entrainment buoyancy flux is also considered. On the other hand, LF17-ST parameterizes the Langmuir turbulence similarly to LF17, but parameters are computed using the 10-m winds instead of using the output from WW3.

L143-144: Not clear why this case is necessary to validate LF17

Reply 16: First we use LF17-ST because it has been implemented and validated in MITgcm by Schultz et al. (2020). In comparison with LF17-ST, LF17 uses a similar approach to determine the critical Richardson number in KPP, but uses different physics inputs from surface winds. Because of the similarities between the two options and the work done by Schultz et al. (2020), we decided to validate LF17 using the implemented LF17-ST option. Now we have revised this sentence:

We also used the well-validated LF17-ST implementation by Schultz et al. (2020) to validate LF17 in the coupled simulations due to the similarity of these two options.

L144-145: True only if it works…

Reply 17: This LF17-ST option has been implemented in MITgcm (Schultz et al., 2020) to investigate the Langmuir turbulence. In our work, we inherited their code but did not run any individual tests only using LF17-ST. Now we have made some minor changes to this sentence:

Because LF17-ST does not need bulk wave parameters as input, it can be also used in uncoupled MITgcm simulations (Schultz et al., 2020) or coupled simulations without waves to parameterize the Langmuir turbulence.

L186: Not the appropriate citation for computing the wind stress?

Reply 18: The equations and parameters to compute the wind stress in MITgcm follow Eq. (6a) in Large and Yeager, 2004. The formulation was from the unpublished work from E.E. Vera in 1983, but this document is not citable.

L417-421: These same arguments apply to LF17 as well -- in LF17 the same Langmuir enhancement factor is applied to amplify the KPP diffusivity term. In fact, the same reduction in near surface velocity shear can be seen for LF17 in Fig. 11(d).

Reply 19: We have replied to this comment in our Reply 3 and 4. Now I am attaching a part of our reply 3 for this specific comment.

We agree with the reviewer that the same arguments apply to LF17 for the |Ur – U| term. However, in Eqs. (7) and (8), the Richardson number in LF17 includes the $V_{tl}(z)$ term to account for the effects of entrainment flux. When the entrainment flux is considered in LF17, it enhances the mixing and reduces the Richardson number, shown in Fig. 11(a). Although the near-surface velocity is also reduced in LF17, the mixing layer deepens and SST gets colder than in VR12-MA and NoLT. Now we have revised our discussion in Section 5 to clarify this:

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