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Interactive comment on "The Effects of Ocean Surface Waves on Global Forecast in CFS Modeling System v2.0" *by* Ruizi Shi et al.

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First, the authors would like to sincerely thank the reviewers for their careful reading of the paper and their valuable comments to the manuscript and helpful suggestions. We will modify the manuscript according to the comments in the next few weeks. In the following, our plans for revision of each comment are given.

Review from Referee #2

This is another piece of work that is claiming that the coupling of an atmospheric model with the 3rd generation wave model for the specification of the surface momentum exchange is novel. Totally disregarding the work of Peter Janssen and colleagues at ECMWF. There is a whole book dedicated to the topic (Janssen 2004). The active

two-way coupled system has been operational in ECMWF medium-range forecasting system since 1998, with frequent updates following thorough testing. See for instance the recent adaptation of ST4 based physics to the ECMWF IFS system (Bidlot 2019) and further enhancement of this parameterization for tropical cyclone forecasts (Bidlot 2020). Robustness of the forecast performance requires many more cases. Obviously, this paper is a set of case studies. This needs to be clearly highlighted and discussed. In Janssen (2004), the impact of the coupling to waves is shown to be even more important at longer lead time.

Response: We acknowledge the pioneering work in ECMWF. We sincerely apologized for any misleading statement in the study. As indicated by the referee, our study focused on the effects of different parameterizations of z0 and Stokes drift-related Langmuir mixing in the CFSv2 in a series of cases. We will make it clear in the revised manuscript. Though Janssen (2004) suggested the impact of the coupling to waves is shown to be even more important at longer lead time, it is still interesting to investigate the effects of waves in a relative short period. We will extend the period of simulations from 7 days to several months, and evaluate more variables, such as 2-m air temperature, moisture and sea surface pressure etc. to investigate the effects of wave coupling in CFSv2.

The addition of the coupling to an ocean circulation model in which a wave model interacts with both the atmosphere and the oceans for the purpose of medium-range forecast is not new either. Following the work of Breivik et al. (2015), all components of ECMWF forecast system have been fully coupled for the past few years. I agree that aspects of Upper Ocean mixing are still very crudely represented in many models. This study explores the potential of using the wave model surface Stokes drift to supplement a Langmuir mixing parameterization. It is presented as a fast process acting quickly on the SST. But is it the right process? My understanding is that Langmuir turbulence might act much more slowly and is a factor in the determination of the mixed layer depth. Again, this study is a bit short to be really able to answer this question.

Response: The Langmuir circulation is also important at weather time scale. As Kukulka et al. (2009) indicated, the modification of Langmuir circulation to upper temperature profiles could be produced quickly in a few hours. We agree that the time period in this study is too short, so we will make longer period simulations to answer the question.

Is GFS only using the Charnock relation for the specification of the roughness length scale (z0) for momentum? There should also be a viscous contribution to z0. See Beljaars (1994). For this reason, I wonder about the coupling via z0. ST3 and ST4 in WW3 do not have a viscous term because they only deal with wave generation. From WW3, it is easy to determine the Charnock coefficient. Would it be more consistent to exchange it with GFS instead of z0? Moreover, the time steps of the different models imply that WW3 provides a new z0 every 900 s. Is it then kept constant until the next update? The Charnock relation implies that even if the Charnock coefficient is constant, the surface roughness can still change because the moment flux is still changing.

Response: We agree that there should be a viscous contribution to z0 especially when the sea surface is smooth and wind speeds are low. As suggested, we will exchange the Charnock coefficient with GFS instead of z0. We will also modify the time step of WW3 to be the same 180 s as CFS in all simulations.

The implementation of ST4 in WW3 was selected. Noting that WW3 documentation suggests different set of values of the parameters for ST4, which one was used? The selections of the parameters for ST4 in WW3 were obtained by running experiments with the stand alone version of the code for given forcing in order to yield the best possible wave results, there is no absolute guarantee that the surface stress and hence z0 are what would be expected in a coupled system. There is obviously limited amount of observations of surface stress, however it would be reassuring that in the mean, the drag coefficient from WW3 is in agreement with field data as estimated from observations (Edson et al. 2013). It is also unclear to me that the surface stress that WW3 is specifying (via z0) is consistent with the surface stress used by MOM4, or is MOM4

surface stress specified using another bulk formula not necessarily consistent with the what prescribed by WW3. Finally, is the GFS formulation for heat and moisture fluxes dependent on z0 (as it is the case in the ECMWF system) and therefore the coupling with WW3 would also influence heat and moisture exchange (this can important in tropical cyclone simulation).

Response: The parameters we used here are the TEST471, which is commonly-used at global scale. But we will further compare its effect with that of TEST471f, since the TEST471f is the CFSR tuned ST4 setup and might be more suitable in our system. We agree that the selections of different input-dissipation source term (ST) need to be further discussed in a coupled system. We will conduct a series of simulations to test the effect of ST3 (BJA) parameterization, whose performance is almost as good as ST4 in a single WW3. And as suggested, we will compare the simulated drag coefficient with observations (Edson et al. 2013). About the surface stress (via z0) in MOM4, it is the same as that in GFS due to coupling, and hence it is consistent with what prescribed by WW3. Yes, the GFS formulation for heat and moisture fluxes is also dependent on z0. Therefore the coupling with WW3 would also influence heat and moisture exchange.

Some specific comments: In section 2.2, the wave model surface Stokes drift is used. The Stokes drift calculation from the wave model 2d spectrum is heavily weighted towards high frequency. Is the frequency cut-off in its calculation the same as the model cut-off ($\sim$0.41 Hz), without the addition of a high frequency tail? In this case, it would be probably be overly under estimated and one might wonder if a simpler parameterization based on the wind speed will not suffice (ust(0)$\sim$0.016*U10), especially that it is mentioned that the potential misalignment between Ust and U10 has been found to be not important?

Response: Yes, the cut-off frequency of Stokes drift is the same as the model cut-off. We will increase the cut-off frequency and test the effects. According to Li et al. (2016), they included high frequency tail assumption for Stokes drift, but their results show the effect of misalignment between ust and U10 are rather small under a relatively coarse spatial resolution. In addition, they tested the ust bias with increasing spatial resolutions in a rather wide frequency range, and the bias differences are negligible. Thus, although their spatial resolutions are coarser than ours, we believe in our system even with the high frequency tail assumption the misalignment between ust and U10 may still not be clearly seen. These need further exploration.

Anemometers mounted on buoys are rarely at 10m height. Nothing is mentioned regarding the adjustment of the buoy winds to 10m. The discussion regarding the bias reduction of 10m winds is only relevant if the buoy winds have been adjusted to 10m.

Response: We will use the method (Hsu et al. 1994) recommended by NDBC to adjust the buoy winds to 10 m.

Minor comments: L50: you might want to add the following publications L63: modern reanalysis such as ERA5 is hourly L108: warm boots -> warm starts (?) Figure 1: so the low resolution surface currents are passed to the wave model, where the gradient in these is more important for wave refraction will therefore be poorly represented, but the same currents are not passed to the atmosphere where they could be used in a more consistent way to compute the momentum balance at the surface. L193: replace all reference to ERA5 by the Hersbach et al. (2020).

Response: We will modify the manuscript according to these comments. Since the gradients of surface currents are poorly represented in the model, we will remove the pass of surface currents to the wave model.

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