The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-95-RC1, 2017

The Cryosphere
Discussions
Open Access

Interactive comment on "Wave-induced stress and breaking of sea ice in a coupled hydrodynamic–discrete-element wave–ice model" *by* Agnieszka Herman

F. Montiel (Referee)

fmontiel@maths.otago.ac.nz

Received and published: 22 June 2017

The article describes a new two-dimensional time-dependent model of ocean waves interactions with sea ice. The model arises from coupling the incompressible, nonhydrostatic Navier-Stokes equations solver NHWAVE for the water domain with a discrete-element model (DEM) for the sea ice. The NHWAVE model is adapted from its original version to accommodate an ice cover with draught instead of a free surface. The DEM describes the ice cover as a discrete set of adjacent identical rectangular rigid bodies, referred to as grains, connected horizontally to each other by elastic beams, referred to as bonds, with same thickness as the grains. A coupled set of ordinary differen-

Printer-friendly version

Discussion paper

tial equations is derived for the angular and vertical velocity components of the grains, which accounts for the hydrodynamic forcing. The coupled water/ice equations are solved numerically with a second order Runge-Kutta time-stepping method. The model is used to investigate the response of a single floating ice floe with constant thickness under regular and irregular wave forcing, with the goal of analysing (i) the evolution of the stress field experienced by the floe and (ii) the floe size pattern obtained after repeated breaking of the floe.

I think this is an interesting and strong piece of work which addresses an important open scientific question. The manuscript is well written and the assumptions are clearly justified. The model described by the author is novel and the conclusions reached follow a reasonable interpretation of the results. My main concern is that the article lacks a detailed discussion on the advantages and drawbacks of this new modelling approach compared to existing waves/ice interactions models. For instance, it would be good to have an idea of the computational complexity of the model compared to others and the main parametric restrictions affecting this complexity, e.g. small water depth. I think it would have been nice to conduct a comparative study of wave propagation through an ice floe with an existing model for validation, but I understand this is not what this paper is about.

I support publication of the manuscript in *The Cryosphere* provided the authors address my comments below.

General comments

1. I think the Introduction section needs some work. To be more specific, I think it could be re-organised more efficiently so it is more relevant to the scientific question addressed in the rest of the manuscript. The first paragraph discusses (i) the geophysical context, i.e. Arctic sea ice reduction and the impact of ocean waves,

(ii) the parametrisation of wave/ice interactions in large scale forecasting models, (iii) the limited observational data, (iv) the attenuation rate of wave height in the MIZ and (v) the impact of waves on sea ice in the Southern Ocean. These are all relevant discussions, but they each need to be expanded and separated in different paragraphs. The second paragraph, on the other hand, lists the range of topics and associated publications on wave/ice modelling in the literature, even though it is not clear how they are relevant (if at all) to the present manuscript. I would suggest a more fit-to-purpose approach to the Introduction, in which the authors state their goals early on, and then discuss what has already been done to address these questions and how the new model proposed here goes beyond what is already established. I also suggest that the two paragraphs at the beginning of section 2, and before the start of section 2.1, are integrated as part of the Introduction, as they give a broad presentation of the two models used in this study, which is lacking in the current version of the Introduction. Additional comments on specific parts of the Introduction are given below.

2. As mentioned earlier, the manuscript needs to discuss in more depth other modelling studies on wave-induced breakup in the MIZ, particularly the papers referenced in page 2 (line 2), to which should be added the recent study by Bennetts et al. (2017) in *The Cryosphere*, in which the breakup is treated in a very different manner to that of the present model. More focus should then be added on how the present model differs from these existing models and what are its relative strengths and weaknesses.

3. The model described in section 2 is presented with much generality, and by doing so, the author introduced many variables that are later either neglected or set to be constant. I understand that the author wants to convey the generality of the modelling approach described here, but considering the main goals of the present study is to analyse the tensile stress in the bonds and breaking of a single floe with uniform properties, I do not think it is necessary to overcomplicate

Printer-friendly version

Discussion paper

the equations. More specifically, the turbulent diffusion terms in equations (3) and (4) are not needed and the quantities associated with the bonds and grains that are constant, e.g. thickness, width, mass, moment of inertia and elasticity parameters, do not need a subscript i. I also wonder whether it would make sense to neglect the shear stress component from the start, as it is found later in section 3.1 to be negligible compared to the tensile and compressive stresses. In this case, the bonds could be modelled as thin beams.

4. In section 3.1, I think it would make more sense to discuss figure 6 before figures 4 and 5, as it provides examples of stress profiles through the ice floe, which helps understand the parametric dependence analysis conducted in the other two figures.

5. I have trouble understanding the decay in vertical displacement and tensile stress with distance from the floe edge observed in the bottom panels of figure 3, in figure 6(a) and in figure (8). The model does not account for dissipative processes and multiple scattering (when multiple floes are present), so I wonder if this decay arises from the numerical scheme, in which case its impact on the results should be discussed, or non-linearities in the wave model, although it would be unlikely considering wave steepness is very small in all cases. The decaying behaviour is mentioned in page 10 (line 30), but its cause is not discussed. In any case, a discussion of this phenomenon is needed.

6. In the second paragraph of section 3.2, the author discusses how neglecting drift and surge motion, and the resulting floe-floe collisions, may lead to undersestimating wave attenuation rates and overestimating the extent of the zone of broken ice. Although I agree with this statement, I think it is also important to discuss the effect of neglecting multiple scattering by an array of floes after breaking has occured which, when accounted for, may lead to constructive or destructive interference and therefore affect the attenuation rate. This particular phenomenon is the

focus of the manuscript *Modelling wave-induced sea ice breakup in the marginal ice zone* by Montiel and Squire currently under review (article can be accessed on arXiv at `https://arxiv.org/abs/1705.05941`). The influence of multiple scattering on the breaking pattern is hard to estimate and I do not suggest that author attempts to do it, but I think it should be mentioned as a limitation of the present study.

Specific comments

1. page 1, line 1: I suggest to rephrase "the variability of wave-induced stress and breaking in sea ice" by "wave-induced stress and breaking in sea ice for a range of wave and ice conditions".
2. page 1, line 3: I do not think quotes are appropriate for joints or in all subsequent instances throughout the manuscript.
3. page 1, lines 4 and 5: I think "part" should be replaced by "module" for consistency.
4. page 1, line 14: I do not think "defining characteristic" is correct as other processes are important in forming the MIZ and several definitions exist in the literature Consider rephrasing slightly.
5. page 1, line 15: I suggest to replace "ice cover" by "ice-covered ocean" or "sea ice cover" to be more precise.
6. page 1, lines 15–20: these statements relate to the Arctic Ocean only, as it is quite a different story in the Southern Ocean. Make sure to specify this.
7. page 1, line 23: I suggest to replace "show" by "suggest".

8. page 1, line 25: “continuum models” should be defined.
9. page 2, line 2: if the purpose is to have an exhaustive list of studies considering parametrisations of wave-ice interactions in large scale models, the authors should also reference Bennetts et al. (2017, The Cryosphere) as well as hindcasts studies conducted with the spectral wave model WAVEWATCH 3, particularly Collins et al. (2015, Geophysical Research Letters), Li et al. (2015, Geophysical Research Letters), Ardhuin et al. (2016, Geophysical Research Letters) and Rogers et al. (2016, Journal of Geophysical Research).
10. page 2, lines 5 and 6: rephrase “Due to low temporal ... sea ice conditions”. A brief statement about recent advances in remote sensing techniques to monitor waves in the MIZ should also be included, as it is currently a very active area of research.
11. page 2, line 8: I do not think statements like “seemingly basic processes” are appropriate, as there is nothing basic about processes governing the interactions of ocean waves with sea ice. In addition, the example given on the attenuation rate of wave height is not a process, but merely an effect of the processes governing the propagation of waves in the MIZ.
12. page 2, line 11: I do not agree with the list of references given here to support the argument. Squire et al. (2009) and Vaughan et al. (2009) do not model wave attenuation in the MIZ but in non-fragmented pack ice, while Dumont et al. (2011) parametrise wave attenuation using the model by Kohout et al. (2008, Journal of Geophysical Research). In addition to the latter study, I suggest the following references: Bennetts and Squire (2012, Proccedings of the Royal Society A), Mosig et al. (2015, Journal of Geophysical Research) and Montiel et al. (2016, Journal of Fluid Mechanics), as different approaches to model wave attenuation in the MIZ.

13. page 2, lines 12 and 13: replace “a storm event” by “a field experiment” and “in height” by “in significant wave height”.
14. page 2, line 14: the observations of those breakup events were reported in Kohout et al. (2016, Deep Sea Research Part II).
15. page 2, lines 14–16: This sentence does not belong here. The discussion is about wave attenuation in the MIZ, while this is a general impact statement for wave-ice interactions.
16. page 2, lines 26 and 27: I think it would be appropriate to give a brief description of the “basic mechanisms of wave-induced ice breaking” mentioned here.
17. page 2, line 33 and 34: is the relationship between wavelength and floe size assumed or is it a consequence of the model as in Williams et al. (2013)? Please clarify this statement.
18. page 3, lines 18 and 19: it is unclear what the author means by “prohibiting inelastic effects from becoming significant” Please clarify this statement.
19. page 3, line 27: replace “module” by “modules”.
20. page 3, line 30: I think the author should say something about σ-coordinates or say that they will define them later, as most readers will likely be unfamiliar with them.
21. page 3, line 31: replace “(moving) ice” by “floating ice”.
22. figure 1: I find some aspects of the figure to be slightly misleading. More specifically, pressure and velocity points in the ice grains suggest a vertical variation of these quantities through the ice thickness, which is obviously not the case. I suggest that the author removes those and denote the center of mass instead.

Further, the situation in which an area of open water exists between two ice floes, as shown in the figure, is not considered in any of the simulations conducted in this study, so I think it would be sensible not to show this situation in the figure. I also think that it would be useful to include a sketch of the bonds in a bent situation, either on Figure 1 or in a separate figure. It would help understand their description at the beginning of page 5.

23. page 4, lines 29–31: the first part of this sentence is oddly constructed and hard to read. Also I do not agree with the conclusion reached in the second part of the sentence, as drift and surge motion do not depend on the compactness of the MIZ (or concentration). The significance of these phenomena depends on floe length and thickness, wavelength and incident wave amplitude.

24. page 5, equations (2)–(4): even though most readers will likely recognise these equations, I think it would be useful to introduce them, i.e. say what they mean.

25. page 6, equation (5): replace h by H.

26. page 6, below equations (11)–(17): I think that the author should introduce briefly the hydrodynamic forcing terms here, even though they are fully described later in section 2.2.3.

27. page 7, lines 4 and 5: I do not understand the meaning of this sentence. In any case, the statement seems to apply only when grain width varies from grain to grain, which is not the case here, so I wonder whether this statement is necessary.

28. page 7, lines 18 and 19: can the author give a reference to support this statement?

29. page 8, equations (8) and (9): these equations only account for the fluid pressure on the bottom surface of each grain. Does the author use modified formulae for

the end grains, which have a side surface in contact with the fluid? The contribution of the pressure field acting on these surfaces will modify the force and moment on the end grains.

30. page 8, equations (27) and (28): the parameter d in these equations is not defined.

31. Table 1: can the author explain the choice of tensile strength, as it approximately one order of magnitude smaller than typical values for sea ice (see, e.g., Timco and Weeks, 2010, particularly Figure 7 therein)?

32. page 9, line 15: does the author mean "modelled ice floe" instead of "model ice"?

33. page 9, lines 20 and 21: I think the author should introduce what is plotted in figure 3 in the text. Also please discuss the attenuation of z_i and σ_t, as suggested in my earlier comment.

34. page 9, lines 24 and 25: this statement is a bit too simplistic and not representative of what is seen in Figure 4(c). The figure shows $\sigma_{t,max}$ plateaus beyond a critical floe size, but the latter depends on thickness in a non-trivial way, as it is about 20, 100 and 50 m for $h = 0.5$, 1 and 2 m, respectively, so I am a bit confused by the statement that it is "equal to approximately two wavelengths", which is about 80 m.

35. page 9, lines 27 and 28: In addition to the large reflection, the author should mention that thicker floes tend to behave more like rigid bodies with lower strains (or curvature of the deflection function) and therefore decreased stresses.

36. page 9, lines 28 and 29: I think the author means that the location of $\sigma_{t,max}$ is independent on thickness, not $\sigma_{t,max}$ itself. It would also be useful to have seen Figure 6 before to understand this statement better, as suggested in my comment earlier.

37. page 10, lines 3–6: Again, I think this explanation could be improved by saying that at low and high frequencies, rigid body motions dominate over flexural motions, as demonstrated for instance by Montiel et al. (2013, Journal of Fluid Mechanics).

38. page 10, line 10: please rephrase “In the floe interior”, e.g. “Sufficiently far from the ice edge” or something similar.

39. page 10, line 11: can the author explain these ripples? Is it numerical or physical?

40. page 10, line 12: This statement seems to hold when the floe length is less than half the wavelength. Maybe the author could mention that.

41. page 10, line 17: I don’t understand the statement “An individual wave is responsible for a few breaking events”. Could the author clarify?

42. page 10, line 18: what is meant by “weaker ice”? Is it small thickness, tensile strength, ...?

43. page 11, line 1: can the author define or at least briefly describe a “Jonswap energy spectrum”?

44. page 11, line 15: I do not agree that breaking does not depend on “the characteristics of the incoming waves”, as clearly it will depend on wave amplitude. We can probably expect a linear relationship between incident wave amplitude and stress. This comment also applies to a similar statement in line 27.

45. page 11, lines 17 and 18: main result (iii) is rather obvious as there is not much else the stress can depend on!

46. page 11, lines 19 and 20: the author has not introduced the concept of “breaking probability” earlier in the manuscript, so it is confusing to have it in the conclusion. Maybe this could be slightly rephrased.

47. page 11, line 22: replace “might be not realistic” by “might not be realistic”.
48. page 11, lines 27 and 28: I think the author should also mention multiple scattering here, as discussed in my earlier comment.
49. page 11, lines 27–29: can the author include a reference to support the statement in brackets?
50. page 11, lines 29–32: I am having trouble agreeing with this statement, as I think the incident wave amplitude plays a determining role in creating the floe size distribution, i.e. the larger the wave amplitude the smaller the floes.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-95, 2017.