Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-327-AC1, 2020

Geoscientific Model Development Discussions

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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Received and published: 24 December 2020

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 #1

This paper presents a study on the effects of surface gravity waves on several parameters obtained from a global simulation using the CFS2.0 climate model (including GFS at ∼100 km and MOM at 0.5°). The coupling effects between the wave model (WW3) and the climate model are the effect of the waves on the Langmuir mixing and on the

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wind stress. The tests are made on several 7-day time periods, either in summer or winter. They show that taking into account the Langmuir mixing reduces the warm SST bias and MLD shallow bias in the ACC, and that the impact of the waves on the roughness length reduces the positive bias of 10-m wind speed and SWH in mid latitude, even though this improvement is modest. This subject is interesting and taking into account the wave impact on simulating the ocean or the atmosphere at climate or NWP timescale has motivated several recent studies. The results and the method used here are rather clearly presented. However, other aspects of the study are much less clear, namely I am not sure neither of the objectives of the study nor that they are addressed the right way. I list my major remarks below, I feel that both the interest and the impact of the study would be significantly improved should they be addressed.

1- I am not sure to understand the positioning of this study: the authors use a climate (OA coupled) model with a rather coarse horizontal resolution close to 100 km for the atmosphere and 0.5° for the ocean. These models are designed for long term studies at climate time scale, I am not aware of their use for NWP, which the authors present as the objective for their sensitivity study to coupling. This has some consequences on the processes that are sensitive to the coupling. Especially, I am not sure that the ocean (MLD and SST evolution) are so sensitive to the Langmuir mixing effect after a 7-day period but rather after months or years as is generally observed for oceanic processes in moderate conditions. If so, this should be assessed by comparing the results with those obtained using longer simulating time. Also, using such climate models for sensitivity tests at the NWP timescale should be justified and discussed. Are there any Met institutes currently using such models for weather forecasting? Global (coupled) models like IFS have horizontal resolutions close to 10 km and are able to represent explicitly mesoscale processes.

Response: As suggested, we will conduct longer simulations to test the effects of wave coupling and compare the effects of Langmuir mixing on the ocean (MLD and SST evolution) in 7-day forecast and longer periods. As stated by Saha et al. (2014), CFSv2

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plays a role in the operational 6-10 day forecasts and two-week forecasts. CFSv2 seems to be ideal for sensitivity tests at NWP timescale and longer timescale. However, we have not found any Met institutes currently using such models for weather forecasting. Although global (coupled) models like IFS with higher resolution are able to represent mesoscale processes, the computational cost for the coupled global forecasting is too high. We will extend our forecasting period from 7-day to several months, and evaluate the sensitivity to coupling.

2- The methodology of the coupling with waves is insufficiently justified. About the wind-wave coupling several methods can be used, based either on the Charnock coefficient directly assessed in the wave model (ST4 in the paper) or parameterized from the wave age or wave steepness (ST4-Fan in the paper). Either has its merit, but the choice the authors made should be better discussed. Is it only based on the variation of z0 as shown Fig. 2? Also, in WW3, the ST3 parameterization is usually considered as the best suited for representing the high-frequency tail of the spectrum, which controls the Charnock coefficient, while ST4 is better suited for wave parameter modelling. So, I wonder why the ST3 parameterization is not considered here along with ST4. Did the authors test this? Please discuss. Same comments could be made about the wave-ocean coupling. The authors consider only the effects on the Langmuir circulation and mixing (maybe on the Stokes-Coriolis force as well, this is not clear to me). Are other terms (see Couvelard et al. 2020 or Bao et al. 2020) not significant? Is this related to the timescale or the resolution considered?

Response: As suggested, we will test the wave steepness-related parameterization and ST3 parameterization for the wind-wave coupling, and clearly discuss the merit of different parameterizations. About the wave-ocean coupling, we currently only test the effects of Langmuir mixing at the relative short time scale. We will add the Stokes-Coriolis force. The aim of this study is to evaluate the effects of different parameterizations of z0 and Stokes drift-related Langmuir mixing. To establish a more complete coupling system, more wave-related processes should be considered, such as sea spray, wave breaking and non-breaking wave effects (Couvelard et al. 2020 or Bao et al. 2019). We will add these effects in our future study.

3- Considering the resolution of the models and especially the short timescale of the experiment, I wonder whether the effects of the waves on the ocean are fully accounted for. Ocean has inertia, especially at 0.5° resolution, and I suspect that the effect of the coupling can increase or appear after the 7-day time period considered here, even with a hot start. Did the authors test longer simulations? I would like to see a comparison between the effects obtained after 7 days and a longer time period, to check that this choice is appropriate – and the corresponding discussion. Also, the diagram of coupling (Fig. 1) shows that surface currents are sent from the ocean model to WW3, but I suspect that their effect cannot be seen due to the coarse resolution of the ocean model (see Ardhuin et al. 2017, most of the current dynamics come from small scale).

Response: As suggested, we will test a series of longer simulations. We compared tests with and without coupling of surface currents, the difference of the results is negligible. So, we will remove the pass of surface current to the wave model.

4- The coupling with the atmosphere is not really a coupling but rather a change of parameterization, as well shown in Fig.2. As the authors correctly state in the text, the change in the wind speed or SWH comes from a mean change of z0 (mean difference between GFS and ST4 or ST4-Fan in Fig. 2), not from the variability induced by the waves (scattering of ST4 for instance). So changing the (constant) Charnock coefficient from 0.014 to e.g. 0.018 should have the same effect. This is a common confusion, as wave coupling is often mixed with change of parameterization (see Pianezze et al. 2018 for instance) but this be made clear in the text.

Response: Although the change in the wind speed or SWH comes from the change of z0, we could not simply change the constant Charnock coefficient. As Shimura et al. (2017) indicated, a constant Charnock coefficient implies only wind speed-dependent roughness, while the change of z0 also depends on the development of waves. From

Fig.2, there is a spread of z0 at the same wind speed induced by wave variability. We will clarify this in the text.

5- I am not sure about the significance of the results itself. Concerning the part I know best, the effect of the waves on the atmosphere, the decrease of the surface wind (and of the SWH) by an increase of the surface roughness, there is nothing really new here (see Renault et al. 2012, Pineau-Guillou et al. 2018 or Sauvage et al. 2020 for studies at NWP time scales). But the impact of waves can also change the heat fluxes and the atmospheric parameters through several mechanisms, which are discussed and assessed in Renault et al. 2012 or Varlas et al. 2020. For instance, more mixing can change the SST and results in a decrease of the turbulent fluxes and air temperature. Could you please detail why you investigate only the effects on surface wind (among other atmospheric parameters)? The general improvement mentioned by the authors is rather modest (5%) and I wonder if considering other coupled effects or a longer time scale could change this. The statistical significance of the effects obtained is never discussed, for instance is the change of MLD between the different experiments shown in Fig. 5 significant with respect to the Argo values? Is the improvement of bias or scores for the SWH in Table 2 significant at 95 or 99%? Information about the number of data used for the comparison would be welcome, especially for along track satellite data. Also (minor point) the Percentage Absolute Difference rather looks like a relative score to me. Please clarify.

Response: As suggested, we will make the statistical significance test. After we extend the period of simulations from 7 days to several months, we will evaluate the change of heat fluxes and other atmospheric parameters such as 2-m air temperature, moisture and sea surface pressure. Previous studies mainly focused on the wave-related effects of global coupled systems in years and decades. While our evaluation focused on the effects of different parameterizations of z0 and Stokes drift-related Langmuir mixing in the CFSv2 in several days and several months, since CFSv2 is applicable in the relative short time scale. In addition, the Percentage Absolute Difference here is a

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relative score, similar to the difference of mean absolute percentage error (MAPE) to evaluate the improve of bias.

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