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The Cryosphere
Discussions

Interactive comment on "Toward a coupled model to investigate wave-sea ice interactions in the Arctic marginal ice zone" *by* Guillaume Boutin et al.

Anonymous Referee #3

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The manuscript Toward a coupled model to investigate wave-sea ice interactions in the Arctic marginal ice zone by Boutin et al. presents a model that couples waves and sea ice dynamics to study the impact of waves on sea ice evolution over the Arctic Ocean. The model includes a floe size and thickness distribution as a prognostic variable that is exchanged between the sea ice and wave components. The FSTD obeys an evolution equation that includes floe-size dependent processes such as lateral melt and wave break-up. A focus is put on the wave radiation stress arising from wave attenuation in sea ice that imposes an additional force on the ice, and on the floe-size dependent lateral melt parameterization. The impact of wave-related processes on sea ice are studied by comparing simulations of NEMO-LIM3 (ice-ocean component) that

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is coupled and uncoupled to WW3 (wave component) over a pan-Arctic domain, and during two storm case. The comparison is done over a month-long period, at the end of summer 2010, after a 8-year spin-up period.

Overall the paper makes a significant contribution to the modeling of polar marine environment in the sense that it provides a very useful tool to study the complexities wave-ice interactions and their impact over different spatio-temporal scales. The discussion puts the study in the context of the recent developments and describes the limitations, thus pointing towards important issues to be addressed in order to make further progress (duration of the simulation, atmospheric and oceanic coupling, floe-size dependent ice rheology missing, freezing period not studied, etc.). It is well written, despite some typos and corrections that need to be made, and descriptions of model implementation and results are detailed enough, although some key information is missing (see below). It is thus worthy of publication, after minor revisions are made.

Specific comments

P4. L18. Wave attenuation is a central piece of the study, as it determines the wave radiation stress and, to a certain extent, the extent of the wave-induced ice break-up area (i.e. the marginal ice zone). Because of this, I suggest that in addition to referring to Ardhuin et al. (2018) for the choice of the wave attenuation, authors recall the main characteristics of the attenuation scheme. Is it floe-size and/or thickness dependent, and how? Is it a dissipative or scattering scheme (or a mix of both)? This could be done in a few lines.

P6. L1. Another central piece of the study is the ice drift resulting from the momentum balance. Here the WRS is added as an external forcing term that will be balanced by the internal stress, and model solutions may depend strongly on rheology parameters. I understand that this term (rheology) has not been modified significantly from what's typically used by LIM3 users, and that studying the ie rheology is not the focus of the paper, but it needs to be described minimally here. The rheology contains a few parameters that can be tuned for various reasons, including the compressive strength, the shear-to-compressive strength ratio, if not the yield curve itself or the numerical scheme. Describe what rheology is used and what are the main parameter values. Maybe adding a table would serve well that purpose.

P11. L14. Warmer and saltier surface waters in the CPL run seems to point towards that enhanced turbulent mixing arising by increased shear stress between the ice and the ocean, dominates over enhanced melting, which tends to produce fresh and cold anomalies. The following section focuses on an interpretation of that response in terms of the differences between the lateral melt parameterization. Have you looked at mixing as a possible mechanism for explaining it? Are there anomalies in the mixing or mixed layer depth in the marginal ice zone? This mechanism is discussed very clearly later in the two storm cases, but it would be interesting to discuss it also for the pan-Arctic case.

P19. EqA3. Define D_*. And later, define also n_*. Is D_* equivalent to D_{n*}?

Some typos

P5. L14. Replace actualized by updated.

P5. L20. $\ldots$ is transferred to what has caused this attenuation.

P5. Eq2. Remove parentheses around σ.

P6. L22. multi-category.

P7. L29. c has already been introduced as the concentration earlier.

P8. L10. Is Toyota et al. (2011) the right reference for this statement? There are older and more appropriate references for this it seems. The smallest floe size that can be generated by flexural break-up is thickness-dependent. Maybe this should be acknowledged.

P8. L25. Uncoupled instead of not coupled (also at various other place in the manuscript).

P9. L8. Based on a number of observations.

P9. L17. Rather than on sea ice conditions.

P10. L8. . . . on sea ice conditions.

P10. L19. There is no panel e on Fig. 5.

P11. L9. Do you refer to the grid cell average thickness? Specify.

P11. L11. There are also differences . . .

P12. L1. . . . property anomalies.

P14. L6. Difference (singular).

P17. L24. when trying to forecast . . .

Fig2. Schematic summary of . . . The two boxes correspond . . .

Fig3. Panel c. notcpl should be replaced by NOT_CPL in the index. You can also specify the run elsewhere than in the index to avoid expanding indices.

Fig5. The black and grey contours . . .

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