The authors would like to thank both reviewers for their most constructive and valuable comments. Below, we address the author comments in the order they were received. A marked-up version of the manuscript prepared with Latexdiff is included with this author comment. Additionally, in response to reviewer feedback, we have added a supplementary materials section to the revised manuscript, which is included with this comment as well.

Response to Comment by Reviewer #1

1 General Comments

> This is a nice paper, which I enjoyed reading very much. I recommend it for publication if my criticisms below can be addressed (may need "Major revisions").

Thank you for these encouraging and most helpful comments. We addressed them in the revised manuscript, and fully describe our revisions below.

2 Specific Comments

2.1 Major Comments

- p2959, L22: Herman (2010) discusses a stochastic (GLV: Generalised Lotka-Volterra) mechanism for generating a Pareto distribution – did the authors look into this at all?

  We thank the reviewer for this reference. While it was cited in the original submission, we agree that it is very relevant and deserves more discussion in the paper, as does Toyota et al. (2011). We added the following to the end of the introduction section.

  > Herman (2010) modeled the FSD as a generalized Lotka-Volterra system, which admits as a solution a Pareto distribution of floe sizes, and suggested that this distribution might fit observed FSDs. Toyota et al. (2011) showed that observed FSDs in the Weddell Sea may be fit by a power law and, that such a scaling relationship may be obtained by assuming that ice fracture is a self-similar process, following a renormalization group method.

- p2970–2972: A limitation of this approach is that it only involves binary collisions. Direct numerical solutions (eg [4, 8]) have shown clear grouping and [4] showed the group size had a power-law distribution. This could be an interesting way to look at this, especially in combination with the thermodynamics part (letting the groups freeze together if it's cold enough).

  Our model indeed considers binary collisions only and we acknowledge this explicitly now. We also note in the revised paper that interactions that form aggregate clusters of floes may be generated as a sequence of binary interactions, which is also the manner in which Herman (2013) developed clusters. We clarify and address this in section 2.2 as follows,

> Here, interactions between floes are treated as binary collisions, and our model does not consider multiple simultaneous collisions in a single time step. Such multiple collisions lead to clustering, which is relevant for granular media undergoing deformation (Shen and Sankaran, 2004), with sea ice being a possible example. However, Herman (2013) demonstrated in numerical simulations that floes may also aggregate into clusters via a sequence of binary interactions between pairs of floes.

- p2972 L17: "This choice eliminates the need for keeping track of sea ice morphology". If the model can produce a good estimate of ridging history (even just a ridging density), both [2] and [6] observed that floe break-up mostly happened at pre-existing weaknesses (cracks and ridges), so there could be some way of connecting floe size distribution with ridge density in the case of wave break-up.

  Our model indeed does not keep track of ridging history, and that its role in later mechanical interactions is an interesting issue. Although adding such physics is beyond the scope of the present article, we now write at the end of section 2.2,

  > Given that our model assumes each floe has a uniform thickness, we treat floes formed by ridging or rafting to be of such uniform thickness, chosen to conserve volume. This choice eliminates the need for keeping track of sea-ice morphology. Observations (Collins et al., 2015; Kohout et al., 2015) have indicated that floes may break up along ridges, in which case equation (18) may be used to provide information about the ridge density. This is a potential future extension of the present work.

- p2974 L15: It is worth discussing/mentioning [7] here.

  Thanks for this reference to Meylan et al. (2014), which we now cite in Sec. 2.3 in the context of observations and modelling of wave attenuation in ice.

  > Future applications of this FSTD model should therefore carefully consider the wave attenuation formulation, based on both model estimates and observations (e.g., Meylan et al., 2014).

- p2975 L10: here the amplitude depends on dλ – is this not a problem?

  This seemingly unusual formulation is standard in the surface wave literature, where dλ corresponds to the finite sampling resolution that separates Fourier components of a wave record. We now make this more explicit as follows,

  > The amplitude of waves with wavelengths in the range λ to $\lambda + d\lambda$ is approximated following $a(\lambda) \approx \sqrt{2S(\lambda)\,d\lambda}$ (see Bouws et al. (1998), p.11, and Meylan et al. (2014), eq. 2). The spectrum $S(\lambda)d\lambda$ is equal to half the mean amplitude squared of waves belonging to waves with wavelengths between λ and $\lambda + d\lambda$, equal to the total wave energy in this wavelength band normalized by ρg. The range $d\lambda$ corresponds to the sampling resolution of Fourier components of the wave record (Bouws et al., 1998).

- p2975 L19 The authors are correct that wave heights are roughly Rayleigh distributed (assuming a Gaussian distribution of wave elevations [1] – ie this doesn't apply to mono-chromatic waves (swell waves)).

  We have expanded the discussion to include a mention of this limiting case, in Sec. 2.3 as follows,

  > The normalization by $A(\mathbf{r}) = \int P_{wa}(a(\lambda))\theta(\epsilon_{crit}(\lambda,\mathbf{r})-\epsilon_{max})\theta(r-\lambda)d\lambda$, where $\theta(x)$ is the Heaviside step function, assures that the integral of P_f over all wavelengths is equal to 1 if the floes of size $\mathbf{r}$ will break. ... In the case of monochromatic swell waves, which are not described by a Rayleigh distribution, the only contribution of $P_{wa}(a(\lambda))$ to $P_f(\mathbf{r},\lambda)$ is at the wavelength of the swell, as the wave amplitude $a(\lambda)$ is zero for all other wavelengths.

  However, I am not sure that it is correct to apply it to individual wave frequencies or frequency bands. Williams et al. (2013a) used a Rayleigh distribution for the strain spectrum, so the breaking probability considering the full spectrum was proposed to be:

  $$P_{breaking} = \mathcal{P}(|\epsilon| > \epsilon_{breaking}) = \frac{2}{\bar{\epsilon}^2} e^{-\frac{\epsilon^2_{breaking}}{2\bar{\epsilon}^2}}$$

  This could be used as the total breaking probability but it doesn't give any idea about the floe sizes produced by the breaking. The authors are suggesting using the wave spectrum to get the floe sizes, which is not a bad idea. It could be used in conjunction with the above perhaps, eg.

  $$P_f(\mathbf{r},\lambda) = P_{breaking}\theta(r-\lambda/2)\frac{\int_{\lambda}^{\lambda+\Delta\lambda} S(\lambda')\,d\lambda'}{\int_0^{2r} S(\lambda')\,d\lambda'}$$

  $$P_{breaking} = \int_0^{\infty} P_f(\mathbf{r},\lambda)\,d\lambda = \int_0^{2r} P_f(\mathbf{r},\lambda')\,d\lambda'.$$

  The reviewer's suggestion is a good one, as it addresses the problem of determining the fraction of the ocean surface that could potentially lead to ice fracture, yet as the reviewer mentions this approach does not explicitly provide information about the floe sizes produced by the breaking. Our model is designed to determine the size of new floes, and for this purpose we need to evaluate the strain rate criteria at each floe size and wavelength. This implies that we need to assume that all wave components interact separately with floes. Significant work would be needed to figure out how to combine the strain probability of Williams et al. (2013a) with the spectral method used in this paper, but for now, we mention this potential extension in the revised manuscript as follows:

> Our approach is to determine the floe size distribution caused by the fracture of ice by surface waves, $F(\mathbf{s},\mathbf{r})$, based on the wave spectrum. Williams et al. (2013a) used a Rayleigh distribution for the strain spectrum to predict breaking of floes, however this does not determine the floe sizes produced by the breaking. The central assumption that we will make in determining the expression for $F(\mathbf{s},\mathbf{r})$ is that individual wave components act separately on floes.

Related to the above point: I think the Rayleigh distribution should be

$$P_{wa} = \frac{2}{\overline{a^2}} e^{-a^2/2\bar{a^2}},$$

so

$$\overline{a^2} = \int_0^\infty S(\lambda) d\lambda = H_s^2/16 = \int_0^\infty a^2 P_{wa} da.$$

We thank the reviewer for noticing this typo: we intended to define the Rayleigh distribution of wave amplitudes, but instead wrote the form of the distribution as if it were in terms of wave *heights*. This has been revised in the updated manuscript,

> Observations of wave amplitudes (see Michel, 1999, p. 9) show wave amplitudes to be Rayleigh distributed,
>
> $$P_{wa}(a) = \frac{8a}{H_s^2} \exp\left(-8a^2/H_s^2\right).$$

There is some additional ambiguity in the definition of $S(\lambda)$, a summary of which is found in Michel (1999), p. 3. In our first submission we chose $S(\lambda)$ to be the less-commonly-used "amplitude squared" spectrum, rather than the "half-amplitude-squared" spectrum (see our definition of a, p.2975 line 11), that appears to be more widespread. In the former case the integral of $(1/2)S(\lambda)d\lambda$ is equal to $H_s^2/16$, the same as the integral of $a^2 P_{wa}(a)$. However, to be more consistent with common notation we have updated the text to be consistent with the more common definition of $S(\lambda)$, as the "half-amplitude spectrum" in the definition of the wave amplitude (Sec. 2.3):

> The amplitude of waves with wavelengths in the range λ to $\lambda + d\lambda$ is approximated following $a(\lambda) \approx \sqrt{2S(\lambda)\, d\lambda}$ (see Bouws et al. (1998), p.11, and Meylan et al. (2014), eq. 2). The spectrum $S(\lambda)d\lambda$ is equal to half the mean amplitude squared of waves belonging to waves with wavelengths between λ and $\lambda + d\lambda$, equal to the total wave energy in this wavelength band normalized by ρg.

Also, in (20), why truncate at $\lambda <$ r instead of $\lambda/2 < r$ since a wavelength of λ has maximum strain at both peaks and troughs (as the authors point out themselves)?

This is an important issue. We use a strain criteria for the ice fracturing (e.g., Dumont et al., 2011). If the ice is assumed elastic it feels the local strain rate of the wave. A passing wave crest will therefore cause the maximum strain value to be felt at each point along the floe, and this implies that the floe fracture at each point into numerous small floes. In reality, small floes wont behave as plastic plates, therefore avoiding this problem. We therefore must choose some minimal length of a floe below which ice breaks. Our choice is somewhat arbitrary, but given the above discussion, it seems that $\lambda/2 < r$ is not much more justifiable than $\lambda < r$. It would also be difficult and arbitrary to decide, for example, in which way to fracture a floe of size $3\lambda/4$ if the criterion is based on a minimum size of $\lambda/2$, while if the minimum is λ we can just fracture each floe into two equal parts. In any case, the proper choice requires further observations and modeling, but the choice does not materially affect the model. We now explain, admittedly briefly,

> If the wavelength is larger than the floe radius, the floe is not fractured. This specification of the minimum floe size that may be fractured by a wave of wavelength λ is somewhat arbitrary, and based on the heuristic assumption that smaller floes float without being significantly strained by the waves. A better choice of this minimum floe size requires further observations and modeling.

- p2975 L1: Breaking time-scale: the authors determine it from the grid size and the wave speed. I think this is similar to using the model time step such as done by [3] or [9]. Both are somewhat artificial. [2] noticed the breaking front travelled at 0.25cg – perhaps this implies the time-scale should be ≈ 0.25 times the wave period?

We thank the reviewer for pointing out this observation, and acknowledge in the revised manuscript that this choice is affected by the lack of data on the response timescale of the ice cover to fracture by waves. We additionally performed a study of the sensitivity of the model to changes in the breaking time-scale, which we have added to the supplement in Sec. S1.3.

> The duration $\tau(\lambda)$ over which breaking occurs is approximated as the domain width divided by the group velocity for surface gravity waves,
>
> $$\tau(\lambda) = \frac{D}{c_g(\lambda)} = 2D\sqrt{\frac{2\pi}{g\lambda}}.$$
>
> Observations of wave propagation in ice (Collins et al., 2015) have suggested that the propagation speed of fracture in ice may be slower than the group velocity of surface waves. With more data, the above choice for $\tau(\lambda)$ may be re-evaluated.

Minor comments

- Is equation (4) correct? When I tried to derive it from (3) I got:

$$\begin{aligned}
\partial_t \partial_r \partial_h C(\mathbf{r}, t) &= \partial_t \left(\frac{f}{\pi r^2} \right) \\
&= \frac{1}{\pi r^2} \partial t f - \frac{2f}{\pi r^2} \partial_t r \\
\partial_t f &= \frac{2f}{r} \partial_t r - \pi r^2 \partial_r \left(\frac{f}{\pi r^2} \partial_t r \right) - \pi r^2 \partial_h \left(f \partial_t h \right) \\
&= \frac{4f}{r} \partial_t r - \nabla_{\mathbf{r}} \cdot (f \mathbf{G})
\end{aligned}$$

  This confusion occurred because of our previous notation, where we used $\dot{r}$ to denote rate of change of size, but this is not the same as the derivative of the size coordinate with respect to time. We changed notation throughout now such that the rate of growth of size and thickness is denoted $\mathbf{G} \equiv (G_r, G_h)$.

- Eqn (5): δ is used many times in many contexts in this paper. Perhaps reserve it for the delta function, and possibly also for the 1d function e.g. $\delta(r_p, h_p) \to \delta(r - r_p)\delta(h - h_p)$. (TC being a geophysical journal). Also perhaps define A_p nearer to (5) (there is a delay of 1 page before it's defined).

  In order to avoid confusion, we now use δ with no subscripts for delta function throughout. When we use delta to denote other quantities (e.g., width of contact zones), it is now always accompanied by an appropriate subscript. The two-dimensional delta function in Sec. 2.2 is explicitly defined now,

  > Note that the function $\delta(\mathbf{r})$ is the two-dimensional delta function: $\delta(\mathbf{r}) = \delta([r, h]) \equiv \delta(r)\delta(h)$.

- What are the limits of the integral in (15)? Is it $\int\limits_{\mathbf{r_1}}^{\infty} \int\limits_{\mathbf{r_2}}^{\infty}$ (if so it is bad notation as r1 and r2 are also the integrated variables)

  The limits are over all resolved values of r_1 and r_2, i.e. $\int\limits_{(r_{1,min}, h_{1,min})}^{(r_{1,max}, h_{1,max})} d\mathbf{r}_1 \int\limits_{(r_{2,min}, h_{2,min})}^{(r_{2,max}, h_{2,max})} d\mathbf{r}_2$, which becomes rather cluttered. Rather than add these definite limits, we have clarified this terminology after eq (15).

  > ... where the notation $\int\limits_{\mathbf{r}} d\mathbf{r}$ is taken to mean an integral over all floe sizes and thicknesses resolved by the model.

- Should the left hand side of (16) be $\partial_t f$?

  It should be, we have added a second equality for clarity.

$$\frac{\partial f(\mathbf{r})}{\partial t} = \pi r^2 \frac{\partial N(\mathbf{r})}{\partial t} = \mathcal{L}_m(\mathbf{r}); (r \neq 0)$$

- p2964: $\overline{h/r} \rightarrow \overline{rh}$? (More natural to define the average using N as the weighting?)
  Since the main distribution we evolve is the FSTD, we would like to reserve the notation $\bar{x}$ as the mean with respect to the FSTD, admittedly this makes for this clunky definition of the average of a quotient.

- p2975 L19: I think the Rayleigh distribution should be

$$P_{wa} = \frac{2}{\overline{a^2}} e^{-a^2/2\bar{a^2}},$$

  See above comment, we have updated the equation to reflect the typo in Sec. 2.3 (an equation in wave amplitude instead of wave heights).

- p2975 L11: I couldn't see the "normalised energy spectrum" the authors were referring to on p11 of the WMO guide.

  Our usage of this term did not follow the WMO guide to the letter: the WMO guide refers to the "energy spectrum", though it has units m^2/Hz, so it is really the energy normalized by the water density and g (or the variance spectrum). To be consistent with the WMO guide, we avoid the term "normalized spectrum" and explain more explicitly,

  > The spectrum $S(\lambda)d\lambda$ is equal to half the mean amplitude squared of waves belonging to waves with wavelengths between λ and $\lambda + d\lambda$, equal to the total wave energy in this wavelength band normalized by ρg.

Typos

Thanks for these, they have been corrected in the revised manuscript.

- p2959, L22: have same $\rightarrow$ have the same

  Thanks, we have corrected this now,

  > assuming that all floes of different sizes have the same ITD.

- p2972, L19: the we $\rightarrow$ that we

  The sentence containing this typo has been eliminated in the revised manuscript.

Response to Comment by Reviewer #2, Luke Bennetts

1 General Comments

1. The paper contains a lot of information. Beyond the consideration of a joint distribution, the novel contributions of the paper are not immediately apparent, e.g. novel contributions to the source terms. Therefore, I suggest a short passage at the end of the Introduction or beginning of the second section to address this.

   Thank you for this comment, we have added a paragraph in a prominent place in the introduction section which addresses this issue.

   > The major contributions of this paper are, first, that it presents the first treatment of the *joint* floe size and thickness distribution. In addition, each of the terms in equation (2) as developed below contains a novel formulation of the corresponding process that is physically based and less heuristic than used in previous studies.

2. The consideration of a joint distribution clearly extends the recent work of Zhang et al. (2015). However, the paper does not show the importance of the joint distribution. The paper would be much stronger if the authors provided more evidence that a joint distribution is necessary (or not). (I acknowledge the sentence on page 2977, lines 13–16.)

   We also agree that more emphasis on this point is necessary. The paper is near a sensible length limit, which does not allow us to present additional numerical results. However, to address this point, we went through the descriptions of each of the included processes, and added relevant information to emphasize the important role of the *joint* FSTD wherever this is relevant, as follows.

   In Sec. 2.1,

   > N is the number distribution introduced above, $2\pi rh$ is the lateral area of one floe, and $\overline{2h/r}$ represents an average over all ice floes, weighted by the floe size and thickness distribution. The above result depends on the model including an explicit joint FSTD, without which this estimate for the lateral area would not be possible to obtain.

   In Sec. 3,

   > This cluster would not be resolved in a model that represented the ice thickness distribution only. The second cluster is due to a ridging interaction between floes of size I and II, leading to new floes of around 90 m size and 0.5 meters thickness. The third, due to self-interaction (ridging) between floes of size II, leads to a positive tendency at floe sizes around 17 meters and thickness around 1.7 meters. Both the second and third clusters of floes would not be resolved in a model that represents the floe size distribution only, showing again the importance of representing the joint FSTD.

In Sec. 2.3,

> We note again that the distribution of both floe size and thicknesses plays a critical role in determining the fracture of ice by waves, underlying the need to use the coupled FSTD for representing the effects of ice fracture due to surface waves.

2 Specific Comments

1. The model appears to be designed for the marginal ice zone. I think this should be explicitly stated, e.g. in the title of the paper.

   While we prefer not to alter the title of the paper, we made more explicitly clear in the abstract, introduction, and conclusion the applicability of the model to the MIZ. Specifically, in the abstract:

   > [Sea ice] is characterized by a complex and continuously changing distribution of floe sizes and thicknesses, particularly in the marginal ice zone (MIZ).
   >
   > The model accounts for effects due to multiple processes that are active in the MIZ: freezing and melting along the lateral side and base of floes, mechanical interactions due to floe collisions (ridging and rafting) and sea-ice fracture due to swell propagation in the MIZ and at the ice margin.

   The introduction now includes

   > The most dramatic intra-annual variability in sea ice cover is found in the MIZ, and in seasonal ice zones, regions which range from being ice-covered to ice-free over the year. As sea-ice cover becomes thinner and more fractured, these regions will become larger, and the distribution of these floes and their size, shape, and properties may change.

   The conclusion now includes

   > We developed a model that simulates the evolution of the FSTD, using as input large-scale oceanic and atmospheric forcing fields, which may be useful as an extension to sea-ice models presently used in global climate models, in particular in regions with a continuously varying FSTD, such as the marginal ice zone.

2. Page 2957, top: The marginal ice zone is often defined as the part of the ice-covered ocean where ocean waves cause ice breakage (see Weeks (2010) and more recently Williams et al. (2013a,b)).

   We agree, since there are two definitions of the MIZ, we have added text that addresses this point.

> ... the Arctic marginal ice zone, defined as either the region of the ocean over which ice waves lead to the fracture of ice (e.g. Williams et al., 2013b), or as the area of ice with concentration between 15% and 80%, has been widening during the summer season (Strong and Rigor, 2013).

3. Page 2957, line 5: The recent publication Kohout et al. (2015) could be cited here.
   We agree, and have added a reference at this suggested place in the introduction.

   > (Asplin et al., 2012; Zhang et al., 2013; Kohout et al., 2015)

   More discussion of this paper has been included in other relevant locations. Specifically, in the development of the mechanical component,

   > Observations (Collins et al., 2015; Kohout et al., 2015) have indicated that floes may break up along ridges, in which case equation (18) may be used to provide information about the time evolution of ridging or ridge density. This is is a potential future extension of the present work.

4. Page 2957, line 15 onwards: Definite statements would help here. For instance, Steele (1992) showed that lateral melting is important for floes of a 30 m diameter or less.

   We agree that further clarification is necessary, and have provided it in the introduction.

   > The fractured sea-ice cover has increased floe perimeter, which may lead to enhanced melting and a more rapid reduction in sea-ice area compared to an unfractured sea-ice cover. Steele (1992) indeed demonstrated an increasing sensitivity of the ice cover to lateral melting with decreasing floe size, finding that below 30 m lateral melting was critically important. Smaller floe sizes may additionally lead to changes in the mechanical response of the sea-ice cover to forcing from the ocean and atmosphere, as floe size is a parameter in collisional models of ice rheology (Shen et al., 1986, 1987; Feltham, 2005, 2008).

5. The statement "level of detail may not suffice... where the ice cover is heterogeneous..." on page 2958, line 23 seems odd considering the statement that "sea ice is heterogeneous" on page 2957, line 7.

   We agree that this wording is a bit clumsy, and so we have clarified the text as follows,

   > Modern approaches to modeling sea ice in GCMs, such as the community ice model (Hunke et al., 2013), generally approximate ice cover as a non-Newtonian fluid with a vertically layered thermodynamics, and simple thickness distribution (Thorndike et al., 1975; Semtner, 1976; Hibler, 1979). This approximation may not suffice, because it does not account for the distribution of floe sizes and therefore for the above mentioned related effects.

   We have additionally moved the reference to Birnbaum and Lüpkes (2002) to earlier in the introduction, and removed the reference to Girard et al. (2009) as it is no longer relevant to the discussion where it was cited,

> Floe sizes can also affect the surface drag coefficient and therefore air-sea fluxes (Birnbaum and Lüpkes, 2002).

6. Page 2959, line 5: Please quantify the 'large observation window' and 'point observations'.

   We have removed the discussion of truncation error with respect to FSD measurements, where the text "large observational window" appears, deciding that this is not necessary given manuscript length limits. We have also clarified the meaning of "point observations" in the text to be reflect the location of the measurements that were made,

   > In spite of these challenges, many observations of the floe size distribution have been made, often using helicopter or ship-board cameras, notably in the Alaskan and Russian Arctic (Holt and Martin, 2001), Sea of Okhotsk (Toyota and Enomoto, 2002; Toyota et al., 2006), Prydz Bay (Lu et al., 2008), and Weddell Sea (Herman, 2010; Toyota et al., 2011).

7. Page 2959, lines 19–20: Dumont et al. (2011) and Williams et al. (2013a,b) focussed on wave attenuation and wave-induced ice breakage. It would be useful to add a short discussion of the relationship between these investigations and the study presented in this paper.

   We agree that further discussion and comparison may be useful, and included information about other models of the FSD. We have updated the introduction to include more discussion of both papers.

   > Other modeling studies involving the temporal evolution of the floe size distribution have mainly focused on understanding ocean wave propagation and attenuation in the marginal ice zone (Dumont et al., 2011; Williams et al., 2013a,b), who developed spectral models of ocean wave propagation, attenuation, and associated ice breakage. Both studies modeled the FSD using the renormalization group method of Toyota et al. (2011).

   We have additionally updated Sec. 2.3 to include a discussion of the model presented by Williams et al. (2013a).

   > Williams et al. (2013a) used a Rayleigh distribution for the strain spectrum to predict breaking of floes, however this does not determine the floe sizes produced by the breaking. The central assumption that we will make in determining the expression for $F(\mathbf{s}, \mathbf{r})$ is that individual wave components act separately on floes.

   > This approach may be compared with the spectral method of Williams et al. (2013a), where the fracture probability is extended, by assuming that the ice strain is Rayleigh distributed, and the size distribution of new floes is determined using the renormalization group method of Toyota et al. (2011), with a maximum floe size equal to half of the wavelength that corresponds to the zero-crossing period.

8. Page 2960, following equation 2: Please define the Laplacian operator and the physical domain.

   We now do this.

   > ... where $\mathbf{r} = (r, h)$, and $\nabla = (\frac{\partial}{\partial x}, \frac{\partial}{\partial y})$ is the two-dimensional Laplacian. The two dimensional spatial domain may be thought of as corresponding to a single grid cell of a climate model, on the order of tens of km on a side.

9. Section 2.2, paragraph 1: Can the floes rebound following a collision and/or cause erosion of the floe edges?

   This is not a feature of the model, though this is an important phenomenon. When floes interact, they are assumed to combine with each other. We have added a sentence to this effect.

   > In reality, some floe collisions may lead to a rebound and erosion of floe edges rather than to a merging of the floes, yet we do not account for such a process.

10. Page 2965, lines 21–23 to page 2966, lines 1–4: This long sentence is unclear and should be rewritten.

    We have rewritten these lines for clarity. The first part has been updated to read:

    > The integral of $f(\mathbf{r})$ over all floe sizes and thicknesses, including open water, is equal to one. Therefore, ignoring thermodynamic and wave effects, we integrate (2) over a range of floe sizes that includes a vanishingly small interval of sizes around $\mathbf{r} = (r, h) = \mathbf{0}$,

    The second sentence referred to by the reviewer has been updated to read:

    > The integral of $f(\mathbf{r})$ over all floe sizes and thicknesses, but excluding open water ($\mathbf{r} = 0$), is equal to the ice concentration, c. Integrating (2) as before but now excluding $\mathbf{r} = \mathbf{0}$,

11. Page 2971, lines 1–2: What is the physical basis for the collision probability? It does not appear to be based on dynamical considerations, e.g. the strength of the prevailing winds and/or waves. Herman (2011, 2013), Shen and Squire (1998) and Fig. 10 of Bennetts and Williams (2015) may be useful for future developments of this aspect of the model.

    We thank the reviewer for bringing up this point, and also for the reference to Bennetts and Williams (2015). We now explain in Sec. 2.2.1,

    > The above probability that two floes will collide is based on geometric constraints. However, the rate of collisions depends also on the ice strain rate tensor $\dot{\boldsymbol{\epsilon}}$ as explained above, and this tensor depends on external forcings such as the strength of the prevailing winds and currents (Shen et al., 1987;

> Herman, 2011, 2013; Bennetts and Williams, 2015), but the determination of that relationship is not a focus of the FSTD model presented here.

12. Page 2973, line 10: I suggest not referring to 'wave-breaking' here, as it is already reserved for a different phenomenon.

    We agree, and wherever appropriate we have updated the text to be more specific. In the beginning of the Sec. 2.3, we have updated two lines:

    > the response of the FSTD to fracture by waves

    and

    > The rate of change of area of floes of size **r** due to fracture by ocean surface waves is then,

13. Page 2974, paragraph 2: Kohout and Meylan (2008)'s wave attenuation model based on scattering is important and should be cited. However, the model has progressed since then. In particular, Vernon Squire and I derived a semi-analytic expression for the attenuation coefficient (Bennetts and Squire, 2012a). Moreover, we approximated the functional dependencies of the attenuation coefficient for applications such as the one presented in this paper (Bennetts and Squire, 2012b). I also suggest adding a statement that Kohout and Meylan (2008)'s Fig. 6 assumes the floes are long, and that the attenuation rate tends to zero as the floes become shorter (see their Fig. 3 and Figs. 6–7 of Bennetts and Squire (2012a)). Of greater significance, Kohout and Meylan (2008), Bennetts et al. (2010) and Bennetts and Squire (2012b) showed that scattering models significantly under predict measured attenuation rates. Thus, using a scattering-attenuation model alone allows long waves to cause ice breakage unrealistically far into the ice-covered ocean (Williams et al., 2013b).

    We have implemented the model of Bennetts and Squire (2012) alongside with the attenuation coefficient of Kohout and Meylan (2008), and now explain,

    > Scattering models may under-predict attenuation rates (Williams et al., 2012), which may allow for longer penetration of waves into the MIZ than is physically realistic. Updated models of the wave attenuation (Bennetts and Squire, 2012) suggest different attenuation coefficients as function of wave period and ice thickness. We tested our model with the Bennetts and Squire (2012) attenuation coefficient, and show in the supplement (Sec. S1.3), that our FSTD model can be sensitive to the choice of attenuation model. Future applications of this FSTD model should therefore carefully consider the wave attenuation formulation, based on both model estimates and observations (e.g., Meylan et al., 2014) .

    In addition, when describing the elastic plate model in Sec, 3.2, we have added, as suggested,

    > Kohout and Meylan (2008) modeled floes as long floating elastic plates

14. Page 2974, line 19: The statement 'wave fracture depends on their wavelengths rather than periods' is not strictly correct.

    This is true, and we have updated the text of Sec. 2.3 to reflect this,

    > We convert the attenuation coefficients, reported as a function of wave period, to a function of wavelength using the deep-water surface gravity wave dispersion relation, $\lambda = gT^2/2\pi$.

15. Page 2975: How does the spectral model differ to that of Williams et al. (2013a)?

    We added such a discussion both in the introduction and in Sec. 2.3. Specifically, in the introduction:

    > Other modeling studies involving the temporal evolution of the floe size distribution have mainly focused on understanding ocean wave propagation and attenuation in the marginal ice zone (Dumont et al., 2011; Williams et al., 2013a,b). These studies developed models of ocean wave propagation, attenuation and associated ice breakage, and modeled the FSD using the renormalization group method of Toyota et al. (2011).

    In the ice fracture section:

    > Our approach is to determine the floe size distribution caused by the fracture of ice by surface waves, $F(\mathbf{s}, \mathbf{r})$, based on the wave spectrum. Williams et al. (2013a) used a Rayleigh distribution for the strain spectrum to predict breaking of floes, however this does not determine the floe sizes produced by the breaking.

16. Page 2977, lines 18–20: Note that Williams et al. (2013a) extended the expression for the critical failure limit, and Williams et al. (2013b) showed the width of region of broken ice predicted by their model could be highly sensitive to this parameter (Section 5.2).

    Thank you, we now note this possible extension to the simpler formulation we use (Sec. 3).

    > The critical strain amplitude for flexural failure, ϵ_{crit}, is set to 3×10^{-5} in line with other studies (Kohout and Meylan, 2008; Dumont et al., 2011). Williams et al. (2013a) formulated a more complex expression for the critical failure limit, and this was found to have a significant effect on wave fracturing (Williams et al., 2013b). We examine the model sensitivity to some of the main parameters used in these model simulations in the supplement (Sec. S1).

17. Section 3: Have convergence and sensitivity tests been conducted?

    We would like to thank the reviewer for this suggestions: in response to this suggestion we performed a set of numerical convergence and sensitivity tests, which are now included as supplementary material as Sec. S1 (sensitivity) and Sec. S2 (convergence), for major model parameters. In doing so we uncovered a bug in the advection scheme that was used to describe the thermodynamic growth and melting of floes, which we are happy to have found. As a consequence, we have separated the former model variables r_p, which was the "pancake floe size", into two variables $r_{\min}$, which is the minimum resolved floe size, and r_{lw}, which is the width of the lead region, to examine the sensitivity to their use independently. We have additionally changed h_p to $h_{\min}$. These changes appear in Sec. 2.1,

> The lead region is defined as the annulus around each floe of width r_{lw} ...
>
> If the water is at its freezing point, a cooling heat flux leads to freezing of pancakes of ice of radius $r_{\min}$ and thickness $h_{\min}$ ...

The sensitivity studies are mentioned in Sec. 3,

> We examine model sensitivity to the parameter choices used in these model runs in the supplement (Sec. S1).

Additionally, when discussing the discretization, we note

> We examine the numerical convergence of the model in the supplement (Sec. S2) finding that increasing this resolution does not alter the numerical results.

18. Section 4: The opening paragraph doesn't seem appropriate for a Conclusions section.

    We agree, and so we have deleted this paragraph.

19. Page 2981, lines 21–22: Have the forcing fields been considered 'when combined'?

    This sentence has been removed, thanks!

Technical Corrections Thanks for these corrections, they have all been addressed in the revised manuscript.

1. Page 2961, line 13: Separate the equation from the text.

   We have separated the equation.

   > The cumulative number distribution is defined as
   >
   > $$C(\mathbf{r}) = \int_0^{\mathbf{r}} N(\mathbf{r}')\, d\mathbf{r}' = \int_0^{\mathbf{r}} (f(\mathbf{r}')/\pi r'^2)\, d\mathbf{r}', \ldots$$

2. Page 2972, line 19: Delete 'the'.

   We have deleted it, thanks for catching this!

3. Page 2977, line 7: 'Wave fracture' → 'ice fracture' or 'wave-induced fracture'.

   In all places with this phrase, we have changed it to either "fracture of ice floes due to surface waves", or some variant.

   In Sec. 2.3,

   > … $\mathcal{L}_W(\mathbf{r})$ is the time rate of change of floes of size and thickness $\mathbf{r} = (r, h)$ due to fracture of ice by surface waves … the response of the FSTD to ice fracture by waves …

   > Our approach is to determine the floe size distribution caused by the fracture of ice by surface waves …

   > … representing the effects of ice fracture due to surface waves.

   > The effects of the fracture of ice by waves … the effects of ice fracture by waves …

   In Sec. 3,

   > … leading to ice fracture.

   > Ice at this size and thickness is susceptible to fracture by surface waves … not susceptible to fracture …

   > Ice thickness does not change when the ice is fractured.

   > … simulates a seven-day period of ice fracture by surface waves …

   In the caption for Fig. 3,

   > Change in response to wave forcing only …

   In the caption for Fig. 5,

   > … one week of ice fracture by surface waves with the specified wave spectrum.

   In Table 4,

   > Ice fracture component of FSTD model.

   > … fracture of floes of size $\mathbf{r}$ by waves.

   > Variables used in the representation of the fracture of ice by surface waves in the FSTD model.

4. Figure 3's caption appears to be incorrect with respect to the labelling.

   This has been updated to fix the typo.

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