In this work the authors coupled a wave model with a sea-ice model to investigate the impact of wave-induced sea-ice fragmentation on the sea-ice floe size distribution(FSD) and sea-ice dynamics. The focus is on the Barents Sea in October 2015. To study the FSD, five simulations are run: coupled and uncoupled runs with sea-ice thickness equal to 15 cm and 30 cm, and a coupled run with smaller floe size bins (more floe size categories). To study sea-ice dynamics, three simulations are run: one with a stand-alone sea-ice model (REF), one with wave radiative stress (CPL_WRS), and one with "damage" (CPL_DMG). The result is that waves modify sea-ice dynamics in the marginal ice zone (MIZ) by lowering the resistance of ice to deformation. The authors recommend that waves be included in sea-ice models to improve their forecasts.

We thank the reviewer for their careful reading of our manuscript and for their comments and suggestions. We have tried to address their questions and concerns in our response. In our comments, PXLY refers to page X line Y of the updated manuscript (attached to this response).
In the updated manuscript, the main changes concern:

- The Introduction, which has been largely rewritten to clarify our motivations, and in which we shortened the description of previous FSD implementations in sea ice models, as it is not the core of our study.
- The FSD implementation section (2.2), in which we rewrote our motivation for the introduction of a second FSD to clarify its use. We also rewrote the part concerning the redistribution of the FSD to clarify the links between our model and previous studies and discuss more the assumptions we made.
- Section 4.2.1 in which the FSD is discussed more carefully following comments of reviewer #1 and #3.
- The Discussion, in which the estimation of the extent of broken ice is discussed more carefully.

My main concern is with the FSD analysis. See page 14, lines 20-21, in reference to Figure 3: "we can distinguish two regimes separated by a cut-off floe size..." Look at Figure 3(a). I do not see two regimes separated by a cut-off floe size, and I don't believe that any statistical test would support such a conclusion. Look at the green curve for latitude 74.2 degrees north. It appears that a "line" has been fit using exactly 2 data points (see the green dashed line). By this method of analysis, one could distinguish a new "regime" for every pair of points. The purple and red dashed lines appear to be based on 3 data points. To my eye, all three curves appear to gradually steepen as the floe size increases. I don't see a cut-off or a regime shift.

We acknowledge that the distinction between the two regimes is somewhat arbitrary. However, looking at the study by Toyota et al. (2011) who first suggest this distinction, their distributions also gradually steepen, and the existence of the two regimes and a cut-off floe size have been contested numerous times before, as raised by reviewer #1 and in the paper you suggest to reference below.
However, the question of whether FSDs follow power-laws with a cut-off floe size or not is not the topic of this paper. Whether this interpretation is wrong or not, it has been used to calibrate wave-in-ice attenuation in the wave model we are using, and it is therefore of interest to know how the FSD we produce compares with the FSD assumed in the wave model before. To this purpose, we want to know what is the exponent of a power-law FSD fitted to small floe categories (it determines the weight given to small floes in the FSD, which can impact scattering), and where Dmax is located compared to the two regimes that could be deduced by fitting two lines like in Toyota et al. (2011).
We have rewritten section 2.2.2 that introduces the redistribution of the FSD to make our motivations clearer. We detail what are the evolutions brought by our study compared to

previous wave-in-ice models using FSDs. In section 4.2, we do not claim that the FSDs we produce are realistic, as there is no consensus about what should be the shape FSD resulting from wave-induced fragmentation. Instead, we present the FSDs we get and still fit lines to the small floes categories, but in order to discuss how our FSDs compare with the fixed-exponent power-law FSDs assumed for small floes in previous waves-in-ice studies and the observations reported by Toyota et al. (2011).

The authors cite Toyota et al (2011) numerous times in the context of concave-down cumulative distribution functions (CDFs) with two regimes. A counterpoint may be found in this paper:
Stern, H.L., A.J. Schweiger, J. Zhang, and M. Steele, 2018. On Reconciling Dis-parate Studies of the Sea-Ice Floe Size Distribution, Elem Sci Anth, 6: 49. DOI:https://doi.org/10.1525/elementa.304In particular, see their Figure 3 and the section called "Break-point analysis".

References to this paper have been added in various places of the text, as it is a nice reminder of the strong assumptions made for the FSD in this study (and in all other wave-ice interactions models based on Toyota et al., 2011).

Page 23, Appendix B. "The shape of the CDFs shown in Figures 3 and 5 strongly depend on the parameterization detailed in section 2.2.2. The value of the cut-off floe size at which the transition between the small and large floes regime happens..." It seems highly undesirable that the shapes of the CDFs depend strongly on the parameterization. This would seem to inject a high degree of uncertainty into the whole simulation. And again, I question that a well-defined cut-off exists between small and large floes.

We agree with this statement. What we meant here is that in the absence of a consensus concerning the shape of the FSD, and with the little knowledge we have of the physics of sea ice break-up due to waves, the shape of the redistributed FSD depends on the hypothesis made in the redistribution process. These hypotheses are however necessary at this stage, and the ones we make are almost the same as the ones in the model by Williams et al. (2013) and have been re-used in many wave-ice interactions studies since. These hypotheses originated from the work by Toyota et al. (2011), and as noted in the previous comments, have been contested since. The only differences with the model by Williams et al. (2013) are that:

- Instead of having a well-defined cut-off floe size with a sharp steepening of the CDF, we have a progressive steepening of the CDF, which is more coherent with the observations reported by Toyota et al. (2011). A sharp steepening of the CDF like in Williams et al. (2013) is all the more unsatisfying as the steepening reported by Toyota et al. (2011) might be the result, at least partly, of windowing issues, as raised in the previous comment. To obtain a more progressive steepening of the CDF, we introduce a continuous function for the probability that an ice floe breaks up instead of a step function in the model by Williams et al. (2013). The steepening of the CDF depends on the values of c2,FS and c2,λ, which are the only two new coefficients introduced by our study. The model by WIlliams et al. (2013) is equivalent to having c2,FS and c2,λ tending towards 0. We found that setting c2,FS and c2,λ to 2 was a good compromise between a progressive steepening of the CDF and coherence with the truncated power-law FSD used to calibrate the wave model.
- Williams et al. (2013) assume that the FSD of the "small floe regime" follows a power law with a constant exponent set to ≈-1.85. This value originates from the work by Toyota et al. (2011) by assuming that, if waves can trigger flexural failure, then the probability that a floe breaks up is always 0.9. As written above, we already introduced a continuous function for the probability that an ice floe breaks up. Therefore, we

substituted the value of 0.9 by our probability function. As a result, the exponent that we obtain when fitting a power-law to the “small floe” regime is allowed to vary, just like in the observations reported by Toyota et al. (2011).

The section 2.2.2 has been largely rewritten to clarify the choices made for the values of our parameters, and to make more apparent the links between our parameterization and the one initially described by Williams et al. (2013), including the comments above. As the hypotheses we use for the FSD redistribution are still mostly based on the work by Toyota et al. (2011), we insist on the potential caveats of this study, and in particular the fact that it is unclear whether a well-defined cut-off exists or not. As we describe in more detail the role of each parameter in the redistribution, Appendix B was found to be useless, and we have therefore removed it. We have also rewritten paragraphs in section 4.1, and now we only use the CDFs to discuss how the changes we introduced may affect wave attenuation compared to the FSDs assumed previously in wave attenuation models.

Minor Comments

Page 1, line 25. It looks like Lemieux et al (2016) is about landfast ice, not the sea-ice edge.

Yes, we were actually thinking about another paper by Lemieux et al. (2016) focusing on a Regional ice prediction system. We eventually found a publication by Shweiger & Zhang (2015) that was more appropriate here.
Schweiger, A. J. and Zhang, J.: Accuracy of short-term sea ice drift forecasts using a coupled ice-ocean model, Journal of Geophysical Research: Oceans, 120, 7827–7841, https://doi.org/10.1002/2015JC011273, https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1002/2015JC011273, 2015.

Page 6, line 21. "floes in the largest floe category are not affected by lateral melt." I don’t see how equation (4) reflects this statement.

We only mentioned it in the text, as adding the very special case of this category to equation (4) would deteriorate its readability in our opinion. Besides, this is only a choice we made here as we are not interested in resolving large floes of size >O(100m), the general case remains well described by equation (4) as it is. We rephrased the sentence to make our motivations clearer:
P7L12: *Here, we neglect lateral melt for the largest floe size category as floes with size O(100) m and more are not resolved in this study and are expected to contribute very little to lateral melt.*

Page 7, lines 2-3. "a uniform FSD made of the smallest floes ... evolves into a uniform FSD made of the biggest possible floes." This does not make sense. A uniform FSD contains floes of all sizes, in equal proportions. The authors probably mean a delta-function FSD, in which all floes are of the smallest size, evolves into a delta-function FSD, in which all floes are of the largest size.
And
Page 7, line 4. Check whether "uniform FSD" is appropriate here – see previous comment.

This is true, and we changed our formulation following the referee’s comment.

Page 7, line 6. "setting kappa = 5 x 10^(-8)" kappa is a rate (see line 1 of page 7).Please give the units

The units (m-2 s-1) have been added to the text.

Page 8, equation (8) and following. You need to say that Y is Young's modulus, nu is Poisson's ratio, and h is ice thickness. Please give values of DFS for h = 15 cm and h= 30 cm.

We added the missing variables to the text, as well as a comment on values of DFS (P10L6).

Page 10, equation (12). This equation is not correct – it is missing a factor of D inside the integral. If g(D) is a probability density function then the mean value of D is the integral of D*g(D) dD.

This is right, it has been corrected. It was only the case in the text, not in the model.

Page 10, lines 21-24. Dmax is supposed to be one order of magnitude larger than the longest wavelength, but lines 23-24 imply that Dmax does not become larger than 1000m. Shouldn't Dmax be 10 times larger than 1000m?

Wavelengths associated with storm swells are in general of the order O(100)m, setting Dmax to 1000m for unbroken sea ice ensures they are dissipated by flexion dissipation. We actually rewrote this part of text to make these motivations clearer.

P12L10: *Besides, the flexion dissipation mechanisms included in WW3 by Boutin et al. (2018) require to discriminate between a sea ice cover made of large floes with size of the order of O(100)m and an unbroken sea ice cover for which the default Dmax in WW3 is set to 1000m. This is because flexion only occurs if the wave wavelength is shorter or of the same order as the floe size. Knowing that long swells can have wavelengths of the order of O(100m), they will only be fully attenuated by inelastic dissipation if floe size is of the order of O(1000)m, which can be larger than the floe size range covered by the FSD defined in neXtSIM. In the case whereD_N<1000m, to make sure that swells are still attenuated in an unbroken sea ice cover by WW3, we linearly increase the value o fDmax sent to WW3 from Dmax=D_N to Dmax= 1000m with the proportion of sea ice in the largest floe size category* $\int_{D_{N-1}}^{D_N} g_{slow}(D)dD/c$

Page 11, end of Section 2. There are a LOT of parameters and empirical functions in this work. It might help to collect them in a table. My list includes these parameters: Gr, c_new, and beta_weld from equation (4); kappa from page 7; tau_heal from equation (5); tau_WF from equation (7); lambda_break, c_1FS, c_2FS, c_1Lambda,c_2Lambda, d_w, DELTA_t, Dmax. And these empirical functions: q (equation 11c),pFS (equation 9a), pLambda (equation 9b), beta (equation 10), Q (equation 7), and c_broken (top of page 11).

We have collected all these parameters and others in a table that we added in an appendix (Appendix A1).

Page 15, line 5, and throughout the paper. Dates are given in the form day/month/year, as in 01/10/2015 for 1 October 2015. Perhaps this is standard notation for The Cryosphere. Just be aware that it will confuse readers from the U.S., who will interpret "01/10/2015" as January 10, 2015. If you switch to the format "1 October 2015" it should be clear to everyone. Just a suggestion.

We followed this suggestion. It also seems to be what is recommended by the journal guidelines.

Page 17, lines 24-25. I can't see the convergence north of Svalbard nor the divergence at the center of the domain in Figure 11d.

This is true, convergence and divergence of sea ice can be seen on Fig.11c, not d. Reference to this panel has been added in the text.

Page 20 line 35 and page 21 line 1. "The sensitivity to tau_heal was investigated by re-running our experiments using this time tau_heal = 15 days..." You might want to remind readers that the default value is 25 days, because they probably won't remember (from page 11, line 27)

We rephrased Page 20 line 35 to give a reminder and a bit more context to the reader.
P23L23*: [...] Its impact was investigated by re-running our experiments using this time* τ_{heal}*=15 days instead of 25 days, the default value in neXtSIM. 15 days corresponds to the lower limit for which neXtSIM reproduces well the multi-scaling of sea ice deformation (Rampal et al., 2016), while 25 days is close to the upper-limit of this range.*

.Page 22, lines 1-2. "waves pose a hazard as they make sea ice thicker" – this must be during freezing conditions, not during melting conditions, right?

This is right, and we actually removed this reference to thickening in the sentence.

Page 22, equation A1. What is G? What is "k" in the function N(k)? Is it supposed to be k_i?

Appendix A has been removed as it was adding more confusion than referring to section 2.3 of Boutin et al. (2018), which is a step by step description of the break-up process in WW3.

Page 22, line 24. Is k_i,max the same thing as the quantity inside the square root on the right-hand side of equation A1? If yes, then wouldn't it make sense to first define k_i,max = max( ) (as in A1) and then lambda_break = 2*pi/k_i,max? And then go on to equations A2 and A3, if necessary?

We thank the reviewer for this remark as it made us realize that (i) the definition of lambda break we gave was wrong (it corresponds to the shortest wavelength for which the wave-induced stress exceeds sea ice resistance to flexural failure) and (ii) this section contained a few mistakes and was actually quite misleading. We decided to remove Appendix A from the manuscript, and to instead refer to section 2.3 of Boutin et al., 2018 that explains the determination of lambda_break with the right level of details.

Page 29, Figure 3. In panel a, the symbols are plotted at the mid-point of each bin. For example, the smallest bin represents floes of size 10-20 meters, and the symbol is plotted between 10 and 20 meters. But in panel b, the symbols are plotted at the left end of each bin. For example, the smallest bin represents floes of size 5-10 meters, and the symbol is plotted at 5 meters. So the data in panels a and b are not plotted consistently.

We updated the two panels to make the plotting of our data consistent. The computation of the exponents of the fitted power-laws was also not consistent between the two panels and has therefore been redone. The changes in the new exponent values we obtain are quite small and do not require modifications in the text.

Typographical Notes

Page 2, line 13. "to conclude on" should probably be "to arrive at"
Edited
Page 5, line 6. "recovered" should be "covered"
Edited
Page 5, line 21. "the caliper diameter" should probably be "the mean caliper diameter"
Edited
Page 5, line 28. Delete the word "respectively"
Edited
Page 6, line 9. "associated to this process" should be "associated with this process"
Edited
Page 9, line 8. "B" should be "Appendix B"
Edited
Page 10, line 7. "ran" should be "run"
Edited
Page 10, line 8. Capitalize "Appendix A"
Edited
Page 10, line 27. Capitalize "Introduction"
Edited
Page 11, line 3. "in general of at least" – delete "of"
Edited
Page 11, line 22. "Wave-current [not currents] interactions"
Edited
Page 11, line 31. "similarly" should be "similar"
Edited
Page 13, line 3. "ran" should be "run"
Edited
Page 14, line 5. "Similarly" should be "Similar"
Edited
Page 14, line 28. "presented on 3" should probably be "presented in Figure 3"
Edited
Page 15, line 8. "large lambda values" – is this lambda_break?
The whole sentence has been rephrased:
P17L21: *In pack ice, where floes are larger than at the ice edge, the speed of the floe size growth in the "fast-growth" FSD is mostly controlled by welding, and therefore depends on the value chosen for rate of decreases of the number of floes kappa.*
Page 15, line 14. "CDFs (b,c)" should be "CDFs (b,d)"
Edited
Page 15, line 14. "at the time of shown" – delete "of"
Edited
Page 15, lines 18-19. "flatten the slope of the large floes regime" should be "flattening of the slope of the large floe regime"
Edited
Page 16, line 3. Delete "that"
Edited
Page 16, line 4. "16 and 60 meridians" should be "16E and 60E meridians"
Edited
Page 16, line 31. "sea ice produce" should be "sea ice produces"
Edited
Page 17, line 13. Delete "is responsible
Edited
Page 17, line 16. "wave" should be "waves"
Edited

Page 18, line 28. "exceeds the one of the wind stress" should be "exceeds that of the wind stress"
Edited
Page 18, line 35. Something is missing after the word "REF"
Edited
Page 20, line 7. "opposes" should be "poses"
Edited
Page 20, line 27. Delete the word "a"
Edited
Page 23, lines 4 and 9. The parameter "c_1,FSD" should be "c_1,FS" (see page 9, equation 9a and following).
This sentence has been removed as details on the role of c_1,FS are now given in section 2.2.2.
Page 23, line 4. "Basically, if c_1,lambda lambda_break > c_1,FS D_FS"
This sentence has been removed as details on the role of c_1,FS are now given in section 2.2.2.
Page 23, line9. "Oppositely, if c_1,lambda lambda_break > c_1,FS D_FS" But the inequalities onlines 4 and 9 are the same, not opposite.
This sentence has been removed as details on the role of c_1,FS are now given in section 2.2.2.

Page 24, line 10. "Tech. rep." is not enough information to locate this technical report.

We replaced this reference by a more recent one:
Yumashev, D., van Hussen, K., Gille, J. et al. Towards a balanced view of Arctic shipping: estimating economic impacts of emissions from increased traffic on the Northern Sea Route. Climatic Change 143, 143–155 (2017). https://doi.org/10.1007/s10584-017-1980-6

Figures
Figure 2. (i) Consider labeling Point Barrow in the lower left corner of a, b, c. (ii) What are the solid and dashed curves in a, b, c? (iii) In panel b, it's almost impossible to see the green cross. (iv) In panel b, what are the black arrows? (v) In panel c, it's impossible to tell whether black represents +100 or -100. Both values are black on the color scale. (vi) In panel d or in the caption, say that the distance along the transect (km) is from north to south.

(i) Done
(ii) They represent contours of sea ice concentration equal to 0.8 and 0.15 respectively.
(iii) All crosses have been made larger and bigger to be more visible.
(iv) They represent the wave mean direction. It is now stated in the caption.
(v) We have truncated the divergent color scale at both ends. Extreme values now correspond to lighter blue and red, which improves the readability of our figures.

Figure 3. "Cumulated" should be "Cumulative" in the axis labels and in the caption.
Edited.

Figure 4. The last sentence of the caption refers to a cross. I don't see it.
The cross has been made bigger and we now mention the panels where it can be seen.

Figure 5. (i) In the caption, "cumulated" should be "cumulative". (ii) The caption should probably say that the histogram bars at 200+ meters in panels a and c represent un-broken ice.

(i) and (ii) : We have edited the caption as suggested.
Figure 7. In the caption and the legend, "meridian component" should be "meridional component".
Edited.

Figure 8. In panel d, it's hard to tell the green arrows from the blue arrows.

The arrows are now blue and red. They are also bigger, slightly less numerous, and over a green color scale. It should be easier to read.

Figure 9. (i) In b and d, it's impossible to tell whether black represents +0.25 or -0.25. Both values are black on the color scale. (ii) The caption says that panels a and c are"damage" but the x-axis labels in those panels say "Sea ice thickness". (iii) The caption refers to green and blue arrows in panels b and d. I don't see them.

(i) We have truncated the divergent color scale at both ends. Extreme values now correspond to lighter blue and red, which improves the readability of our figures.
(ii) It has been corrected.
(ii) This sentence was in the wrong place, we removed it. There are no arrows in panels b and d.

Figure 10. In panels a and b in the legend, "DMG/WRS" should probably be"CPL_DMG".
Edited.

Figure 11 (b,d) and Figure 12 (all panels). Same comment about the color scale – both ends are black. How can we distinguish the highest values from the lowest values?

We have truncated the divergent color scale at both ends. Extreme values now correspond to lighter blue and red, which improves the readability of our figures.

Figures 2, 4, 6, 8, 9, 11, 12. Why not make all the panels larger?

The size of the figures correspond to the one prescribed by the template provided by Copernicus.