Review Comments 3

The manuscript described the integration of the WW3 wave model into the SKRIPS couple framework. The cyclone Mekunu is chosen as study case in order to validate and compare a set of coupled and uncoupled simulations. Moreover, different Langmuir parametrizations have been implemented in order to investigate the impact of the wave to ocean coupling with an emphasis on SST and mixed layer depth analysis. Different surface roughness parametrizations are also compared to estimate the impact of waves in the coupled system. The simulations are validated against observations of the cyclone's characteristics and SST from drifters. Overall, the coupled simulations show better skill in reproducing the cyclonic event. The sensitivity to wave coupling on the characteristics of the cyclone is not found to be significant. However, the impact on SST and mixed layer is shown with a decrease of 0.5 degC and a deepening of the mixed layer of up to 20m in the wake of the cyclone compare to the simulation without Langmuir turbulence. Although, one of the parameterizations, VR12-MA, showed counterintuitive results with weaker SST cooling and shallower MLD. This is explained looking at vertical profiles showing a reduced horizontal shear velocity when using that specific scheme.

General comments:

Overall this is a good paper and well written. Although some of the results of the study are well known (i.e. coupled model generally have better skill in modeling cyclone events) I believe the emphasis is also put on the technical implementation of the wave model into the SKRIPS framework and of the different Langmuir and roughness parameterizations. For that reason, I believe it would be a good fit for GMD journal. However, one of the major issues in my opinion is that the manuscript needs more clarity on certain aspects and more discussion of the results which would be beneficial to the overall paper. As well as maybe a reorganization/clarification between sections 4 and 5 where the goals are sometimes unclear or confusing whether it aims to compare coupled and uncoupled, the impact of the different parametrizations or some validation of background ocean state. Some findings of the study are really interesting but often times too briefly discussed or even some figures not discussed at all. Also, as an optional comment the discussion on the appendices A and C on the roughness parametrizations may fit well in the main text.

We thank the reviewer for the comments that acknowledge our manuscript. This manuscript aims to demonstrate the development of the coupled model and we tried not to add many too discussions on the physical insights in our initial submission. Now we have added more discussions on the physical insights and the SST trend in the sensitivity analysis.

Hereafter are the details of the specific's comments.

Specific comments:

Line 80: It could be useful to reference the section 2.4 when mentioning the "momentum flux terms due to waves", it feels a bit too vague otherwise.

We thank the reviewer for pointing out this. Now we have revised Section 2.1 in the manuscript:

WW3 sends the wave variables to ESMF, including the (1) bulk wave parameters (i.e., significant wave height, peak wavelength and mean wavenumber), (2) Surface Stokes drift, (3) momentum flux terms due to surface waves, and (4) Langmuir turbulence parameters (i.e., Langmuir number and enhancement factor). The details of these wave variables are summarized in the latter sections.

Line 86: Maybe mention here that details of the calculation of the momentum stress is given later on, otherwise one can wonder why you're not mentioning it here.

Now we have added this to the end of Section 2.1:

The implementations of the wave effects are discussed in the latter sections. The surface Stokes drift forces, the Langmuir turbulence parameters, and the momentum fluxes are detailed in Sections 2.2, 2.3, and 2.4, respectively. The sea surface roughness parameterizations are summarized in Section 2.5.

Section 2.4: Mention that the momentum terms are output from WW3. Is the total air-side stress calculated inside MITgcm? If yes, why don't send the total air-side stress calculated within WRF which was calculated using wave information? And thus what kind of bulk formulae is used in MITgcm to calculate the air-side stress (line 83-84)? (similar remark can be made for heat flux). I think that a bit more clarity on how the surface flux are calculated and exchanged could be useful.

Yes. In the coupled model, we add the momentum terms from WW3 to those calculated from MITgcm. We mentioned that we are using the MITgcm momentum fluxes in our previous work (Sun et al. 2019). The heat fluxes and freshwater fluxes are calculated within WRF. We don't use the momentum fluxes in WRF directly because the WRF model does not directly output the momentum flux or friction velocity terms. Now we have added the following text to our manuscript to clarify this:

In the coupled model, tau_a is calculated in MITgcm (Large and Yeager, 2004) because WRF does not directly output the momentum flux terms. The parts that go into wave growth tau_aw and wave breaking tau_ow are calculated in WW3.

Line 147: In which scheme did you implemented these parametrizations ? Aren't some of these already available in WRF?

We implemented all these parameterizations for the MYNN scheme. They are not already available in WRF but are implemented in COAWST (Olabarrieta et al., 2012). Now we have clarified this in our manuscript:

We have also implemented a few other ocean roughness closure models that have been used in COAWST and discussed in Olabarrieta et al. (2012)...We implemented these options in the WRF Mellor--Yamada--Nakanishi--Niino (MYNN) surface layer scheme.

Line 147:150: Since you implemented several roughness parametrizations, any reasons why you didn't implemented COARE3.5 which is more recent and as also formulation of surface roughness based on sea state variables ?

We actually implemented everything available for COARE 3.5, but for the specific WRF version (v 4.1.3) we are using, COARE 3.0 is the default option and COARE 3.5 is commented out in the source code. Because of this, we used the default option COARE 3.0 in our experiments, aiming to represent our coupled model.

In Appendix A, line 373-374: please describe this options using the Charnock from WW3.

Now we have added the Charnock options that we used for WW3. We have also added the other physics options in WW3 to the manuscript:

In the present study, we used the ST4 option in WW3 and thus the Charnock coefficients are calculated based on Ardhuin et al. (2010). In this manuscript we used WAVEWATCH III version 6.0.7 compiled with the following switches:

F90 NOGRB NOPA LRB4 SCRIP SCRIPNC NC4 TRKNC DIST MPI PR3 UQ FLX0 LN1 ST4 STAB0 NL1 BT4 DB1 MLIM TR0 BS0 IC2 IS2 REF1 IG0 XX0 WNT2 WNX1 RWND CRT1 CRX1 TIDE O0 O1 O2 O2a O2b O2c O3 O4 O5 O6 O7

Table 1: Is the surface layer scheme used in WRF the same as PBL, MYNN?

Yes. We are using the MYNN scheme for the surface layer. Now we have added it to Table 1.

Line 207: Is a coupled frequency of 120 seconds necessary? An Hourly coupling or 30 min coupling would allow to capture the diurnal cycle as well, any reason behind the choice of the coupling frequency? Also wondering if the computation time is impacted by this high coupling frequency ?

The coupling process takes less than 5% of the total computational resources when using the ESMF coupler in our case (Sun et al., 2019, Table 4). In our previous work (Seo et al., 2014: https://doi.org/10.1175/JCLI-D-14-00141.1), we found that using a smaller coupling interval can better resolve the diurnal variation of SST. We used a 2-minute coupling frequency because ESMF allows us to exchange the air–sea fluxes without significantly increasing the computational costs.

Line 210-211: In line with my previous comment, I found it not clear how the surface stress is calculated. On Figure 1, WW3 send the surface roughness to WRF to get the stress but here it is mentioned that the Charnock coefficient is used. Is it actually the Charnock coefficient that is sent to WRF or the roughness length? Please clarify and modify Figure 1 if necessary.

We thank the reviewer for pointing out this in our figure and our text. In WRF the surface roughness height Z0 is used for the surface stress. In DGHQ, TY2001, and OOST, Z0 is calculated based on bulk wave parameters (e.g., significant wave height, peak wavelength, and mean wavenumber). When we use the Charnock coefficients from WW3 (e.g., CHNK), we replace the Charnock coefficients used to compute Z0 in WRF. WW3 is not sending the surface roughness length directly to WRF. We have revised this Figure 1 and revised our text in Sections 2.5 and 3.2.

Section 2.5:

When coupled with WW3 we parameterize the surface roughness based on the Charnock coefficient calculated from WW3 to make the surface roughness consistent. We have also implemented a few other ocean roughness closure models that have been used in COAWST: DGHQ (which is based on wave age), TY2001 (which is based on wave steepness), and OOST (which considers both the effects of wave age and steepness). These models parameterize Z0 using the bulk wave parameters from WW3. More detailed descriptions of these closure models and sensitivity analysis are presented in Appendices B and C.

Section 3.2:

When the effects of the surface waves are considered in CPL.AOW, the model setup is as follows. The Langmuir turbulence is parameterized in the same way as Li et al., 2017; the Stokes-Coriolis and the Stokes-Advection in Eq. (1) are considered; the ocean surface roughness is parameterized using the Charnock coefficient (CHNK) from WW3…

Section 3.2: What are the models frequency outputs (if different from one model to another)? It would be useful to add it here.

The models output the simulation results every three hours. In this work we set the same output frequency, but the output interval is flexible when using the SKRIPS model. Users can output the simulation results at different intervals for each model component. We have added the model output interval in Section 3.2:

We output the results every three hours to demonstrate the evolution of the tropical cyclone simulated by the coupled model.

Section 4: The name of the section, “Results”, is fairly generic. The goal of this section needs some clarification, it includes some results between coupled simulation and some more validation part, i.e. section 4.2 or 4.3 which resemble more to a validation of the wave field and no impact of waves on air-sea interaction or between simulations are discussed yet.

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. Section 5 details the sensitivity analysis of Langmuir turbulence; Appendix C presents the sensitivity analysis of other effects of the waves.

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 present the impact all the ocean/atmosphere variables obtained in main text.

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.

Line 250: Any hypothesis as why the CPL.AOW would give “worse” results than the CPL.AO ?

We agree that the CPL.AOW does not show better results compared with CPL.AO. The two sets of coupled runs show similar results and we have examined the contribution of different components of wave effect in Section 5 and the appendix. It is maybe because the CPL.AOW considers the effect of Langmuir turbulence that strengthens the SST cooling, and thus reduces the intensity of the cyclone. But generally speaking the differences between CPL.AOW and CPL.AO are smaller than the standard deviation of the results . Now we have added more specific discussions to the revised manuscript:

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… The differences between the tropical cyclones simulated in the coupled and uncoupled simulations are associated with the SST cooling in the simulations, which are further discussed in Section 4.2. The differences between CPL.AOW and CPL.AO are further investigated in Section 5.

Line 271: “Fig. 5(c)”

Now we have corrected this text.

Line 271: Is this a known cold bias from HYCOM? Any reason as why HYCOM would show stronger SST cooling?

The bias from HYCOM/NCODA data is unknown in this region and this specific case due to the lack of in-situ observations, although it has small bias compared with other observational data. In this case study the SST cooling in HYCOM is stronger than the coupled model, and thus the simulation results of the tropical cyclone using this colder SST will be different. This section aims to explain the difference between CPL.AOW and ATM.DYN in simulating the tropical cyclone. We have revised our manuscript here to clarify this:

It can be seen in Fig. 5 that the SST cooling in CPL.AOW is weaker than that in HYCOM, indicating the SST is warmer throughout the simulation in CPL.AOW. Contributed by the warmer SST, the intensity of the tropical cyclone is also stronger in CPL.AOW than ATM.DYN. Due to chaotic nature of the atmosphere, it is still unknown why the warmer SST in CPL.AOW improves the simulation of tropical cyclone characteristics. In addition, although we compared the simulation results using available data, the lack of in-situ observations in this region makes it challenging to validate the SST used the simulations.

Line 272:273: Does this mean it is in agreement with some observations? Please clarify this statement.

For the results obtained for the Arabian Gulf and the Gulf of Aden, we only compared our simulation results with the HYCOM/NCODA data, but not with the in-situ observations. Now we have revised this sentence to avoid confusion:

It should be noted that CPL.AOW also captures the SST warming in the Arabian Gulf and the Gulf of Aden compared with the HYCOM analysis data.

Section 4.2: the results of the MLD differences in HYCOM showed in Figure 6c are not discussed, such as any hypothesis as why HYCOM would show shallower MLD while stronger cooling. Overall this section could benefit from more explanation of the figures and results showed.

It is not straightforward to discuss the differences in SST and MLD from two different models. The models are different and the surface heat fluxes are different. HYCOM/NCODA also assimilates observations and makes the comparison more difficult. Because the goal of our paper is to present the coupled model, investigating the difference is out of our scope. We have added our hypothesis to the manuscript:

It is noted that CPL.AOW has stronger MLD deepening than HYCOM, but weaker SST cooling. We hypothesize that this is because (1) the parameterization of the ocean mixing layer is different when the effects of Langmuir turbulence are considered in CPL.AOW; (2) the atmosphere forcing used in the coupled model has a higher spatial and temporal resolution that makes the SST and MLD different.

Figure 7: Caption does not match the actual figure, please clarify.

Now we have fixed the caption for Fig. 7:

Snapshots of the significant wave height Hs at 00 UTC May 22 24, and 26, 2018. Panel (a-c) show the ensemble averaged Hs obtained from CPL.AOW; Panel (d-f) show the standard deviation of Hs of the ensembles from CPL.AOW. The 15~m/s contour of wind speed is used to highlight the location of the tropical cyclone.

Line 284: You probably meant Figure 7b or Figure 7c.

Now we have corrected this in our manuscript. We have also gone through the manuscript and fixed all issues with the index of the figures.

Line 291: Please precise Appendix C.

Now it is corrected.

Line 304: Please precise Appendix B. Also, in the text Appendix C is cited before Appendix B so maybe the order of the appendices could be revised.

We have have fixed this in the manuscript. We have also revised the appendices and changed their order.

Line 389: “which are”

We have revised this paragraph and removed this sentence.

Line 391: Please be more explicit than “CPL” here as on the figure they all have different names and none is CPL

Now we have replaced “CPL” using “CPL.AOW (CHNK)”.

Line 395-396: Please specify the region, as in the Figure A4 it looks like there are some regions where the wind speed and the latent heat flux are weaker compared to CPL.CHAR.

Now we have revised this sentence:

It can be seen that the 10-m wind speed and latent heat loss are different when using TY2001, DGHQ, and OOST in comparison with using CHNK in CPL.AOW. However, the differences are not significant from the t-test (regions with $P < 0.05$ are highlighted), and analyzing these differences remains to be a future work.

Figure A4: “parameterization”; Also what do you mean by “without parameterization”, isn’t CPL.CHAR parameterized using the Charnock coefficient from WW3 ?

We have revised the caption of Fig. A4:

Panels (a-c) show the 10-m wind speed difference between the simulations using different options to parameterize the surface roughness (TY2001, DGHQ, and OOST) compared with CPL.AOW (CHNK).

Section 5.1: The Figure 8c showing HYCOM SST compared to drifters is not discussed although it looks like the cooling along the track is the closest compare to the drifters, is that right? However, in Section 4.2 it seems like the SST cooling in HYCOM was too strong which was one of the reasons leading to a too low intensity of the cyclone simulated ATM.DYN, please clarify or comment on that. Here again, this is interesting results and a bit more discussion on these findings could be beneficial.

Yes. The HYCOM/NCODA SST under the cyclone track is closer to the drifters than the simulations. We have revised Section 4.2 (shown in our previous replies) and added the following text in Section 5.1 to clarify this:

It is noted that the SST cooling in HYCOM/NCODA data is 3.23 degrees, which is closer to the drifter data than the coupled simulations. However, because the in-situ observations are few in this region, future work still needs to be done on investigating the performance of the coupled model.

Figure 11: Looks like the mixed layer depths (dashed lines) are different in panel (d) than the others, please correct/clarify this.

Now we have replotted Figure 11 and fixed this.