

Reviewer 2

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 resubmission. 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.

We thank the reviewer for their careful reading of our manuscript and for their comments and suggestions. We have tried our best to address their questions and concerns, as detailed in the following. A careful proof-reading of the text has been done to improve the readability of the text. In our comments, PXL Y refers to page X line Y of the attached updated manuscript.

Specific Comments:

P1 L2 and P1 L20: Strong et 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.

We now refer to Strong et Rigor (2013) in the introduction (P2L2)

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.

The sentence has been changed to:

P1L2: *"Yet, state-of-the-art models exhibit significant biases in their representation of the complex ocean-sea ice interactions taking place in the MIZ."*

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

We have added a few examples:

P1L5: *"Indeed, the MIZ is characterized by a wide variety of processes resulting from the highly non-linear interactions between the atmosphere, ocean and sea ice: sea ice floe fragmentation and welding, lead opening and associated heat transfers, mesoscale and submesoscale features arising from strong temperature and salinity gradients (see Lee et al., 2012, for a review and references therein)..."*

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

This has been fixed.

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 et 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.

We agree with the reviewer that this paragraph was too negative considering the amount of work recently done on FSDs in sea ice models. We therefore rephrased it to emphasize the step-by-step progress that have allowed us to perform this study. We also added missing references to the work of Roach et al. (2018) and Bateson et al. (2019).

P3L1: “In parallel, progress has also been made regarding the inclusion of the effects of waves in coupled ocean-sea ice models. Using a very simple parameterization, Steele et al. (1989) and Perrie and Hu (1997) have investigated the effect of WRS on sea ice drift in the MIZ, only considering the attenuation of waves generated between the ice floes, and found a limited impact on the sea ice conditions. More recently, Williams et al. (2017) implemented a wave module in the semi-Lagrangian sea ice model neXtSIM (Rampal et al., 2016) and found that high wave conditions can cause a significant displacement of the sea ice edge. The implementation of FSDs in different sea ice models, as introduced by Zhang et al. (2015) and Horvat and Tziperman (2015) for instance, has also opened the way to the assessment of the potential enhancement of lateral melt by wave-induced ice fragmentation (Zhang et al., 2016; Bennetts et al., 2017; Roach et al., 2018; Bateson et al., 2019), but the representation of waves remains too simple to simulate the full effect of waves on the evolution of sea ice.”

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.

We have added the following sentence in section 2 to briefly describe the model detailed in Boutin et al. (2018):

P3L27: “These processes are scattering (which redistributes the wave energy without dissipation), friction under sea ice (with a viscous and a turbulent part depending on the wave Reynolds number), and inelastic flexion. All these processes depend on sea ice thickness and concentration, and scattering and inelastic flexion also depend on floe size.”

P4 L1: What does ‘Arctic realistic simulation’ mean?

This sentence has been fully rephrased:

P4L17: “The wave spectrum used as forcing at the boundary is extracted at a point south of Svalbard from an Arctic hindcast performed with WW3 described by Stopa et al. (2016). It covers the period of May 2nd to 3rd, 2010, during which a storm occurred in this particular area (Collins et al. 2015).”

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

We have removed this reference and modified the sentence as follows:

P5L7: "[...], in which the already existing lateral melt parameterization in LIM3 is activated."

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?

Regarding the ocean-sea ice model, the spin up would indeed be too short to allow for a full adjustment of the full water column. However, here, we only focus on ocean surface processes, that are expected to quickly respond to the atmospheric and sea ice forcing, for which 9 years is largely enough to equilibrate. Regarding waves, a spin up of a few days is what we typically use in all WW3 simulations.

We also want to stress that, here, we are estimating the wave impact by comparing two simulations. We thus believe that what matters the most is that all our simulations have been spun up for the same amount of time.

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

This is indeed unclear. The floe size in our study refers to the caliper diameter of the floes as defined by Rothrock and Thorndike (1984). We have added this information at the beginning of section 3 (P5L27).

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'?

We thank the reviewer for reporting these unclear expressions. The first one has been removed as the whole paragraph has been edited.

For the two other expressions mentioned, we simply removed the ambiguous word "actual", and we now refer to the 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.

We have added the following sentence in the last paragraph of the introduction:

P3L15 "We focus in particular on two aspects of these interactions: firstly the effect of including the WRS, computed by the wave model, in the sea ice model, and secondly the wave-induced sea ice fragmentation and its effects on lateral melt through the addition of a FSD in the sea ice model."

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

The estimation of sea ice-induced wave attenuation in WW3 is scaled by the sea ice concentration provided by forcing/coupling. As the WRS is directly proportional to this attenuation, it is therefore actually already scaled by the sea ice concentration. To make it clearer, we have rephrased our sentence as follows:

P6L20: "[...] does not need to be multiplied by c, as the wave attenuation estimation in WW3 (and hence the WRS) is already scaled by the sea ice concentration to account for the partial sea ice cover."

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

In our model, the FSD is indeed an areal distribution (normalized by the cell area, just like sea ice fraction). Introduction to the sea ice thickness and floe size distribution has been added at the beginning of section 3.2. We have also added the following comment:

P8L6: "From a technical point of view, the FSD in LIM3 is implemented as an areal distribution divided into floe size categories. It is advected in the same way as other sea ice tracers like sea ice concentration or thickness."

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 et Tziperman (2015) approach. The sentence from P18 L13 ‘assuming floes of different sizes. . .’ should be stated here as well.

In their study, Horvat et Tziperman (2015) are using a thickness and floe size joint distribution in order to represent the evolution of sea ice floes affected by a great variety of processes, not necessarily related to waves (e.g. welding, refreezing, ridging...). Zhang et al. (2015) approach is simpler and computationally cheaper, as it assumes that all floes of a given size have the same ice thickness distribution, allowing the FSD to be treated independently from the sea ice thickness distribution. To do so, they hypothesize that the FSD mostly results from the fragmentation of large unbroken floes randomly yielding floes of any smaller size than the original ones.

Here, we choose to follow the simpler approach of Zhang et al. (2015), as we only consider the effects of wave-induced sea ice fragmentation and lateral melt on the FSD evolution, and our formulation of lateral melt does not depend on sea ice thickness (Steele, 1992). We have added these comments at the beginning of section 3.2 (P7L10), along with the definitions of floe size and sea ice thickness distributions.

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?

This part was actually misleading and has been rephrased. In reality, Williams et al. (2017) are using a Lagrangian model, in which they advect the maximum floe size by associating it with another quantity, the “number of floes”, that is assumed to be conserved. This is not directly comparable to our case and we do not think it needs to be detailed.

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.

The wording is indeed awkward. The paragraph describing the implementation of the FSD has been largely re-written, following other comments from the reviewer.

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.

As said before, the paragraph describing the implementation of the FSD has been largely re-written. More specifically, we answer the reviewer's questions:

- Here, we assume only a single power-law exponent, as done in the studies of Dumont et al. (2011) and Williams et al. (2013). As noted by Toyota et al. (2011), the value of the exponent of the FSD found for the large floe regime that they observe is too large (>2) to be solely due to repeated break-up of the sea ice floes. It is likely resulting from other processes, welding in particular. As we do not include such processes, we do not represent this transition.
- We have added a comment to highlight that Toyota et al. (2011) study covers a small area in space and time in the discussion section:
P18L32: "This assumption is made based on the observations analyzed by Toyota et al. (2011), that only sample a small area in time and space, so that their findings may not be applicable globally".
- We have removed the reference to Dumont et al. (2011) here.

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.

Again, the part describing the implementation of the FSD and the sea ice fragmentation has been largely re-written, as we agree that the choices we have made were not justified properly. We have also added a paragraph about the choices made regarding the FSD in the discussion section. Here we also try to explain our reasoning.

- The wording of our section, with the use of the terms "forcing" and "over-ruling" was indeed a bit awkward. The right word is actually "redistribution of the FSD", just like in Zhang et al. (2015). The difference is that instead of using a redistribution scheme that will lead to power-law FSDs with a varying exponent (as the scheme used by Zhang et al. does), our scheme redistributes the FSD to make it tend towards a power law with a constant exponent. This redistribution process has indeed a strong impact on the FSD, potentially erasing the effects of advection, but so it is in nature: fragmentation by waves is an instantaneous, violent phenomenon, that completely changes the FSD (see Collins et al. (2015) for the description of a fragmentation event).
- It is difficult to quantify the number of redistributions occurring in the model as it depends on the occurrence of fragmentation events, hence on local sea ice conditions and sea states. In general, fragmentation occurrences are higher when we get closer from the sea ice edge.

- Alternative approaches for the redistribution exist, like the one suggested by Horvat et Tziperman (2015). We added a comment on this topic in the discussion (P19L1).
- Handling the sea ice fragmentation in the sea ice model is indeed an option, however it would not solve the problem raised by the reviewer of defining the value of “Dmax” from the FSD. This variable is indeed needed by the wave model to estimate the wave attenuation. The problem of how to redistribute the sea ice after fragmentation would also remain.

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.

We have performed several simulations with different numbers of categories (from 15 to 120) and different categories widths (from 2.5 to 20m) and did not find a strong sensitivity to those parameters. The results are however much more sensitive to the choice of Dmin (see our answer below).

P8 L16: Is this the same experiment as in Fig. 1? It would help the reader to re- state 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.

These are indeed the same experiments as those presented in Fig. 1. We have added the following sentence to make it clearer:

P10L4: *In the uncoupled WW3 simulation, Dmax evolves depending on the sea state, but sea ice thickness and concentration are constant. In the WW3-LIM3 coupled simulation, sea ice properties are all evolving as sea ice is pushed by the WRS, and Dmax is advected with the FSD in LIM3.*

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.

The Lupkes parametrization is indeed what LIM3 uses as standard for D in Eqn. 5, and we therefore aimed to compare the standard parametrization to the one we included following Horvat et Tziperman (2015), which depends on the FSD. It has been clarified in the text:

P10L29: *By default, $\langle D \rangle$, which represents the average floe size (referred to as the caliper diameter), is a function of the sea ice concentration obtained empirically from observational data by Lupkes et al. (2012).*

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 I suggest removing the first part of this sentence.

We actually decided to remove the whole sentence as this effect is commented in section 4.

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.

This is a good point. To quantify this sensitivity, we ran again the simulations described in section 3.3 for 3 different values of Dmin: 4m, 8m (the standard value), and 16m. We find that

the dependency is particularly strong when using the formula of Lupkes et al.2012. After 4 days, the quantity of sea ice volume melted laterally is more than doubled when D_{min} is reduced by a factor 2 (see figure below). Using the FSD to estimate the floe size significantly reduces this sensitivity, with a value of sea ice volume melted laterally after 4 days increasing by 26% between $D_{min}=8m$ and $D_{min}=4m$, and decreasing by 18% between $D_{min}=8m$ and $D_{min}=16m$.

The following figure and a new paragraph have been added in Section 3.3.

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.

We have largely edited this section to increase the readability, and to present the NO-CPL run as the reference (as this is already done in the figures). However, we do not think that we should include additional simulations and decompose even more the inclusion of the processes in the realistic set up. Indeed, we would have to compare too many runs and the text and figures would become too long and too numerous. We do believe that the current set of simulations allows us to describe and quantify the effect of each process.

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 mismatched parentheses here.

We have added a short reminder of the differences between the simulations at the beginning of this paragraph (and thus removed the note at L27).

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.

This is not straightforward due to the different natures of the floe size in these two parameterizations. When using the Lupkes parameterization, the floe size is a scalar, that cannot be directly compared to a distribution as used in the coupled simulation. Comparing the scalar with the mean floe size from the FSD would not add value here.

P12 L11: 'This result does not reflect the fact. . .' What does this sentence mean?

We have rephrased this sentence:

P14L6: *"This result masks the fact..."*

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.

The reviewer is right that our results cannot be directly compared to these previous papers. We have removed this sentence and replaced it by a statement highlighting that compensation of lateral melt enhancement by bottom melt decrease was also reported by Roach et al. (2018) and Bateson et al. (2019) (P14L12).

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.

We have again edited this section, trying our best to streamline the text and increase its readability. In an earlier draft of this paper we have tried to make individual figures corresponding to the different cases, but it would require more figures than we have at the moment. Moreover, we do believe that the current organization of the figures helps the reader to comprehend the differences between the different cases.

In this section specifically, most of our ‘not shown’ occurrences refer to the conditions before the storms... while we do believe that it should be mentioned in the text because it helps explain the difference between the cases considered, we do not think that it would add much value to the paper to show these figures in Supplementary Material.

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.

The text was actually ‘compensated by an increase of bottom melt’, which is similar to what was found by Bateson et al. (2019), as we now mention in the text.

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.

We have changed the sentence to:

P18L6: *“(ii) the effects of using the wave-induced sea ice fragmentation to estimate lateral melt”*

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.

We have modified the discussion to:

P19L22: *“Note also that we evaluate the impact of changing the lateral melt parameterization by comparing two simulations for which lateral melt depends on a varying floe size, either deduced from the FSD or estimated from the sea ice concentration using the parameterization suggested in Lüpkes et al. (2012). It differs from Zhang et al. (2016) who compare their FSD-model with a reference run without lateral melt, and from Roach et al. (2018) who use a constant floe size of*

300 m in their lateral melt parameterization. This might partly explain the discrepancies between our respective conclusions.”

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.

We agree that our sentence is not accurate, and we have rephrased it as follows:

P19L18: *“One should also remember that the studies of Zhang et al. (2016) and Roach et al. (2018) aimed to represent the evolution of floes with sizes ranging from a few cm to roughly 1 km on long time scales, whereas we focus on the important processes for wave-sea ice interactions and make the assumption that unbroken floes have a uniform floe size set to 1000 m.”*

We have checked the occurrences of Roach et al. (2018a) and Roach et al. (2018b) carefully.

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

We rephrased this sentence:

P19L28: *“Among the wave-sea ice interaction processes considered in this study, we find that the dynamical effect of the waves (the WRS) has a larger impact on sea ice conditions and sea surface properties than the modulation of lateral melt by sea ice fragmentation.”*

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

We have replaced ‘wave’ by ‘ocean surface waves’. Regarding the use of ‘sea ice break up’, we have changed it to ‘sea ice fragmentation’ as we do believe that it is a more realistic representation of the process occurring. This terminology was already used in previous studies (e.g. Zhang et al. 2015).

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.

We accounted for these remarks and fixed the syntax mistakes: The paper has also undergone rephrasing in many parts with the help of native speakers in order to make it clearer.

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

Fixed

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’

Fixed

P2 L5: -> ‘and sea ice drift’

Fixed

P2 L12: 'in the direction of the propagation'

Fixed

P2 L13: 'Southern ocean' -> 'Southern Ocean'

Fixed

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

Fixed

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

Fixed

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

Fixed

P3 L14: change 'on' to 'of'

Fixed

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

Fixed

P4 L31 'in this particular year'

Fixed

P4 L31: reword 'storms occurring during it'

Fixed

P5 L1: 'referred to as WAVE'

Fixed

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

Fixed

P5 L29: define vector k

Fixed

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

Fixed

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

Fixed

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

Fixed: The sentence has been changed to *"in order to quantify the impact of the coupling on wave, sea ice and ocean surface properties"*

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

Fixed (removing "and waves")

P14 L19: 'could in principle modified' - reword

The whole sentence has actually been edited.

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

The sentence has been changed to *"the strong storm generates high waves, inducing a WRS as large as the wind stress close to the sea ice where most of the attenuation takes place."*

P15 L14: 'pattern than' -> 'pattern to'

Fixed

P15 L32 'low concentrated' -> 'of low concentration'

Fixed

P16 L15: 'generating higher and more energetic waves'

Fixed

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

Fixed

P17 L14: 'additional lateral source melt' - reword

Fixed

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

Fixed

Fig. 7: y-axis label lists the units as %, but values on the y-axis are out of 1. I presume that the 10^2 km^3 corresponds the numbers on the figure, but this should be noted in the legend.

Fixed