This paper follows a series of recent works by the authors and others in coupling ocean waves and sea ice in large scale models. Here, the Wavewatch III and neXtSIM models are coupled, and simulations with different levels of coupling are compared, with a focus on fragmentation of the ice cover and resulting changes in ice dynamics. The main contribution of the paper is the inclusion of FSD memory, by using two FSD functions where one FSD (the "dynamic" FSD) evolves more slowly than the other one (the "thermodynamic" FSD). This is an interesting new avenue in sea ice modelling, and I'm surprised it hasn't been highlighted in the title of the paper.

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.

The authors motivate the memory by saying "there are reports of waves breaking ice at weak points such as refrozen leads and pressure ridges (e.g. Kohout et al 2016)" (p4, l3–4), but only ever refer to the one paper (Kohout et al, 2016). Are there any more reports of this kind? If so, give them. If not, weaken the motivating statement. In either case, it would be useful to give brief descriptions of the events reported.

To the best of our knowledge, only Kohout et al. (2016) mention that break-up occurs at a weak point in the ice cover, and we, therefore, rephrased our motivating statement (P6L4).

We have also removed this motivating statement from the introduction to include it at the beginning of section 2.2, with more details. We think it improves the clarity of the paper. In particular, we wanted to insist on the difference between the damage variable and the "mechanical/slow-growth" FSD. The memory of previous deformation events in our model is contained in the damage variable, not in the FSD, and this is the link between fragmentation and this damage variable that we found to have an impact on sea ice dynamics. This is why we prefer to insist on the "Brittle-rheological framework" (hence the damage) in the title than on the second FSD (although we think it is an interesting addition to describe the mechanical nature of the sea ice cover).

I found the names of the FSDs difficult, as both depend on dynamic and thermodynamic source terms. Calling them,e.g., slow and fast would be easier (and probably more accurate).
We agree with the statement that these names were misleading. We have changed their names to "slow-growth" and "fast-growth", as it will make the reading easier.

Another motivation given for the study is that “neXtSIM is now including a Maxwell-Elastomer-Brittle rheology” (p3, l31). But there is no explanation of why the MEB rheology is an improvement over the EB rheology for modelling marginal ice zone dynamics, or rheologies used in other models. Even if the authors don’t intend this as a main motivation, they should discuss the relevance of the MEB rheology for the MIZ.

The interesting feature about both the EB and MEB rheologies is the presence of the damage variable which can be increased for fragmented ice to make it more mobile. The MEB itself is not any better than EB, or even visco-pastic rheologies, for the MIZ. All these rheologies assume that when sea ice has a low concentration, it behaves almost in free drift. In terms of sea ice dynamics, the MIZ is therefore defined as a function of sea ice concentration. The damage variable in EB/MEB offers an easy way to account for the effects of fragmentation, and therefore to allow wave to modulate sea ice dynamics in the MIZ, in addition to sea ice concentration. We have tried to make this motivation clearer in the Introduction. We have also made clearer the differences between EB and MEB, and how it could affect MIZ dynamics (in the Introduction also).

Wave attenuation, ice fragmentation and wave radiation stress models are very important for this study. The developments of these models, key assumptions, etc should be discussed, as all the models contain large uncertainties.

We agree. We have therefore added modifications to answer their following comments.
Notably, only one sentence is given to wave attenuation models in the introduction (p2, l24; not including the short sentence referencing review papers), despite uncertainties in attenuation being identified as important later (bottom of p20).

We rewrote this paragraph of the introduction in the updated manuscript (P2L31). We have tried to give more context to the reader, and make clearer the importance of interactions between floe size and wave attenuation.
We also rewrote the paragraph concerning wave attenuation in the Discussion (P23L29).

The scattering reference (Montiel & Squire,2017) is actually a fragmentation (or ice breakup) study (see its title) and should be used elsewhere. The scattering model used for that study is the 3D model by Montiel et al (2016), but I’m not aware of 3D scattering models being available in Wavewatch III.

The whole paragraph has been rephrased (see our answer to the previous comment), and we now refer to Montiel et al. (2016) when we mention scattering.

The review of fragmentation models focuses on the FSD shape, and overlooks the models used to predict if waves are capable of breaking the ice cover. The lab model by Herman et al (2018) is relevant here (noting that it is for regular, low steepness waves only), as is the lab model by Dolatshah et al (2018).

This is actually a very good point. We added a few sentences about the uncertainties of the 1-D break-up model in the Discussion.
P24L16: *The estimation of the extent of broken ice is also likely to depend on the model used to determine the occurrence of sea ice break-up. We use a break-up model identical to most studies interested in wave-ice interactions (e.g. Williams et al., 2017; Bateson et al., 2020; Roach et al., 2019). It remains extremely simplified and assumes that break-up only occurs in the case of flexural failure in one dimension. However, recent results from laboratory experiments (Herman et al., 2018b; Dolatshah et al., 2018) tend to show that there is not such a clear relationship between the wave forcing and the floe size resulting from fragmentation. This is because a complete break-up model should include effects that are*

currently missing (e.g. from the floe shape and size, 2-D flexure modes, floe-floe collisions, rafting).

Similarly, the WRS model needs to be discussed; based on Williams et al (2017), it seems to contain large uncertainties and arbitrary assumptions.

We have added some comments on these assumptions when we introduce the WRS:
P5L12: *This computation provides an estimate of the WRS which is likely to be an upper-bound of its real value, as it assumes that all the momentum lost by attenuated waves is transferred to sea ice, therefore ignoring a potential partitioning of this momentum transfer between the ocean, sea ice and atmosphere.* [...] *As discussed by Williams et al. (2017), the estimation of the WRS and its distribution in the MIZ also strongly depends on the parameterization chosen for the wave attenuation.*

Other comments, suggestions and questions:
The statement "There are two main processes through which waves can affect sea ice dynamics" (p2, l3) is far too strong. What about, e.g., collisions? Just say "we investigate two processes..." or similar.

This is exact, and we rewrote this paragraph following this comment and the following one, see below.

The subsequent discussion on the role of WRS needs to be more balanced, e.g. Williams et al (2017) found that "wind stress dominates the WRS", and Alberello et al found negligible WRS in a pancake ice MIZ, even during large wave events.
We agree, and rewrote the paragraph as follows to make it more accurate:
P2L3: Waves can impact sea ice dynamics in the MIZ through a variety of processes. For instance wave attenuation transfers momentum from waves to sea ice through the wave radiation stress (WRS, Longuet-Higgins, 1977), which acts as a force that pushes the sea ice in the direction of the incident waves. Being mostly directed on-ice, its main effect is to maintain a compact sea ice pack near the ice edge, but its importance is still discussed. Estimating wave attenuation from SAR images, Stopa et al.(2018b) estimated it to be as important as the wind stress over the first 50 km of the MIZ in the Southern Ocean, whereas Alberello et al. (2019) do not observe any wave-induced sea ice drift in pancake ice in the Southern Ocean from in situ measurements, despite a strong wave-in-ice activity. Fragmentation is also likely to change the mechanical properties of the ice, but the evolution of dynamical and mechanical properties of a sea ice cover with the floe size remains poorly understood.

On p2 l15, replace "fragmentation" by "floe size", as neither Shen et al (1986) nor Feltham (2005) included fragmentation in their models.

It has been replaced as suggested.

Saying "their FSD depends only on the wave field" (p3, l8) is not true, as D_max depends on the ice properties, as does the breaking criterion.

We actually removed this sentence from the introduction.

The work by Rynders (2018) is conspicuously missing from the introduction.

We added a few sentences mentioning the approach of Rynders (2017) in the Introduction:
P3L25: *Concerning the impact of wave-induced fragmentation, Rynders (2017) suggests combining the classical elasto-visco-plastic rheology used in most sea ice models with a*

granular rheology in the MIZ to better represent floe-floe interactions. This granular rheology depends on the floe size. Numerical simulations with this approach show an overall increase of the sea ice drift speed in the Arctic all year round compared to a reference simulation using a standard version of the sea ice model CICE (Hunke, 2010).

The line at the top of p6 is awkward and should be reworded.

This sentence was reworded as:
P6L23 Thus, floes with sizes in [D_0 D_1] cannot be broken into smaller pieces, and we refer to floes with sizes in [D_{N-1} D_N] as unbroken floes.

Regarding the reference to Roach et al two lines below, please clarify if you are considering an FSD or an ITFSD?

We are considering a FSD only. We substituted the reference to Roach by one to Zhang et al. (2015), which is more appropriate in the case of a FSD only.

Does "lateral melt will not be discussed here" mean that it won't be included in the model for the study?

Lateral melt is included in the model for this study, but we look at a time and a region where it does not happen. To make it clearer, we rewrite the sentence as "Note that lateral melt is included in the model in this study but will not be discussed here..."

The opening paragraph of 2.2.2 is very long-winded for describing a simple method.
This paragraph has been rewritten to be sharper and clearer.

Please give a reference to back the statement "sea ice fragmentation is a violent phenomenon ... impact the floe size". This doesn't seem to allow for fatigue.

We have rewritten this sentence to justify the quick relaxation of the "fast-growth" towards the "slow-growth" FSD. It now reads:
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.*
This idea of keeping a memory of previous fragmentation here must be distinguished from the concept of fatigue. Fatigue depends on the evolution of sea ice microstructure and there are too many things we ignore about it: how much it lowers sea ice resistance to break-up, how much bending is needed to significantly affect the sea ice, if it can heal and how long it would take. We thus do not account for fatigue in our model. We added a mention to fatigue in the discussion, when discussing the break-up model as suggested in one of the following comments
P24L23: *We also point out that while our model includes some memory of previous fragmentation events, we do not account for the fatigue of the ice when determining if break-up occurs or not. The "slow-growth" FSD is used to keep a memory of the distribution of consolidated floes. It is associated with large-scale mechanical properties of the ice cover, while fatigue is related to the micro-structure of the ice. Accounting for fatigue could significantly lower the ice resistance to flexural failure in some events (Langhorne et al., 1998).*

Do the lines at the top of p8 mean D_N=1000m?

No, it does not. We added the mention "in WW3" to remove the ambiguity.

On l10, “freedom” doesn’t seem to be the correct word.

We replaced freedom by flexibility, which is more appropriate given that the shape of the FSD is indeed not free.

Say a bit more about tau_WF below Eq 7. Is it a numerical parameter of does it have physical meaning?

Tau_wf is mostly numerical, as its introduction mostly serves the purpose of avoiding the FSD redistribution to depend too much on the coupling time step. Physically, it also represents the fact that the fragmentation of the ice cover experiencing a constant sea state is not immediate, but rather associated with a timescale of the order 1 hour. We rewrote this paragraph and added a bit more information in the updated manuscript:
P10L10: *We introduced tau_WF to avoid dependency of the FSD redistribution to the coupling time step. It represents the timescale needed for the FSD of a fragmenting sea ice cover to reach a new equilibrium under a constant sea state. We set it to 30 minutes, as it corresponds to the timescale of the fragmentation event described in Collins et al. (2015).*

On p8 l11, please check the interval bounds, and on l13 reword “over within which”.

These sentences have been edited as the notations (D,D’, D_1, D_2) were confusing. The sentence containing “over within which” was not necessary after these edits, and has therefore been removed.

At the bottom of p9, what exactly is being conserved?
This is sea ice cover surface area. This sentence has been removed from the text, as we refer instead to Boutin et al. (2020) which uses the same formulation and give the details for the choices of D1 and D2.

Please clarify the two sentences starting p10 l19. Also, is this the ice-coupled or open-water wavelength?

We rewrote these two sentences to explain the motivation of increasing the value of Dmax sent to WW3:

P12L10: *Besides, the flexure dissipation mechanisms included in WW3 by Boutin et al. (2018) requires 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 flexure 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 where* D_N*<1000m, to make sure that swells are still attenuated in an unbroken sea ice cover by WW3, we linearly increase the value of Dmax sent to WW3 from Dmax=*D_N *to Dmax= 1000m with the proportion of sea ice in the largest floe size category* $\int D_N D_{N-1} g_{slow}(D)dD/c$
In the general case, the wavelength that we refer to here is the one relevant for waves in ice, hence the ice-coupled wavelength. However, the evolution of the wave dispersion relationship in a fragmented sea ice cover being largely unknown, we chose in our simulations to use the “open water” wavelength everywhere. This information was actually missing from the text, and we now justify this choice in the beginning of section 2:

P4L31: *Like in the study by Ardhuin et al. (2018), we assume that deviations from the ice-free wave dispersion relationship induced by the presence of ice are small and can be neglected. This is likely to be the case once sea ice has been broken (Sutherland and Rabault, 2016).*

Should it be "fragmentation and/or refreezing" on p11 l6?

If the reviewer refers to the sentence "This process is repeated every time fragmentation occurs in the sea ice model.", then the current sentence is correct. The process we describe only increases the value of damage. Refreezing reduces the value of damage in a way that is unchanged by the addition of waves in this study (the damage healing is described in Rampal et al., 2016).

Has the sensitivity of the coupling time step between the wave and ice models been tested (section 3.1)?

The sensitivity of the coupling time step on the FSD resulting from fragmentation has been tested in the Beaufort Sea test case, which led us to add the tau_wf parameter we discussed above. We found that wave attenuation and sea ice broken extent estimations were almost unchanged between stand-alone WW3 runs and coupled simulation, giving us confidence that the coupling timestep was appropriate. However, the sensitivity of the REF, CPL_WRS and CPL_DMG simulations to the coupling time step has not been investigated.

Also, why is tau_heal set to 25 days?

This is the default value in the model. Details are given in Rampal et al. (2016): nextsim results have little sensitivity for healing time relaxation ranging between 15 and 30 days, and using 25 days gives a very good match with observation for the temporal scaling analysis of sea ice deformation. We added the Rampal et al. (2016) reference in the text.

You say "the other main novelties..." (p14 l11), but this is the first mention of novelty.

We rewrote and shorten the introduction of section 4.1.2 that is now:
P16L11 : Our coupled framework introduces two FSDs to represent the evolution of the floe size from two different points of view. It also introduces a new redistribution scheme used when wave-induced sea ice fragmentation occurs. This section provides a quick evaluation of these new features.

The CPL simulations first appear in 4.1.2, but in section 3.3 it says they will be used insection 4.2.

This is true and now fixed.
P14L26: *We also evaluate the evolution of the two FSDs with refreezing/healing using the CPL_DMG simulation described in the following section 3.3*

Explain the statement "this quick re-generation ... making welding very efficient" (p15,l8).

Roach et al. (2018) use a constant welding rate in their welding parameterization, meaning that the reduction of the number of floes during a period delta t is independent from the floe size. As a consequence, the welding of O(10)m floes occurs at the same rate as the welding of O(1)m floes. This has consequences in pack ice, where floes are larger, hence with fewer

floes per surface area, the proportion of floes that merge during delta t is higher than at the edge, and the growth of the floe size is then controlled by the rate of welding.
We rephrased our statement to make this importance of welding in pack ice clearer:
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 This is because, like Roach et al., (2018), we use a constant value for kappa, meaning that the fewer floes there are {i.e. the larger the floe size), the higher the proportion of floes that merge during a given time period is.*

What “impact of waves on sea ice dynamics” is being referred to in section 5?

We replaced wave by wave-induced fragmentation to be more specific here.

Also, does “large fragmentation events” mean fragmentation over a large area or something else?

Yes, fixed.

The sentence at the end of the first paragraph of p21 is incorrect. Bennetts et al (2017) use a parameterization based on in-situ measurements by Meylan et al (2014). More generally, starting with Bennetts & Squire and Williams et al (2013a,b), it is usual to model attenuation using a viscous dissipation term for low-frequency waves and a scattering term for mid-frequency waves (see also Squire & Montiel, 2016).

This is true, and we removed this sentence as we rewrote this paragraph as advised by the reviewer’s major comments on wave attenuation.

Check the inequalities on l4 and l9 of p23.

There was indeed a mistake in the sense of the first inequality, but this appendix has been removed as we improved our motivations for the choices of the parameters controlling the redistribution of the FSD in the main text.

What are the vectors in Fig 2b?
These black arrows indicate the wave mean direction. We have added this information in the caption.