

Review of A prognostic model of the sea ice floe size and thickness distribution
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In this paper, the authors propose a theoretical model of a joint sea ice floe radius/thicknesses distribution evolving in response to a prescribed flow, thermodynamic forcing, collisions and wave-induced breakage. The paper is interesting and potentially useful to large-scale models of the marginal ice zone. Its main contribution is the consideration of a joint size/thickness distribution, as opposed to Zhang et al. (2015)'s model that considered the thickness distribution to be independent of the floe size distribution. I hope the authors find my comments and suggestions useful.

I have two 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.
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.)

I have the following 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.
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)).
3. Page 2957, line 5: The recent publication Kohout et al. (2015) could be cited here.
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.
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.
6. Page 2959, line 5: Please quantify the 'large observation window' and 'point observations'.
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.

8. Page 2960, following equation 2: Please define the Laplacian operator and the physical domain.
9. Section 2.2, paragraph 1: Can the floes rebound following a collision and/or cause erosion of the floe edges?
10. Page 2965, lines 21–23 to page 2966, lines 1–4: This long sentence is unclear and should be rewritten.
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.
12. Page 2973, line 10: I suggest not referring to ‘wave-breaking’ here, as it is already reserved for a different phenomenon.
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).
14. Page 2974, line 19: The statement ‘wave fracture depends on their wavelengths rather than periods’ is not strictly correct.
15. Page 2975: How does the spectral model differ to that of Williams et al. (2013a)?
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).
17. Section 3: Have convergence and sensitivity tests been conducted?
18. Section 4: The opening paragraph doesn’t seem appropriate for a Conclusions section.
19. Page 2981, lines 21–22: Have the forcing fields been considered ‘when combined’?

Some technical corrections:

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

2. Page 2972, line 19: Delete ‘the’.
3. Page 2977, line 7: ‘Wave fracture’ → ‘ice fracture’ or ‘wave-induced fracture’.
4. Figure 3’s caption appears to be incorrect with respect to the labelling.

References

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