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Comment on gmd-2021-322

Anonymous Referee #1

Referee comment on "The effects of ocean surface waves on global intraseasonal prediction: case studies with a coupled CFSv2.0–WW3 system" by Ruizi Shi et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-322-RC1, 2021

Interactive comments on The Effects of Ocean Surface Waves on Global Intraseasonal Prediction: Case Studies with a Coupled CFSv2.0-WW3, Shi et al., GMDD, https://doi.org/10.5194/gmd-2021-322

This paper is a resubmission of a paper I already made comments about as a reviewer (https://doi.org/10.5194/gmd-2020-327). I thank the authors for re submitting an updated version of their work, and congratulate them for the general improvement of the manuscript, and for taking into account most of my comments. Especially, I appreciate that they use longer time spans for the sensitivity tests. The statistical analysis of the results is also much improved.

The present paper is a case study investigating the effects of several parameterizations representing the impact of waves on the ocean surface layer (Langmuir mixing and Stokes-Coriolis force with entrainment) and atmosphere surface layer (change of roughness length, effect of surface currents on the turbulent fluxes). The CFS2.0 ocean-atmosphere climate model and the WAVEWATCHIII wave model (WW3) are used in coupled mode for global simulations at resolution 0.25° to 0.5° for two time periods of 53 days, in boreal summer and winter. Four different simulations enable to assess the different effects on the SST, ocean mixed layer depth (MLD), 10-m wind speed, significant wave height (SWH) and latent heat flux in an incremental way. The conclusion is that refining the CFS2.0 representation of the surface exchanges by including additional terms due to waves leads to an overall (although modest) improvement of the SST and MLD biases with respect to observations. The improvement is larger in the Southern Ocean in boreal winter. Some improvement is also obtained on the surface wind speed and SWH, compared to ERA5.

The results presented here are not especially new, as recent sensitivity studies using the same kind of modeling platforms showed similar effects (e.g. Shimura et al., 2017 ; Torres et al., 2018 ; Bao et al., 2020 ;Couvelard et al., 2020 ;). But the sensitivity of the system CFS2.0-WW3 to these wave effects has not been studied so far.

Nevertheless, I have several major comments about the description of the coupled system, the evaluation of the impact of the different parameterizations, and the interpretation of the results.

General comments

1- Part 2 describes the representation of several physical effects impact the wave-ocean or wave-atmosphere exchanges, which has been implemented in the coupled system. The effects of Stokes-Coriolis and Langmuir mixing come as additional terms in the Richardson number or turbulent velocity scale of the KPP mixing scheme, the wave effect on the atmospheric roughness length comes through a change of the Charnock parameter, and the effect of the surface currents corresponds to the use of the relative surface velocity in computing the turbulent fluxes. For the effect of the Langmuir mixing, the authors assume that wind and waves are aligned, arguing the effect of misalignment has been shown to be non significant by Li et al. 2016. However, other studies like Polonichko (1997), Van Roekel et al. (2012), and Li et al (2017) showed that the Langmuir cell intensity strongly depends on the alignment between the Stokes drift and wind direction. The latter study especially concluded that assuming alignment of wind and waves leads to excessive mixing, particularly in winter. As the strongest effect of the Stokes-Coriolis and Langmuir mixing parameterization is obtained on the Southern Ocean in winter, I suggest to mention the results of these works in comparing the results of the VR12-AL-SC-EN experiment with respect to the CTRL one. Also Couvelard et al (2020) showed that there is a significant difference between annual averages of the module of the surface Stokes drift and of the part that is aligned with the wind (their Fig. 2). Please discuss. Also, the description of the exchanges of the different parameters between model compartments is unclear to me. I understand that all additional terms are computed in WW3, and that the Stokes drift and Langmuir mixing terms are transferred to MOM4, that the Stokes drift is transferred to GFS for computed the surface relative wind, and that the Charnock parameter is also transferred to GFS for computing the surface roughness (Fig.1). What is unclear is what is exchanged between GFS and MOM4? Especially, are the (regular) surface currents transferred from MOM4 to GFS and used for estimating a relative wind velocity in computing the turbulent fluxes by GFS? If so, is it consistent with the transfer and use of the Stokes drift from WW3? Please provide the corresponding information, with an update of Fig.1. What is the meaning of the blue arrow from the coupler to GFS in Fig.1? About the effect of the surface current on the atmosphere: I guess from eq. 7 to 9 and section 2.4 that only the effect of the currents (and especially of the Stokes drift) on the turbulent fluxes is taken into account, and not the effect of the current on the surface wind through the tridiagonal matrix (see the work of Lemarié 2015). If so, the fact that the coupling is not complete should be clearly stated in section 2.4.

2- The statistical analysis of the different sensitivity tests is much clearer and easier to understand than in the previous version of the paper. I still feel rather uncomfortable with the different diagnostics used by the authors. For instance, the correlation between the bias reduction and the absolute bias shown in Fig. 3,4, 8 to 11 is almost never commented, and I am not sure about its meaning: from the text, I guess that its corresponds to the correlation between the relative change between CTRL and ALL (the so-called PRD) and the absolute bias, but only when the time evolution of the bias corresponds to an increase. Is it so? What is the additional information with respect to the

PRD as shown elsewhere? Please elaborate. For most of the parameters compared in this study, the maps represent the relative improvement (PRD). For the 10-m wind speed and MLD however, differences with the CTRL are given and I find these maps easier to read. Please justify why you use different diagnostics or homogenize. The relative improvement (PRD) depends strongly on the initial value of the bias. Why not showing maps of the biases for the different simulations? It would help to appreciate where the biases have been corrected or not. Please give the values of the final biases (and RMSE) for every parameter/experiment, in addition to the PRD. The comparison of the 10-m wind speed and SWH with the NDBC follows some of my previous recommendation, and I thank the authors for that. I think, however, that the way this comparison is presented could be greatly improved. I suggested that maybe, the wind speed can influence the bias and the difference between CTRL and ALL, and this comment is still valid. There is some effect of the value of the bias with CTRL, even though the current presentation of the results makes it difficult to apprehend. Rather than a table giving the relative difference for different quantiles of biases, I would suggest using a graph comparing directly the 10-m wind speed of the simulation outputs (y-axis) with the 10-m wind speed of the NDBC buoys (x-axis) in wintertime (same in summertime, and for the SWH), every dot on the graph representing a buoy (4 graphs in total). The results of the different simulations can be plotted in the same graph, with different colors. This would enable a direct comparison, including the effect of the wind speed (x-axis) and of the bias (distance to the y=x line). The changes between the different simulations can be given by the mean biases and standard deviations with respect to observations, rather than the relative mean changes.

3- Interpreting the results could, again, be made in a more accurate and concise way. For instance, the discussion in section 4.3 is rather long and not very easy to follow. Probably the effects in boreal summer with respect to boreal winter could be presented more briefly.

Overall, I am not sure commenting in details improvements of a few percent is meaningful, but adding information about the correlation between the changes of different parameters can help to interpret the results. For instance, what is the correlation between the 2-m temperature and the SST changes? It confirm that the SST change is actually at the origin of the 2-m temperature change. Also, the correlation between the bias changes in 10-m wind speed and SWH is probably high. Please give values and discuss.

At some parts, the interpretation is not complete. For instance, the part about the latent heat flux does not lead to clear, concise results.

From Fig. S3, I understand that the time evolution of the absolute value of the biases of the different parameters considered is overall positive, both in winter and in summer. At most places, these trends are significant, and even large, like more than 0.02°C/day (corresponding to more than 1°C difference for the 53-day simulated period) or 0.02 m/day for SWH (more than 1 m difference). What is the implication for the mean biases, and their changes from CTRL to ALL? Does it mean that the simulations are drifting in time from their initial state, because there is no assimilation of data? Or, conversely, that their stationarity is not reached yet because the simulated period are too close from the initial state, despite the hot start? Please comment on that.

I specifically asked about the possible effects of including the parameterization of processes related with waves on the turbulent heat fluxes. The authors added a section about that effect, and I thank them for investigating it. However, it appears not significant, at least not for the time scales considered in this study.

Detailed comments

l. 26-29: are you sure that the SST change is at the origin of the 10-m wind speed change? I understand from 4.3 that the change of z0 also plays a role. Please check.

l. 65-66: the studies cited here are at climate scale, not for numerical prediction. The only model including wave effects and used for numerical prediction is ECMWF (IFS-WAM).

l. 112-114: this set of experiment follows one of my previous question (is there any impact of the coupling frequency?). Table S1 brings some statistics but they are not commented in the text. Please justify why 1800s was chosen as the coupling frequency.

l. 122-124: "the daily initial fields at 00:00 UTC.." I guess it is rather the initial field of the first day of each experiment? Or is the model re initialized every 24h from the operational analysis? Please specify.

l. 285 and following: Couvelard et al (2020) also obtained improvement of SST/MLD biases in the Southern Ocean. Please discuss your results against theirs.

l. 315-316: "generally consistent", please quantify.

l. 332 and following: please give the correlation coefficient between ALL MLD and the observations, so they can be compared with the 0.55 value given for CTRL.

l. 348: I do not know how to interpret the "negative trends of bias". What is the meaning of that?

l. 349-351: I would rather say the opposite: the biases in SWH are directly related to the biases of the 10-m wind speed.

l. 390 and following: see general comments. A graph showing the biases (model vs obs) in the different simulations would be easier to understand. Also, listing the buoy numbers in the SI is probably not useful, especially without additional information (position, number of observations). Please indicate, for every comparison, the number of buoys used.

Section 4.4: investigating the heat exchanges is nice at climate scale, but probably not relevant at the time scale of 2 months. I asked to authors to check for that, but it seems that the latent heat flux is directly influenced by the 10-m wind. Plus, the discussion in this part does not lead to clear results (to me). What would be your conclusion, beyond "the latent heat flux depends on the wind speed only"?

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