This paper starts with the CICE sea-ice model and adds waves that break up the sea ice in grid cells near the ice edge, causing that ice to melt more quickly in summer due to greater lateral surface area. In general the paper is well conceived and the mechanism is plausible. My comments (below) are minor.

We thank the referee for reviewing our paper, and his/her supportive comments and useful suggestions.

Page 1, Abstract. Total ice volume is reduced by over 500 km^3*, but this needs to be put into context. What is the volume of the entire ice cover? Page 2, lines 6-7. Same comment as above.*

We now quantify the volume loss in terms of proportion of the total ice volume.

Page 2, equation (3). What are the units of beta_2 and beta_4? From equation (2), alpha must have units of 1/length. The sea-ice concentration, c, is dimensionless, and omega has units of 1/time, so beta_2 must have units of time^2*/length, and beta_4 must have units of time*^{4}*/length. This should be explicitly noted.*

We added units to the values of $\hat{\alpha}_2$ and $\hat{\alpha}_4$ (formerly β_2 and β_4; changed to avoid confusion with β_0 and β_1 used in the FSD).

Page 3, line 10. How is a_bk(x) chosen or determined? There is nothing about it in the rest of the paper.

We now provide a clearer definition of $a_{\rm bk}$.

Page 3, lines 13-14. The assumption here is that the floe size distribution (FSD) follows a split power law, with one exponent for floes smaller than a critical size and another exponent for floes larger than the critical size, following Toyota et al (2011). This is a dubious formulation of the FSD. First of all, if one actually looks at figure 9 of Toyota et al (2011), one sees that the FSD is a continuously curving concave-down shape, rather than two power-law regimes. This was noted in earlier work by Herman (2010), who wrote in reference to an earlier paper by Toyota: "However, contrary to how the above authors interpret their results, in both cases the change in slope of the FSD seems rather gradual than abrupt. Instead of a combination of two power laws glued together at a highly arbitrarily chosen floe diameter, another type of distribution would be desirable. It should reflect the observed gradually increasing deviation from a power-law distribution for decreasing floe diameter." Herman, A. (2010), Sea ice floe size distribution in the context of spontaneous scaling emergence in stochastic systems, Physical Review E, 81, DOI: 10.1103/ PhysRevE.81.066123 Furthermore, other researchers have found power-law behavior for the Antarctic FSD in which the exponent changes as the ice edge is approached, but without a critical floe size separating two power-law regimes (Paget et al, 2001; Lu et al, 2008). Returning to the current paper, a much simpler assumption for the FSD would have been a simple power law with one exponent. This would have eliminated the need for four parameters: D_cr, q, gamma_1, and P0. It would be interesting to know whether the results hold up under this simpler (and possibly more realistic) FSD. However, I would not insist that the authors re-do all their calculations, unless it's a simple thing to do (maybe just set D_cr

= D_mn and P0 = 0). But they should acknowledge that their results rest on the questionable split power-law formulation of the FSD.

We added an acknowledgement that other FSD's have been postulated for the transition from small–large floe sizes.

In a preliminary version of the model, we used a fixed breakup diameter $D_{\mathrm{bk}} = 30\,\mathrm{m}$ (based on anecdotal observations from our colleagues), and later found that the move to a breakup diameter based on an FSD and the local wavelength produced only small quantitative changes in our results. Therefore, we predict that adjusting the fine details of the in-cell FSD will not significantly impact our findings.

Page 3, equation (5). In the definition of P0, there is an exponent gamma_1. Is this the same gamma_1 as in equation (4b)? Why should it be the same exponent as in the probability density function? I don't understand the reasoning or the math for the use of gamma_1 here.

We have corrected $\gamma_0 \to \gamma_1$ and $\gamma_1 \to \gamma_2$ in the paragraph below Eqs. (4a–b).

We set the parameter $\mathbb{P}_0$ so that the proportion q of the floes have diameters greater than the predicted predicted breakup diameter, i.e.

$$
\begin{aligned}
1 - q &= \int_{D_{\mathrm{mn}}}^{D_{\mathrm{pr}}} p(d)\ \mathrm{d}d \\
&= \mathbb{P}_0 + (1 - \mathbb{P}_0)(1 - D_{\mathrm{cr}}^{\gamma_1}/D_{\mathrm{pr}}^{\gamma_1}) \\
\Rightarrow \quad \mathbb{P}_0 &= 1 - q\left(\frac{D_{\mathrm{pr}}}{D_{\mathrm{cr}}}\right)^{\gamma_1},
\end{aligned}
$$

as given in the text. We made no changes in response to this part of the comment.

Page 4, equation (7) and following. What are the units of r_lat and w_lat? What is the value of the time step delta_t?

We added units for w_{lat} and clarified that r_{lat} is a fraction.

In the first paragraph of § 2, we now explicitly give the time step in terms of the notation Δt.

Page 6, lines 8-9, and Figure 2 (left panel). The text and the figure indicate that LESS attenuation of waves results in HIGHER mean ice concentration. I would have thought that less attenuation would allow more wave energy to penetrate into the ice pack and break up the ice, resulting in lower ice concentration. Please explain why less attenuation leads to higher ice concentration, and more attenuation leads to lower ice concentration.

We corrected the mistake in the text. . .

Page 6, line 15. "reducing the attenuation rate increases the impact..." I agree that reducing the attenuation rate SHOULD increase the impact of the waves, but Figure 2 shows that reducing the attenuation rate actually reduces the impact of the waves. The symbols for reduced attenuation rate (upward-pointing triangles) are much closer to the no-break-up case (crosses) than the symbols for increased attenuation rate (downward-pointing triangles). This doesn?t make sense to me.

…and the corresponding mistake in the Fig. 2 caption.

Page 7, line 8. "volume losses of 0.5 km^3 per grid cell" — This doesn't mean anything unless we know how big a grid cell is. On page 2, lines 15-16, we are told that the nominal resolution of the grid is 1 degree in latitude and 1 degree in longitude. The extent of 1 degree of longitude depends on latitude, so the size of a grid cell (in km^2) depends on latitude. At the latitude of the Antarctic Circle, 1 degree of longitude is about 44 km. So I calculate that the area of a grid cell is roughly 111 x 44 = 4884 km^2. A volume of 0.5 km^3 of ice spread over such a grid cell is about 10 cm of ice thickness (at 100% concentration) or 20 cm of ice thickness (at 50% concentration). So now I can understand roughly what a volume loss of 0.5 km^3 per grid cell means. Please help out the reader by providing this kind of information.

We now present volume losses per unit area.

Page 7, lines 20-23. If I understand this correctly, the eastern sector contains mostly first-year ice ("new ice"), with no memory of break-up, while the western sector presumably contains some multiyear ice, which retains memory of break-up?

We have rewritten this passage to clarify that summer volume losses are carried forward into winter in the western sector only, removing the word 'memory' that may cause confusion.

Technical Comments

Page 1, Abstract, line 2. "Model output shows that WAVE-INDUCED breakup…"

Added.

Page 2, equation (1). Cite Thorndike et al (1975), The Thickness Distribution of Sea Ice, JGR.

Citation added.

Page 3, line 18. "which is chosen to be equal TO the diameter…"

Typo corrected.

Page 3, line 20. Values are given for gamma_1 and gamma_2, but they should probably be gamma_0 and gamma_1. There is no gamma_2 in the equations.

Corrected: see earlier response.

Page 3, line 25. propagating through a uniform floe FIELD (?)

The predicted breakup diameter is calculated as the distance between successive peaks in strain produced by a regular wave propagating along a uniform ice cover of infinite extent.

We added the description "infinitely long" to the text.

Page 3, equation (6). Inside the integral, "pD" should be "p(D)" i.e. put parentheses around the "D"

Corrected $pD \longrightarrow p(d)d$.

Page 4, lines 21-22. In reference to Figure 1, in the left-hand panels showing wave height, it looks to me like the "sharp outer boundaries indicating the latitudes at which data is extracted from the wave model" are at the same latitude in each panel. But the next sentence says, "The boundary is farther north in winter..." I don?t see that the outer boundary of extracted data is farther north in the bottom panel. The outer circular boundary appears to be at exactly the same latitude in both panels. If the authors are referring to an INNER boundary that is several grid cells inside the outer boundary, they should mark it more clearly.

We added text to clarify that the "sharp out boundaries" we refer to are of non-zero wave heights.

Page 4, line 32. This sentence should refer to the middle column of Figure 1, just as the previous sentence refers to the left-hand column. Otherwise the reader may not shift her/his attention to the middle column.

We have amended this sentence.

Page 5, Figure 1, upper right panel showing concentration change. This panel is a bit too small — it's hard to see the regions of large change. A figure the size of Fig 3 would be better.

We changed to orientation of the array to maximise the size of the panels.

Page 7, line 9. "bottom-left panel" should be "bottom-right panel"

Typo corrected.

Page 7, line 17. "ice volume per latitude" should be "ice volume per degree of latitude". Similarly in Figure 3 (in the title of the bottom right panel) and in the caption.

Changes made.

Page 8, Figure 3. The eastern and western sectors should be marked in the upper panels (the maps).

Sectors are now marked.

Page 9, lines 2-3. "If the community judges the impacts..." ? maybe better to say "If further research finds the impacts..."

We made the suggested change.

Page 11. "Schwinger" should be "Schweiger"

Typo corrected.