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Comment on gmd-2021-322

Anonymous Referee #2

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-RC2>, 2021

Review "The Effects of Ocean Surface Waves on Global Intraseasonal Prediction: Case Studies with a Coupled CFSv2.0-WW3"

Here are my comments.

Lines 150, 152 and 157 Van -> Van Roekel

Line 169, section 2.3.2: the Stokes drift should also be used in the advection of any tracer, including temperature and also in the calculation of the vertical velocity in difference/convergence term NEMO4.

Line 179, section 2.4: there is an inconsistency in considering the impact of the surface

current and of the surface Stokes drift on the momentum flux in the atmosphere model, but not in the wave model. With ST4 (and ST3), the surface momentum balance is re-evaluated in order to determine the friction velocity that is then used as part of the source terms calculation, and hence the evolution of the wave field. To be consistent, WW3 should be forced not with the absolute 10m wind, with the relative 10m wind with respect to the surface current.

Line 216: as far as I can understand from the text, the ST4-Fan scheme is used for z_0 in the atmospheric model only, and not in WW3. This is not consistent and should be made clearer that z_0 and hence u^* inside WW3 will still be based on a Charnock determined from a modified version of Janssen wave induced stress (Ardhuin et al. 2010).

Also, WW3 can be run with a cap on z_0 , and hence on the Charnock it could return to the atmosphere. See z_{0max} in table 2.6 in the WW3 manual. It is indeed set to a large value for TEST473f, however, TEST500 has $z_{0max}=0.002$ for instance. I believe, it is worth mentioning as ST4-Fan is not the only way to limit Charnock for high winds. One should also mention some recent developments on modifying ST4 for high winds (Bidlot et al. 2020 and Li et al. 2021), without a very awkward parameterisation of Fan et al. (sorry I notice that it is mentioned later (line 488), but it might too late).

I have serious problem with ST4-Fan. It will indeed limit Charnock for large winds, but from figure S2, it does not seem to make sense that Charnock is largest in the Tropics. The Charnock parameter was introduced to represent the impact of waves on the momentum transfer at the sea surface. It was recognised that young windsea should extract more momentum than older more mature old sea. So why is Charnock largest in the Tropics where the sea state should be dominated by old windsea and swell?

Line 231: it is more than just the fetch, but also the development stage of the sea state (young sea, old sea...)

Line 233, before section 2.6: Could you say what is done in WW3 when sea ice is present. It seems that there is a large impact near or within the sea ice, so it is important to know how the different fields were produced by WW3. Quite often, WW3 is set-up so that the wave spectra are reset to 0 every time step for all areas with a sea ice cover above a certain threshold. If it is the case, this will mean that the estimate for Stokes drift and wave age would be those for very young windsea. It can then be debated whether these estimates are correct, as it is known that in the presence of sea ice, the high frequency waves are heavily damped.

Line 256 and table S4: NDBC website reports its own buoy data, as well as buoy data from other NOAA agencies, such as the data from the TAO array, and a few other buoy data providers along the US. But it also reports surface observations from many coastal stations (i.e. on land, piers or towers) that are definitely not buoy. They also report observations from the oil and gas industry in the Gulf of Mexico. In table S4, anything that does not have a 5-digit identifier is probably not a buoy, and the oil and gas data from the Gulf of Mexico have an identifier like 423xx, 428xx. All those non buoy data should not be use in this analysis.

Line 261: section 4.1: I am surprised by the lack of impact of the FLUX experiment on the SST, in particular in the equatorial Pacific. My experience is that including the winds relative the surface current in the momentum flux (surface stress) used to force the ocean model has quite an impact on the SST around the equatorial Pacific. To be sure, could you confirm, as indicated that MOM4 is indeed forced with the surface stress as shown in equation (7).

Line 474: it not true, the SST bias in the Tropics was not reduced due to drag from swell. Breivik et al. (2015) considered the impact of sea state dependent Charnock, using the formulation from Janssen on the air-side surface stress, which was modulated further considering the momentum flux balance between wind input and wave breaking to determine the ocean-side stress that is then used to force the ocean. Moreover, the impact of wave breaking was also considered as input to the upper ocean mixing scheme (TKE). All these effects had an impact on the SST. What was not discussed in Breivik et al. (2015) is the impact of surface currents on the SST response in the Tropical Pacific as that effect has already been introduced in the ECMWF system years before. As I mentioned

earlier, I am surprised by the lack of sensitivity on the CFS system to surface currents in the Tropics.