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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 #2

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The manuscript describes a coupling between the Wavewatch III ocean surface wave model and the LIM3 sea ice model, which accounts for (i) wave radiation stress (WRS) on sea ice and (ii) floe-size-dependent lateral melt. Results are presented from an idealized model configuration, demonstrating the impact of individual steps in model development, and then from a pan-Arctic configuration where the sea ice model is coupled to NEMO. The authors find that (i) has an impact on sea ice dynamics on the order of 10%, and (ii) results in generally lower lateral melt than a parametrization based on sea ice concentration. They include interesting regional case studies as well as a pan-Arctic evaluation. The case studies highlight the importance of the direction of the WRS in determining its impact on the sea ice edge.

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Discussion paper

Coupling a sea ice and an ocean surface wave model is a valuable step forward, enabling investigation of marginal ice zone physics as well as potential advances for both sea ice and wave forecasting. The results on the impact of wave radiation stress on sea ice are very interesting, and certainly worth publication. In fact, the novelty of including this process in a commonly-used, pan-Arctic sea ice model should be emphasized further in the text. However, the manuscript includes some unclear reasoning and the floe size distribution model developed to examine the impact of lateral melt raises some questions that I list below in 'Specific Comments'. I also noted some incorrect representation of the literature.

In general, the manuscript is hard to follow, uses inaccurate or informal phrasing in places and contains a number of grammatical and typographical errors. It requires a thorough proof-read before re-submission. I have listed some sentences to be rephrased at the end of the review, but note that this is not an exhaustive list. When re-writing, the authors should carefully check where the text can be made clearer and more concise.

Specific Comments

P1 L2 and P1 L20: Strong & Rigor (2013) find that the Arctic MIZ (defined by sea ice concentration) has been expanding in summer and contracting in winter over the recent historical period. This should be referenced in the text.

P1 L2: 'Yet, state-of-the-art models are not capturing the complexity of the varied processes occurring in the MIZ, and in particular the processes involved in the ocean-sea ice interactions.' This is a very broad and vague sentence. The models may not include certain processes that occur in the MIZ, but they may be able to capture their large-scale impacts through parametrizations.

P1 L15-19: I would suggest the authors be more specific here about what processes they are referring to.

P2 L20: Check the location of reference placement in these sentences.

P2 L31: I would dispute the phrasing that 'In contrast, little progress has been done regarding the inclusion of waves in coupled ocean-sea ice models.' Simulation of the FSD within a climate-scale sea ice model is the first step required to model fracture of sea ice by ocean surface waves, and the past few years have seen much progress in this area: Zhang et al. (2015,2016), Horvat & Tziperman (2015), Bennetts et al. (2017), Roach et al. (2018), Bateson et al. (2019, in review). These studies have used simple representations of waves in order to develop the physics relating to sea ice, just as the studies focusing on the impact of sea ice on waves (Dumont et al. 2011, Williams et al. 2013, etc) have prescribed sea ice conditions and/or neglected certain sea ice physics. These paragraphs should be rewritten to more accurately reflect the current state of the literature, including all references I listed above. Additionally, I would use 'simple' rather than 'crude', which has negative connotations.

P3 L16: It would be useful to include a brief summary of the different processes by which sea ice affects waves in the model, for readers not familiar with the Boutin et al. (2018) paper.

P4 L1: What does 'Arctic realistic simulation' mean?

P4 L24: Describe the Lupkes et al. (2012) parametrization at its first mention, or don't mention it here.

P4 L23: If I understand correctly, the full NEMO ocean model is initialized from a climatology and spun-up for nine years. This seems to be a rather short spin-up period. How was it determined that nine years was sufficient? Similarly, how was it determined that three days was a sufficient adjustment period for the introduction of the wave coupling?

P5 L12: 'Updated floe size.' how is 'floe size' defined?

P5 L14: 'floe size is actualized'. What does this mean? Also, P9 L11: What is 'actual floe size'? Similarly P9 L25: What is the 'actual FSD'?

P5 L14: 'LIM3 takes into account the WRS in its ice transport equation'. This should be stated in the Introduction, as it is a key contribution of the manuscript.

P6 L4: How is the partial sea ice cover already accounted for in WW3?

P6 L22: Define the sea ice thickness distribution and the FSD function. Is the latter an areal distribution?

P6 L22: I think a little more explanation would be useful here for readers not familiar with the various FSD schemes in the literature. Add a sentence or so on why the Zhang et al. (2015) approach is chosen over the Horvat & Tziperman (2015) approach. The sentence from P18 L13 'assuming floes of different sizes. . .' should be stated here as well.

P6 L28: 'implemented a FSD that enables floes to be advected..' – the FSD itself does not enable this, presumably this should say that Williams et al. (2017) implemented a scheme for advection of the FSD. Consider summarizing how this works e.g. what quantity is advected?

P6 L31: 'We do not make any assumption on its shape in general, but the FSD is forced to follow the power-law assumed in WW3 as soon as wave-induced sea ice break-up occurs.' This sentence seems somewhat self-contradictory: there is an assumption on its shape if the FSD is constrained to follow a power-law.

P7 L4: 'Assuming a power-law FSD is coherent with a distribution caused by a succession of break-up events (Toyota et al., 2011, Dumont et al. 2011).' The Toyota et al. 2011 study finds a change in the value of the exponent of a power-law fit to their data at around 40m. Does the model presented here assume a single power-law exponent, or include this transition? Also note that the Toyota et al. 2011 study covers a small area in space and time, and therefore may not be globally applicable. The Dumont et al. 2011 study itself does not show that a power-law FSD arises from a succession of break-up events, but rather provides a mathematical description for this assumption, so this citation should be removed or discussed in a different way.

P7 L8-22: The authors assume a power-law FSD in WW3 and then force LIM3 to follow the same power-law when wave fracture occurs. As they state, the effects of sea ice advection and thermodynamics cause deviations from a power-law. However, the effects of these processes may be over-ruled to continue to force the FSD to follow a power law, at a frequency determined by an arbitrary parameter. I don't understand why the authors take this approach. Why include other FSD processes if they are not always allowed to affect the FSD? How often does such over-ruling occur - is this most of the time or in a small fraction of timesteps? Is there an alternative approach to the power-law assumption? The assumption has not been well justified in the manuscript.

Similarly, can sea ice fracture be handled in the sea ice model rather than the wave model? I would have thought that this would avoid the need for the Dmax adjustment.

P8 L3: 'This sensitivity remains really small.' This statement should be quantified more precisely, and the authors should describe how they determined this or consider adding their sensitivity results to Supplementary Material. Was sensitivity to the smallest resolved floe size tested? I would expect that lateral melt would be particularly sensitive to this.

P8 L16: Is this the same experiment as in Fig. 1? It would help the reader to restate what the differences in the two runs correspond to physically. I think there are quite a few differences - evolving sea ice, advection of Dmax - which make it hard to understand what the differences in the model output mean.

P9 L8: The Lupkes et al parametrization should be defined explicitly. Is this what LIM3 uses as standard for D in Eqn. 5? Please explain the reason for using it here.

P9 L11: A situation where ice concentration is less than 0.6 and floe size is greater than 10 m could occur anywhere, for example near the ice edge in wave-free conditions, so

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I suggest removing the first part of this sentence.

P9 L30: As stated above, I would expect the amount of lateral melt to depend strongly on Dmin. Have the authors investigated this? If not, the results on lateral melt should include some discussion of this.

Section 4.1: This section compares the CPL and WAVE simulations at the pan-Arctic scale. The differences between the simulations include the impact of wave radiation stress and floe-size-dependent lateral melt. The authors then try to attribute various impacts to one of these two processes. Why not consider two separate runs here, one which adds the wave radiation stress only and one which adds the floe-size-dependent lateral melt only? As it is, I found it difficult to understand this evaluation.

Section 4.1 in general was difficult to read. The text usually described differences to the CPL run. However, the WAVE and NO-CPL runs should be considered as the reference simulations, and so differences should be described in the CPL run relative to the reference runs (i.e. describe an increase in CPL relative to NO-CPL, rather than a decrease in NO-CPL). I think this would improve the readability.

P10 L19: It would help the reader to briefly restate the differences in the runs at the start of the paragraph, including the note at L27 (‘One should keep in mind…). Also note mis-matched parentheses here.

Sec. 4.1.3: The discussion of lateral melt would be aided by figures showing some equivalent floe size statistic from the Lupkes parametrization and from the FSD model.

P12 L11: ‘This result does not reflect the fact…’ What does this sentence mean?

P12 L17: ‘Actually, in contrast to what was found in previous studies by Zhang et al. (2016), Bennetts et al. (2017), Roach et al. (2018a), de-activating completely lateral melt in both runs (not shown) has a negligible effect on the quantity of melted ice in our simulations (not shown).’ The three named studies did not deactivate lateral melt, so the results presented here cannot be ‘in contrast’ to theirs. However, Roach, Dean and Renwick (2018) did essentially deactivate lateral melt, by setting all floe sizes to 10000m, and showed that this had no impact on sea ice concentration in the Antarctic.

Section 4.2: This subsection is very interesting, but again hard to follow. Perhaps consider using one figure for each case, reducing the number of variables shown in figures in the main body of the paper, and moving the remainder to Supplementary Information. More figures could be added in the Supplementary for some of the 'not shown' aspects. I counted thirteen 'not shown' aspects in the paper, which seems rather high.

P16 L13: 'It is, however, mostly compensated by an increase of lateral melt.' Add that this is the converse of what has been shown in previous studies.

P16 L8: 'The coupled model was then used to examine . . .. the effects of wave-induced sea ice break-up on sea ice melt.' Rather, the study compares their model to an alternative parametrization for lateral melt (the Lupkes parametrization), that is designed to approximate varying floe sizes for different concentrations. To isolate the impact of the wave-induced break-up, or the 'impact of the coupling' as mentioned earlier, a more suitable comparison would be to a simulation where all floes were unbroken. Otherwise, modify the discussion in the text.

P16 L30: Similarly, the paragraph at P16 L30 compares the difference in lateral melt between the FSD model and the Lupkes parametrization (with varying floe size) to the differences found in previous studies. However, these previous studies show differences between a FSD model and a constant floe size parametrization for lateral melt, so should not be directly compared to this study. The discussion of the various studies should reflect this.

P17 L7: 'One should also remember that the studies of Zhang et al. (2016) and Roach et al. (2018b) were aiming at representing the evolution of floes larger than 1000 m.' This is incorrect. Both studies represent floes up to a maximum floe size of around 1000 m (radius). Also note that Roach et al. (2018a) and Roach et al. (2018b) are confused in places.

P17 L13: ‘Among the wave-sea ice interaction processes. . .’ This sentence is unclear. Impact on what?

Presentational Comments

Throughout, I would suggest referring to ‘ocean surface waves’ in the abstract and early parts of the Introduction, rather than simply ‘waves’ for clarity.

I would also suggest using ‘sea ice fracture’ rather than ‘sea ice break-up,’ as this is used in other studies

In general, the definite article is over-used e.g. ‘the sea ice near the sea ice edge’ can simply be ‘sea ice near the sea ice edge’, ‘impact the sea ice floe size’ can be ‘impact sea ice floe size’ etc.

P1 L3: ‘In the present study. . ..’ - clumsy sentence, suggest rewording

P1 ‘highlight the need to include the wave-sea ice processes in models aiming at forecasting sea ice condition on short time scale’ -> ’highlight the need to include wave-sea ice processes in models used to forecast sea ice conditions on short time scales’

P2 L5: -> ‘and sea ice drift’

P2 L12: ‘in the direction of the propagation’

P2 L13: ‘Southern ocean’ -> ‘Southern Ocean’

P2 L14: ‘may become more prominent in the Arctic in the future.’

P2 L27: ‘a first step was done’ -> ‘a first step was made’, similarly elsewhere progress is ‘made’ rather than ‘done’

P3: reword ‘wave by sea ice’; ‘is implemented or not’; ‘without any wind or ocean current’; also the sentences on timesteps

P3 L14: change ‘on’ to ‘of’

P4 L20: ‘aim at compensating’ -> ‘was made to compensate’

P4 L31 ‘in this particular year’

P4 L31: reword ‘storms occurring during it’

P5 L1: ‘referred to as WAVE’

P5 L9: sentences about average thickness - seem to use a lot of words to say something fairly straightforward

P5 L29: define vector k

P7 L8: ‘the coupling between the two models can be done’ -> ‘the two models can be coupled’

P10 L4: The introduction to Section 4 seems unnecessarily lengthy and should be made more concise.

P10 L5: ‘the impact of the including the wave-sea ice interactions’ - reword

P13 L25: ‘that is exposed upwind (and waves)’ - reword

P14 L19: ‘could in principle modified’ - reword

P14 L34 ‘very high waves of which attenuation induces WRS’ - reword

P15 L14: ‘pattern than’ -> ‘pattern to’

P15 L32 ‘low concentrated’ -> ‘of low concentration’

P16 L15: ‘generating higher and more energetic waves’

P17 L7: ‘were aiming at representing’ -> ‘aimed to represent’

P17 L14: ‘additional lateral source melt’ - reword

Section 4.1 figures – in the reference plots, I found the colormaps rather counter-intuitive. Consider choosing maps that are white at zero.

Fig. 7: y-axis label lists the units as %, but values on the y-axis are out of 1. I presume that the 10ˆ2 kmˆ3 corresponds the numbers on the figure, but this should be noted in the legend.

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