This is a review of Boutin et al (2020) - who explore the interaction of wave fracture and improved rheological modeling in the neXtSIM model. The paper is interesting and a piece of model development that ought to be done and published. My major comments are on their 2-FSD parameterization.

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.

Main comment: a few questions about your model.

In your 2-FSD implementation, I would like some more clarity on the meaning of the "mechanical" FSD - this is an interesting idea. My read is the point is to provide memory of past deformation - but how is this separate from the role of damage in neXtSIM/MEB?

It is indeed a good question, and we agree that we were not clear enough on this point. The short answer is that damage provides a qualitative memory of past deformations (a FSD would not be needed for that), while the mechanical FSD provides more quantitative information on the last fragmentation that occured.

The damage variable provides a qualitative estimate of the density of cracks in the ice associated with deformation events that can be due to winds, ocean currents, waves etc. Damage increases every time intense deformation events occur, and then reduces slowly with time, thus keeping a memory of previous deformation events.

It remains however a very qualitative information, useful in the case of ice dynamics, but that is very hard to transform into a more quantitative information (e.g the quantity of leads, floes, ridges in the mesh element). Conversely, the FSD provides quantitative information: a high proportion of small floes means that the density of cracks in the ice is likely to be very high, and that the damage value should be high too. In this way, it is therefore relatively easy to write a relationship between the FSD and the value of damage, but it does not work in the other way around: knowing the value of damage does not tell us much about what the FSD could look like (just like the spatial distribution of a quantity cannot be inferred from an integrated value).

Then, why do we need to keep a memory of the FSD? After a fragmentation event, once refreezing occurs, floes start to be aggregated together by welding together, or by joints of thin ice forming at the ocean surface. The ice layer that is formed might be continuous, in a sense that there is no lead, but remains heterogeneous as the mechanical properties of individual floes differ from the one of the continuous ice layer. For some processes like wave attenuation, the quantity that matters are the elasticity of the ice cover (scattering, dissipation associated to the flexion of ice) and its heterogeneity (scattering). For these processes, the length scale of interest is more likely to correspond to the one of individual consolidated floes than to the one of the continuous ice cover made of recently aggregated floes. The "mechanical FSD" keeps a memory of this information. Another motivation to implement this FSD is that in the case where a fragmentation event occurs in a sea ice cover made of recently

aggregated floe, the thin ice joining the floes is likely to break very quickly, just like at a larger scale cracks between ice plates can be re-activated. This failure of the joints will make the FSD return to a state close to the FSD resulting from the latest fragmentation event, and this is this state that the “mechanical” FSD keeps in its memory.
In order to address these comments, we have largely rewritten the beginning of section 2. to make our motivations for the introduction of the “mechanical” (that we now call “slow-growth”) FSD clearer. We have also added a comment in section 2.4:
P13L1 : *Note also that floe size and damage are not explicitly linked by this relationship, but the relaxation time associated with the healing of damage and of the "slow-growth" FSD are the same, making their evolution parallel in the regions of broken ice.*

If one wanted to compare your output to observations, how would you do that?

The evolution of the “thermodynamical FSD” could be compared to “classical” FSD observations (from aerial photography for instance). In theory, the “mechanical” FSD is linked to mechanical properties of the sea ice cover, and could be inferred from local and repeated in-situ measurements of small-scale spatial thickness distribution in the MIZ to distinguish between “homogeneous thick floes” and “thin ice joints”. The FSD observations would need to be at a sufficiently high temporal frequency (at least 1 per day) to monitor their evolution, and should be used in conjunction with information on the wave state. To evaluate the impact of fragmentation on sea ice deformation, Radar Doppler measurements of ice drift such as those proposed for future satellite missions like SKIM would be excellent; in situ drifters could also help if there were enough and if they were in the ice long enough - at least a few weeks, to be able to identify fragmentation events and determine its impact on the variability of sea ice drift.

Why should we expect your mechanical FSD to look like the thermodynamic one, i.e. obey the same evolution equations?

Both FSDs describe the evolution of floe size in the ice cover, but with a different definition of what the floe size is. The “thermodynamical FSD” considers the floe size as the length scale of the continuous ice cover: it ignores the heterogeneity within this ice cover, for instance if it is made of individual consolidated floes joined together by thin ice. The second “mechanical FSD” considers the floe size as the length scale of individual consolidated floes, even if they are joined by a thin ice layer. As a consequence, these two FSDs are not independent from each other, they undergo the same processes, mechanical and thermodynamical, at the same time. They should therefore follow the same general equations of evolution. Actually, these two FSD only differ after fragmentation has occurred:

- Floe size in the thermodynamic FSD will regrow quickly as the ocean surface refreezes.
- Floe size in the mechanical FSD will regrow slowly, as the healing of the cracks between individual floes takes several days to several weeks.

To make this clearer, and following the suggestions of Referee #2, we actually decided to rename the two FSD “slow-growth” and “fast-growth” instead of “mechanical” and “thermodynamical” that were misleading. We have also largely rephrased the introduction to section 2 to make this distinction between the two definitions clearer to the reader.

Why should the mechanical healing term look like the thermodynamic one, couldn’t it evolve independently? I think a figure to add would be plotting the mean floe size for both FSDs in time for the period documented in Fig 13, even for just a single grid cell.

The two FSD are using independent healing rates, as described in the answer to the previous question. Moreover, the healing of the slow-growth (mechanical) FSD is a relaxation towards the fast-growth (thermodynamical) FSD because the two are not really independent. Mechanical healing depends on refreezing, as it only represents the additional time needed for the ice to thicken and strengthen.
We don't think that plotting the evolution of the mean floe size in the two FSDs would be of great interest here. The links between thermodynamics and the FSDs are not the main topic of our study: floe size does not impact the amount of ice that is formed, and we focus on a time period with negligible lateral melting. The timescales associated with floe size growth are discussed in section 4.1. The mean floe size evolution in a grid-cell for the slow growth FSD is equivalent to the maximum floe size Dmax, and its spatial distribution is similar to the one of Dmax (but with lower values) already shown in Fig. 4 and Fig. 6.

The impact is clearly seen in Fig 4. Were I designing a separate depiction of sea ice fracturing, I'd expect it to be most relevant in the interior pack - this is where FSD models don't get crack features right yet. This mechanical FSD implementation seems to pinch in near the margins, not in dynamically active but waveless regions, but the neXtSIM model does get damage in the interior, doesn't it?

Yes it does, damage in neXtSIM is defined everywhere there is ice. However, inverting the damage, or modelling the floe sizes produced by a fracturing event in the pack is an extremely difficult problem, out of the scope of our study.

This leads me to believe that there is a difference between the description of where the mechanical FSD would be relevant (Sec 2.2, i.e. interior regions with leads) and where your model makes it relevant (exterior regions with waves). I think this approach is potentially fruitful for fixing the problem of bad pack ice fracturing, but you may be approaching it from the wrong place!

We agree that the links between FSD and damage could be of interest for future model developments, however the focus of this study is wave-sea interactions, which are only relevant in the MIZ.

Minor Comments:
Please remove the mention of eddies from your abstract - the role of the ocean is not explored here except to cite a couple papers in the conclusions.
Done
P2 L4 - I think you forget to explicitly mention the second main process?

We rephrased this paragraph following your comment and a remark from Referee #2. The introduction sentence is now: *Waves can impact sea ice dynamics in the MIZ through a variety of processes.* (P2L3)

Pg 2 - Using the power law FSD, especially in early days, is fine, but just note that meta-analyses (Herman 2011, Stern 2018) and new datasets (Horvat et al 2019) indicate an absence of power-law tails. Still... it is tough to justify (or putting the cart before the horse) designing a model that gets an answer, and then forcing its conservation via parameter choices. This is a particular problem because it is majority opinion that the "cutoff" power laws observed by Toyota and others are an artifact of the use of CDFs and finite measurement windows (Burroughs and Tebbens 2001, Stern 2018) not physics. Now a model has been designed (more than one) that produce these distributions. But you have no windowing issues (so no expectation of a truncated power law distribution) or sampling issues (so no need to produce a CDF). I'd advise plotting the FSDs proper alone (as you do in Fig 5), living with the results. At these early

days, you'll be forgiven for having weird distributions, and for making changes to your models, too.

We have clarified our motivations for the redistribution towards a power-law FSD. We would however want to keep the CDFs here (while adding the comments raised by reviewers #1 and #3 on the limits of their use).
Our reason for keeping the CDFs is that they illustrate how our FSD redistribution model compares with the assumptions made previously in the studies of Dumont et al. (2011) and Williams et al. (2013), who followed the interpretation of the CDFs made by Toyota et al. (2011). This is needed here, as the FSD in neXtSIM is mostly used to provide the wave model with information on the floe size evolution when fragmentation occurs. The wave attenuation parameterization we use here has been evaluated before with a power-law FSD truncated at a cut-off floe size, it is therefore of interest to know how the FSD in our coupled model compares with these assumptions.
We have changed in the updated manuscript the way we discuss these CDFs (section 4.2.1). We don't use them to claim that our model reproduces well observations of FSD anymore, instead we discuss how our FSD redistribution scheme may affect wave attenuation compared to other wave-ice interactions studies, and in particular the FSD . We clearly mention the fact that using CDFs can be misleading to understand the FSD, and that the floe size cut-off and the distinction of two regimes are a way to interpret the CDF following Toyota et al. (2011), not necessarily a sign that the model reproduces "real" FSD well.
The paragraph introducing the FSD redistribution (2.2.2) due to fragmentation has therefore been largely rewritten, with extra-care brought to our motivations and the limits of our approach. We also added a sentence in the Discussion that reminds the reader that the "cutoff" power laws are certainly not the way to go in the future.
P25L2: In these early days of the implementation of FSDs in sea ice models, we have built on what was done in wave-in-ice models and used a redistribution scheme that yields FSDs relatively similar to the ones described by Toyota et al. (2011), although their methods and interpretations have been contested (Stern et al., 2018; Horvat et al., 2019)

Pg 3 L 5 and otherwise - (ITFSD –> FSTD).

It has been edited as suggested.

Pg 5 L 30 it has been pointed out by Stern (and a wide literature from applied math, see Virkar and Clauset 2014) that fixed-width bins will bias your ability to represent or examine scale-invariant behavior.

This is right, and we added this comment in the text:
P6L24: *Using fixed-width bins may bias our ability to represent or examine scale-invariant behaviour (Stern et al., 2018) but it has the advantage of being simple, and the study of the FSD evolution and its impact on sea ice is out of the scope of this study.*

P 6 - I think the most updated Roach model was published in 2019 and included coupled wave-ice physics. Might provide better sourcing for the comparison here.

We added this reference in the manuscript, but not in this section as the implementation we refer to is described in detail in the 2018 paper.

P 6 L29 - do you mean that once the concentration is high, all the ice is in the highest size category? Is this also true for the mechanical FSD, or do you still require the relaxation?

Once the concentration is 1, sea ice is supposed to cover all the area of the element and the model therefore considers that in the point of view of the thermodynamical FSD (which considers that floes are elements of ice separated by leads), all the ice is in the highest size category. However, the mechanical (slow-healing) FSD, which retains the memory of prior fragmentation events for longer, still requires the relaxation.

P 7 L 25 "a quick and violent process" is a wonderful phrase albeit not exactly accurate. I know I should object scientifically but I really like it.

Reviewer #2 was less sensitive to the “wonderfulness” of this sentence and objected more willingly. We now motivate our choice for the relaxation time of the “fast-growth” FSD towards the “slow-growth” FSD with the following arguments:

P8L29: We justify this short relaxation time by the fact that (i) waves can fragment a consolidated sea ice cover in a few tens of minutes only (Collins et al., 2015) and (ii) the "fast-growth" FSD g_fast is only used for thermodynamical processes associated with timescales of at least a few hours, and is therefore relatively unaffected by the choice of a relaxation time value one order of magnitude lower.

P9 L 25 - See earlier comment. At the very least, please explain these parameter choices naturally through your model design not as a post-facto requirement.

Following the comments on the CDFs by reviewers #1 and #3, we rewrote this section. The origin of all these parameters is now explained more clearly. We emphasize in particular the changes we made compared to the model introduced by Williams et al. (2013), and our motivations for these changes. It made Appendix B relatively useless, and we have therefore removed it. Instead, we have added a Table summarizing all the parameters we use in this study, as suggested by reviewer #3.

P13 L 5 "it also includes storms" - could you be more clear about what you mean here?

We have tried to clarify our sentence as follows:
P14L29: *This period of the year is also characterised by the combination of a low sea ice extent (thus a large available fetch) and regular occurrence of storms in the Arctic, which increases the opportunities to evaluate the impact of waves on sea ice with fragmentation events over wide areas.*

P13 L21 - What does it mean "very satisfactory results"? What is the metric?

The sentence has been rephrased:
P15L20: “[...] *shown to give a good match with observations for both the extent of broken ice and the wave attenuation in this particular case.”*

P13 L27 - "Perfectly acceptable given the uncertainties" - I’m not sure what you mean - which is perfectly acceptable, and why does this relate to wave attenuation uncertainties?

Ice break-up is determined by wave properties, therefore the extent of broken ice depends directly on wave attenuation. In this section, we switched the order of the wave attenuation and the broken sea ice extent paragraph and rephrased to make the relationship between the two more explicit.
P16L5: *Although the extent of broken ice is slightly smaller in the coupled run, the difference does not exceed 2 grid cells, therefore representing a distance of about 25km, which is*

acceptable given the large uncertainties associated with wave attenuation in ice (see for instance Nose et al., 2020).

P15 L2 - why not show this contour?

As thickness is not a smooth field, there are a lot of very small “spots” of ice over 1m which deteriorates the readability of the panel when we plot this contour.

P16 L1 - "It is particularly true..." - rewrite?

This paragraph has been rephrased. The sentence is now:
P18L15: *The available fetch in particular remains relatively constant, and is large enough to allow for storm waves to penetrate far into the ice.*

P16 L17 - A bit confused here, "regenerate unbroken ice" isn’t really the process - healing between floe joints is how you describe it.

We have substituted the word “regenerated” by “heal”, as it is indeed more suitable here.

Fig 3 - Again I’d advise not using the CDF here, preferring the FSD because as pointed out by Stern (2018) the CDF gives a false impression of scale-invariance, and P19 - I would prefer a clearer description of this process. In effect, you are saying that the influence of fragmentation (at least in your model) is not because of wave events, but after them when the sea state relaxes?

We have added the comments to the drawbacks of using CDFs, but kept the CDFs in the plots as discussed above.
Concerning your comment on P19: Fragmentation in the model can only occur because of wave events, and this increases the damage variable which in turn can influence the ice drift. What we see from our model results is that the drift of sea ice damaged by waves is not modified during extreme events, but instead is modified after these events, when wind speed lowers but damage remains high. This is because fragmentation events in our simulations coincide with high winds, and these high winds are able to deform the ice cover whatever the internal stress of ice is. When the wind speed lowers however, sea ice deformation is only possible if the internal stress of ice is low, i.e if sea ice is not compact or has been damaged. We have rephrased the paragraph as follows:
P21L30: *In our case study, the damage added by wave-induced sea ice fragmentation does not significantly enhance sea ice deformation during wave-induced fragmentation events, but after them, when the sea state relaxes. This is because these fragmentation events coincide with high wind speeds, with wind stress dominating the internal stress of sea ice in all simulations, whatever the level of damage is. Once the wind speed lowers, the internal stress of sea ice dominates over the wind stress in places where sea ice is compact and not damaged, and limits deformation. However, in the regions that have been previously damaged by wave-induced fragmentation, the level of damage remains high in the first days following the storm, and sea ice can still deform relatively freely. This high level of damage significantly enhances sea ice mobility in the MIZ in the CPL_DMG simulation compared to CPL_WRS. This behaviour of the MIZ, with fragmentation events followed by calm periods during which sea ice mobility is enhanced, is not limited to the particular event we describe here. In the Barents Sea, for instance, maxima in the difference between ice drift velocities in the CPL_WRS and CPL_DMG simulations during October 2015 occur after maxima in the ice drift velocity magnitude (Fig. 13, and we noted a similar behaviour in the Greenland Sea (not shown).*

P20 L 35 - "it depends on two factors" but then you mention it doesn't depend on reducing t_heal to 15 days. Also, this isn't a sensitivity experiment as you haven't also increased the healing time. You also don't really address the sensitivity to attenuation just mention it is uncertain.

This is true, and we replaced "depends on" by "could be affected by".
We have not increased the healing time, as 25 days is already close to the upper limit of the range of values for which neXtSIM reproduces well the range of deformation (Rampal et al., 2016).
For the sensitivity to attenuation, we have rewritten the paragraph so that:

- We refer the reader to Ardhuin et al. (2018) and Boutin et al. (2018) in which sensitivity of the extent of broken ice to wave attenuation in WW3 is already extensively discussed.
- We have strengthened and clarified our discussion on the sensitivity of our results to wave attenuation, in particular the comparison with wave attenuation parameterizations used in other studies.