

Review of the revised manuscript *Wave-induced stress and breaking of sea ice in a coupled hydrodynamic–discrete-element wave–ice model*

by A. Herman

My comments and suggestions were addressed appropriately by the author and I support publication of the manuscript. I respect the author’s decision not to follow some of my suggestions, as it is her paper and should decide what general form the investigation takes. I think this is a significant piece of work which may attract much attention in the field of wave/ice interactions, and as such it is important to make it as easy as possible for the reader to go through the paper. My suggestions, e.g. comments 3 and 4 of the original review, were only made to help improve the clarity and readability of the paper in this respect. A few of my original points also seem to have been misunderstood by the author, so I clarify them below and suggest that the author makes further minor revisions in response. A few other minor comments are also detailed below.

Comments

1. What I meant regarding the limitation of “small water depth” is that the all the simulations are done for a water depth of 10 m which is relatively small for the range of wave periods considered. In particular, the effect of water depth on the wavelength in open water is important for wave periods larger than approximately 5 s. I understand that no shallow-water approximation was used here. I simply wonder whether it would be computationally feasible to perform simulations with larger water depth of $O(100\text{ m})$, for which deep water conditions are well approximated. The author partially addressed this in the response to my comment, but I think this should also be discussed in the manuscript, probably at the end of section 4.
2. The first two paragraphs of the Introduction are much improved. My original comment regarding the second paragraph mainly concerned the beginning of the paragraph and especially the long list of references of previous work on many aspects of wave/ice interactions. It is disconnected from the list of topics studied in this area provided earlier in the paragraph. In particular, it is unclear which paper studies what aspect of the problem. I suggest merging the two lists, i.e. topics and references, so an appropriate reference is given for each topic when it is mentioned. Also, there is an inconsistency in the reference Montiel et al. (2016). The corresponding item in the reference list is a paper published in *Annals of Glaciology* in 2015, not 2016. I assume the author intended to, or otherwise should, reference the paper by the same authors published in *Journal of Fluid Mechanics* in 2016, which is a generalisation of that of 2015.
3. Page 2, line 3: a more recent release of WW3 (The WAVEWATCH III Development Group, 2016) with more ice parametrisations is now available and should therefore replace

the reference to that of 2014.

4. Page 2, lines 7, 8: “Due to low temporal resolution of satellite data in polar regions, they provide only snapshots of sea ice conditions” needs to be rephrased as it does not read well. I suggest: “Due to their low temporal resolution in polar regions, satellite data only provide snapshots of sea ice conditions, ...”. A reference for recent advances in monitoring waves in the MIZ should be added, e.g. Arduin et al. (2017, *Remote Sensing of Environment*).
5. Page 2, line 12: is missing from “one of them IS ice breaking”.
6. Page 2, line 24: it will be unclear to most readers what “secondary ice-coupled waves” means. I suggest removing this or explaining more. I think the “basic mechanisms of wave-induced ice breaking” can be more simply described as flexural failure, as it is not mentioned before in the text that ice floes do flex under wave action. An explanation of the “secondary ice-coupled wave” can be given in page 13, line 30. The key here is that this damped oscillatory mode constructively interferes with the travelling mode (or primary wave) to give the maximum in strain at some small distance from ice edge.
7. Page 5, line 5: the observation that surge and drift are negligible in compact sea ice is not likely to be known and understood by all readers, so a reference may be useful here or additional explanation. I imagine the reason why a compact broken ice cover does not show surge motion is that individual floes are in contact and therefore continually collide, therefore restricting their ability to surge.
8. Page 10, line 12: replace “they” by “that” or “which”.
9. Page 12, line 4: I think “an individual wave” is slightly confusing. I suggest that the sentence is rephrased to clarify that the several breakup events occur within one wavelength.
10. Page 13, section 4: I’m having trouble getting my head around the statements that the location of breaking is independent from wave characteristics and that amplitude only acts as a switch that decides whether or not breaking takes place. As I understand it, the author’s argument is that breakup occurs where the primary and secondary ice-coupled waves constructively interact to produce a maximum in strain at a small distance from the ice edge. Changing the wave amplitude does not change the location of this maximum; I agree with that. However increasing the amplitude beyond a certain threshold will result in a situation where the primary travelling wave causes breakup well beyond this local strain maximum in a region where the secondary wave is insignificant. The primary wave will only attenuate due to dissipative processes or multiple scattering, which are slow attenuation mechanisms. Wave breakup events were reported hundreds of kilometres from the ice during the SIPEX-2 voyage in the Ross Sea as a result of large amplitude storm waves travelling through the MIZ almost unattenuated (see Kohout et al., 2016, *Deep Sea Research II*). This breaking mechanism is not captured by the present model and I think the author should simply acknowledge this in their conclusion.
11. Page 14, line 15: “directional with” should be “directional width” I imagine.
12. Page 14, line 23: replace “are” by “is”.