Community Comments 1

This manuscript presents a coupled atmosphere-wave-ocean model for regional studies of cyclone development including options to simulate both 2-way atmosphere-ocean and 3-way atmosphere-wave-ocean configurations. The model formulation and wave-coupling physics are briefly discussed and then a case-study is applied to analyze the impacts of the different model configurations. The text is clear and the presentation/writing is of good quality. The topic is presently significant for the ocean and wave modeling communities. However, the manuscript does not presently provide compelling arguments for new advances, capabilities, and/or findings related to wave/ocean coupled simulations under cyclones beyond what has been demonstrated in previous studies on the topic.

One main result is that the role of ocean coupling improves the simulation, but this could be investigated further to better explain why the improvement is found (see Major Concern 1). Another primary result is that Langmuir turbulence parameterizations can deepen the mixed layer and decrease the SST, but the effect of this on the coupled model is inconclusive and it is not clear if/how these conclusions would extend to other cyclone simulations. Additional analysis into the other wave processes mentioned in the model description but not analyzed would also help clarify what is learned here and what should be considered for future studies. I have several additional important technical concerns with the model and study, which are detailed below. At this point I cannot recommend the article for publication and recommend substantial revision.

We appreciate the reviewer for those insightful comments. In this work, we aim to demonstrate our most recent technical developments of the SKRIPS coupled model. We have implemented the Langmuir turbulence parameterizations in MITgcm using the most recent version of WAVEWATCH III. In addition, we used ESMF coupler based on NUOPC Consortium, which is also a state-of-the-art tool for developing the coupled system.

In our case study, we focused on the air-sea interactions in the Arabian Sea, also including the Red Sea and the Arabian Gulf. This region is important because of its rich and diverse ecosystem, its economic impact on the surrounding countries, and its important role in international trade. In our work, we contrast methods for implementing Langmuir turbulence. We also document the counter-intuitive SST changes when using the VR12-MA option to parameterize the Langmuir turbulence in the sensitivity analysis. In the words of Reviewer 1, “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.”

Now we have extensively revised our manuscript to highlight the innovations in our work. Please refer to the changes in Sections 1 and 2.

Major Concerns

1. Adding ocean coupling to a cyclone model is expected to improve the cyclone simulation by improving SSTs, most clearly seen in cases where simulating ocean cooling in place of static SSTs reduces the cyclone intensity (e.g., Ginis & Bender, 2000: doi 10.1175/1520-

0493(2000)128<0917:RCSOHO>2.0.CO;2). This study presents a downscaled model, however, and not a forecast model, so that what is called an “uncoupled” case here has time evolving SST with ocean cooling. It is unclear from the given results whether the coupling has indeed improved SST relative to the observation data (drifters or any other sources) compared to the HYCOM assimilating product SST (it might be useful to look at biases in SST, similar to Figure 8). This step and discussion would be useful to clarify that improved SST is indeed why the cyclone intensity is improved by coupling, and not, for example, other biases that are not ruled out by present analysis.

We have validated SST changes using in-situ observations in Section 5.1, but because of the lack of in-situ observations, only one drifter in the tropical cyclone wake zone is available for us to validate the SST from the simulations. The SST and MLD changes in the coupled model are also validated against HYCOM/NCODA data in Section 4.2.

We totally agree with the reviewer that we did not clarify why the cyclone intensity is improved in the coupled model. However, it is challenging to investigate the coupling processes during the tropical cyclone event. The simulation results are sensitive to the initial condition and the WRF physics options. The uncertainty of the model is also significantly large. In addition, due to the lack of in-situ observation, the SST obtained in the coupled simulation remains to be future work.

Now we have added the following text in our manuscript 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.

2. The description of model physics and equations needs some clarifications in the text. Some specific questions/issues:

2.1 The text states that the Stokes shear term in equation (1) is “parameterized” through Langmuir turbulence. L209 then implies that this term is dropped in the resolved scale model for this reason. However, there are scale separation issues here that are not discussed. Langmuir turbulence schemes are usually interpreted as representing the impacts of the WANS equations on turbulence at scales <~1km (usual LES domain, below the model resolved grid). Since the MITgcm model also solves the larger-scale “resolved” WANS equations including Stokes advection/Coriolis, including a Langmuir turbulence parameterization is not a formally consistent reason to drop the Stokes shear term in the model equations. At the relatively fine horizontal resolution of this model, the importance of the Stokes shear force should be more carefully considered and possibly retained to avoid changing the model’s resolved momentum balance (see discussions in Suzuki & Fox-Kemper, 2016, for example).

We thank the reviewer for pointing out this issue in our manuscript. In our work, we implemented the governing equations based on Wu et al., 2019 that considered the Stokes-Advection and Stokes-Corolis terms, but did not include the effect of Langmuir turbulence. Although we do not explicitly resolve all the Stokes Shear force terms, it is worth noting that some of these effects might have been implicitly incorporated by tuning the parameterizations (e.g., KPP) using ocean observations. In our work, we only consider the contribution of Stokes shear forces to TKE through Langmuir turbulence parameterizations following the work from Li and Fox-Kemper (Li et al., 2016 and Li et al., 2017).

We acknowledge that a scale separation is critical for justifying dropping the horizontal Stokes shear terms in the coupled model. In our simulations the grid resolution is about 8 km (0.075 degree) and we verified that the horizontal shear of the Stokes drift is several orders of magnitude smaller than the vertical shear. In the study case that we present, the ocean model is still far from resolving submesocales, for which one could expect the horizontal and vertical components of the Stokes shear to be comparable.

Now we have clarified the Stokes shear force in Section 2.3:

Considering the impact of the surface waves, the Stokes drift provides a source of the turbulent kinetic energy~(TKE) through the vortex force and modified pressure (Craik et al., 1976), or more cleanly the Stokes shear force (Suzuki et al., 2016) as mentioned in Eq.~(1). Evidence of this enhanced vertical mixing has been documented from observations and large-eddy simulations. Although this effect is not explicitly accounted for in KPP (K-profile parameterization)~(Large et al., 1994), KPP might have implicitly incorporated some effects of Langmuir turbulence from tuning the parameters to ocean observations (Reichl et al., 2016). In the test case discussed here, we implemented the model at about 8 km resolution (0.075 degree), for which horizontal gradients of the Stokes drift are several orders of magnitude smaller than vertical gradients. Based on this scale separation, we only consider the effect of Stokes shear force through Langmuir turbulence parametrized in KPP (Suzuki and Fox-Kemper 2016)

Although there are many unknowns about the exact physics by which Langmuir mixing enhances entrainment, there are many options to parameterize the Stokes shear force in Eq.~(1) that could alleviate the model bias from the simulations. Within the KPP scheme, we implemented three Langmuir turbulence parameterizations: (1) VR12-MA; (2) LF17; (3) LF17-ST…

Section 3.2:

When the effects of the 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.

2.2. Discussions in McWilliams et al. (2014, doi: 10.1175/JPO-D-13-0122.1) and Reichl et al. (2016b) suggest from Large Eddy Simulations that the KPP bulk Richardson number and the model's parameterized vertical momentum fluxes are improved by parameterizing with the Lagrangian current and Lagrangian current shear. I am concerned that separating between

Lagrangian/Eulerian currents in some parts of the model equations and not considering this difference in parameterizations could lead to inconsistencies in how the parameterizations are applied (e.g., this seems consistent with the explanation in the text for unintuitive results when using the VR12 parameterization, perhaps using the Lagrangian current would rectify this difference).

In our coupled model, MITgcm solves the Eulerian currents. The Stokes drift is added to terms on the right-hand side of Equation (1). We agree with the reviewer that using Lagrangian currents can alleviate the problem in VR12, but investigating the effect of Lagrangian currents is out of the scope of this model development paper. Now we have added the following discussions to the effect of Lagrangian currents in Section 5.2:

The drawbacks of VR12-MA are also discussed in Reichl et al., (2016b), where the authors show that using Lagrangian currents u^L on Langmuir turbulence parametrizations can alleviate the bias when using VR12-MA.

2.3. Is the Stokes drift considered for the volume conservation equation? Wu et al. (2019) express it as a non-divergent condition on the Stokes drift vector, which presumably results in a vertical component of “Stokes drift” since the horizontal components of the Stokes drift can be divergent. It should be clarified how this is dealt with in this model.

In the coupled model, we did not consider the vertical component of Stokes drift. Hence our governing equations do not include Eq. (13) and Eq. (14) as Wu et al., 2019. Now we have clarified this in Section 2:

It is noted that our implementations and tests aim to demonstrate the impact of Langmuir turbulence on the ocean, and thus the divergence of the Stokes drift is not considered in our governing equations as discussed in Wu et al., (2019a, b).

2.4. It is not clear what is gained by including the “VR12” <w’w’> scaling as a parameterization in this study. The previous studies of LF17 (and also Reichl et al., 2016b) have shown that the ad-hoc assumption of applying <w’w’> based enhancements to the diffusivity and Vt2 in KPP are inadequate Langmuir parameterization approaches. Since the results of this scheme in the study are unintuitive, it might be best to drop this from the study (or clarify what specifically is learned by including it).

Yes, LF17 is developed based on VR12 with consideration of the entrainment. By demonstrating the differences between VR12 and LF17, we aim to illustrate the differences due to entrainment. In addition, VR12 has also been proven to be able to reduce the error in some case studies (e.g., Li et al., 2016). In our work, we documented the non-intuitive SST warming due to VR12 and discussed it.

We have revised Section 2.3 in our manuscript:

VR12-MA and LF17 are implemented because they are used in a variety of case studies and substantially improve the shallow biases of mixed layer depth (Li et al., 2016; Li et al., 2019). We aim to compare the performance of VR12-MA and LF17 to demonstrate the impact of entrainment on the simulations.

Section 5.1 is also revised:

Though it is demonstrated in Reichl et al., 2016 that VR12-MA is not adequate to parameterize the Langmuir turbulence, this non-intuitive SST change needs to be documented and discussed.

2.5. The inclusion of the LF17-ST parameterization model in the comparison is also not well motivated/discussed. Presumably the difference from the LF17 WW3 version can demonstrate what can be gained by including a wave model (sea-state dependence) for the turbulence scheme (an interesting topic), but that point is not motivated or discussed.

First, we include this option to validate our implementations of LF17 and VR12-MA. The option LF17-ST parameterizes the Langmuir turbulence in the same way as LF17, but using the surface wind to compute the enhancement factor. We have used the intermediate results from LF17-ST (implemented by Shultz et al., 2020) to validate our implementations of LF17 and VR12-MA. In addition, because SKRIPS can run with or without the wave component, LF17-ST can be used to parameterize the Langmuir turbulence in atmosphere-ocean coupled simulations without waves.

We have added the following discussion on LF17-ST in Section 2.3:

LF17-ST is implemented to validate LF17 in the coupled simulations. Because LF17-ST does not need waves, it can be also used in the coupled ocean-atmosphere simulations to parameterize the Langmuir turbulence.

We have also added the following text to discuss the differences of the simulation results in Section 5.1:

The results obtained using LF17 and LF17-ST are generally consistent, because they use a similar way to calculate the enhancement coefficient and entrainment flux. Their differences are because of different options to parameterize the Langmuir number La.

2.6. The implementation of the wave-budget terms and the Charnock coefficient in computing the wind stress is highlighted in the model formulation, but it is not discussed in the results. Some more analysis to clarify how including these terms impacts the simulations would be beneficial.

We have discussed the impact of sea surface roughness parameterization in Appendix C, but we found out that they do not have major impacts on the characteristics of the tropical cyclone. Although surface roughness is parameterized based on wave age and steepness, there are uncertainties in simulating the tropical cyclone and thus the surface variables (e.g., 10-m wind speed, latent heat fluxes) are not significantly different. Now we have added the discussions at the beginning of Section 5:

We also performed a similar sensitivity analysis for different surface roughness parameters that may impact the atmosphere surface variables. However, these results are summarized in the appendix C because they are not significantly different.

Minor Comments

1. The impression of the abstract is that the wave coupling improves the model, but this seems misleading. The “improvements” appear to come from coupling to the ocean model and the impacts of waves are less significant (but also note major concern 1).

We have revised this in our abstract:

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

2. Regarding the wave momentum flux budget terms: How are the input and dissipation source terms parameterized for this study? Are these directly from WAVEWATCH? If so, clarify which source term packages are utilized and some discussion how these source terms are validated for cyclone wind speeds.

Yes. The input and dissipation source terms are directly from WAVEWATCH III based on Tolman 1995 and Ardhuin et al. 2003:

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 (Tolman 1995; Ardhuin et al. 2003).

We have also added the setup of WAVEWATCH in Appendix C:

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

3. Regarding the use of HYCOM velocities to initialize the MITgcm model: Assuming the velocities are not somehow made dynamically consistent with the regridded hydrography, are any potential implications from the initial shock/adjustment times assessed?

Yes. There are potential impacts from initial shocks that are not assessed in this work. We used the HYCOM/NCODA data because we want to make the ocean state closer to the “truth”. If we spin up the ocean model and assimilate the observation data, we still cannot completely get rid of the initial shocks. Now we have added the following discussion of the initial shocks in the revised manuscript:

To initialize the wave model, we allowed the wave field to spin-up for 19 days from May 01, 2018 and then we analyze the period from May 20, 2018. On the other hand, we did not spin up MITgcm or WRF, trying to initialize the coupled model using the analysis data directly. This may cause an initial shock in the coupled simulation, but we did not observe the initial shocks in the simulations.

4. Is the current passed to WW3 the same as the current passed to the atmosphere (e.g., Fig 1)? Presumably the atmosphere needs the surface current, but the current appropriate for WW3 is usually assumed at a depth related to the dominant wavelength (e.g., as in Fig 1 of Fan et al., 2009). Furthermore, the relative (and neutral) 10m wind should be used to drive WW3, adjusting for the surface current. It is unclear from the text/diagram if this is done.

The same ocean surface current velocity is passed to WW3 and the atmosphere model WRF. We are using the current velocity in the first layer of the ocean model (z0 = 4 m) to represent the surface current velocity. It can be found in Fig. 11(d) that the vertical shear is relatively weak within the ocean boundary layer. This implementation of the current effects on waves is similar to COAWST but not consistent with Fan et al., 2009. We have modified Section 2 to clarify this point:

The surface current velocity sent to WRF and WW3 is consistent in our model, using the current velocity in the first layer of MITgcm. This may overestimate the strength of surface currents passed to WW3 compared with Fan et al., (2009) which used the currents at $L/4\pi$, where L is the mean wavelength.

For the surface wind, we did not change anything in the WW3 bulk formula, but replaced the WND forcing in WW3 by sending the 10-m wind speed (U10/V10) from WRF. In WW3 the relative wind (RWND) switch is used and thus the current velocity is considered for computing the wind stress. Now we have clarified what we did in our coupled model in Section 3:

The wind speed sent to WW3 and MITgcm is the 10-m wind speed, then WW3 and MITgcm correct the 10-m wind speed using the current velocity in the simulations.

We have also added the switch in WW3 to Appendix C:

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

5. The time derivative should be a partial and not material derivative in equations 1&2.

Now we have fixed the typo in the equations.

6. L185: Why was a 19-day spin-up chosen for waves? This seems excessive for a forced regional model including boundary conditions. It would be interesting to know if this integration time was deemed necessary.

We set up the spin-up according to the literature (Sabique et al., 2012; Boutin et al., 2021). Usually, the spin-up time takes 15 days, but we simply start the spin-up on the first day of the month (May 1st 2018) and make it a 19-day spin-up. The spin-up for the waves is necessary because we need to provide the initial and boundary conditions for the regional model.

Sabique, L., Annapurnaiah, K., Nair, T.B. and Srinivas, K., 2012. Contribution of Southern Indian Ocean swells on the wave heights in the Northern Indian Ocean–A modeling study. Ocean Engineering, 43, pp.113-120.

Boutin, G., Williams, T., Rampal, P., Olason, E. and Lique, C., 2021. Wave-sea-ice interactions in a brittle rheological framework. The Cryosphere, 15(1), pp.431-457.

7. L234: both -> all

Now we have fixed the text.

8. L270: HYCOM yields colder SSTs, but appears to yield shallower MLDs. Is the reason for this understood?

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.

9. L290: Are the beams physical or numerical?

The beams are due to the refraction of ocean surface currents. We have documented these beams in our previous paper using the uncoupled WAVEWATCH III model (Sun et al. 2022 paper: https://doi.org/10.1029/2021JC018112). The beams disappear when we turn off the ocean current forcing or directional shift in WAVEWATCH III model, and they are generally consistent with satellite altimetry data. Now we have added the text to clarify this in the manuscript:

The spatial pattern of high and low beams of Hs is due to surface wave refraction by ocean currents. We have performed uncoupled simulations to investigate these beams and more details can be found in Sun et al., 2022.

10. Figure 7: Panels are mislabeled in the caption.

Now we have revised the caption of this figure.

11. Figure 8: Missing units

Now we have revised this figure, added the missing units, and adjusted the location of the figures.

12. Figure 11 is poor quality, e.g., what is “drho/dr”, what are units, etc. Why not use the KPP boundary layer depth for mixing layer, rather than the mixed layer depth?

Now we have added the names and units of all the variables shown in the figure. We have also changed the style of the lines to improve the quality. Yes, we were presenting the KPP boundary layer depth in those figures, now we have clarified this.

13. WaveWatch should always be capitalized WAVEWATCH (as an acronym).

Now we have corrected this text throughout the manuscript.