Response to reviewes
and a marked-up version of the manuscript
"Wave-induced stress and breaking of sea ice
in a coupled hydrodynamic–discrete-element
wave–ice model"

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

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General comments

First of all, I would like to thank the Reviewers for very insightful comments and valuable suggestions for corrections and modifications to the manuscript. I would also like to thank the Editors for selecting reviewers whose expertise covers both theoretical aspects of the manuscript, those related to the physics of wave–ice interactions, as well as numerics.

Below, I first reply to general issues raised by at least two Reviewers, and I also de-

scribe the major changes that have been made to the manuscript. In the further parts of this document, I reply to the particular comments of individual Reviewers.

In the revised manuscript, modified/new text is typeset in blue.

1. The most important general issue raised by the Reviewers seems to be the simulated wave attenuation clearly seen in the results and not discussed in sufficient detail in the text. The wave model was run without any dissipation mechanism (although it is worth mentioning that various such mechanisms, e.g., bottom friction or a turbulence model, are implemented in NHWAVE and can be readily used in the coupled model if necessary). NHWAVE often produces artificial attenuation, for example if the horizontal resolution of the model is too coarse relative to the modelled wavelengths, or when the number of layers is too small to reproduce the vertical structure of the wave motion. However, the model configuration used in the simulations presented in this manuscript was tested by running the wave model without ice – and no such effects were present. Therefore, as the Reviewers correctly remarked, the attenuation visible in the results takes place in the ice. (Another possible source of damping is in the coupling mechanism, e.g., when the coupling time step is not small enough — however, this was not the case in the present simulations.)

The numerical scheme used to solve the sea ice equations leads to damping, so that, effectively, the bonds do not behave perfectly elastic. The intensity of damping depends on wavelength – shorter waves get damped more than long waves – as well as the time step used. These are clear drawbacks and I'm going to improve the present scheme or replace it with a more robust one in the future versions of the model. Interestingly, it is a standard scheme used in the LIGGGHTS discrete-element model, and thus also used in the DESIgn sea ice toolbox. Apparently, the damping manifests itself particularly strongly in simulations with cyclic, fast time-varying forcing – as is the case in simulations with ice moving on waves. This is the reason why the problem remained unnoticed in previous applications of the DESIgn model done by myself and others. (Even more than that: in the default code of LIGGGHTS, an additional damping coef-

ficient is used, as it is assumed that it reduces spurious oscillations in bonds, without negatively affecting modelling results!)

However, it must be stressed that the wave attenuation within the ice plays a very minor role from the point of view of the analysis presented in this paper. Ice breaking takes place in a narrow zone close to the edge of the unbroken ice, where the effects related to the presence of this edge lead to enhanced wave amplitude and therefore strain. I performed tests in which the damping within bonds was artificially modified (by introducing a damping coefficient to the bond equations (15)–(17) and varying its value) and in terms of the floe sizes the results were exactly the same. In other words, reducing damping in the bonds is undoubtedly crucial for future applications of the model, but it does not affect the results discussed here. Nevertheless, I agree with the Reviewers that this issue should be clearly stated in the paper. I added information about damping to Section 3.1, in which issues related to the model setup are discussed, as well as to the discussion section at the end of the paper – please see the text of the revised manuscript.

The Reviewers also asked about the choice of the model parameters (ice strength etc.). As I said in the paper, I did not attempt to calibrate the values of the parameters to any real-world situation. Apart from the results that are presented in the manuscript, I made several more simulations with widely varying ice and wave characteristics, and none of these results affected the conclusions of the manuscript. The set of model parameters described in the paper is simply the one for which I collected the largest, the most complete set of results. This choice was affected by my present work, in which the model is used to reproduce the results of a laboratory experiment on ice breakup by waves. Thus, the fact that the wave amplitude in the simulations presented in the paper is so small and the ice strength so low can be seen as an "artefact" of this work. But once again: the results are very robust, and even though different model parameters would certainly produce different numbers (e.g., larger floes, with lengths closer to those reported from the field), the conclusions would remain the same.

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2. In response to the critics of the Reviewers, I rewrote/rearranged large parts of the Introduction – even though the modifications do not always follow the detailed comments of the Reviewers. In general, I tried to follow the advice to make the introduction more "fit to purpose". In particular, I removed the text on wave attenuation in the MIZ – first, because many parts of it were (rightfully!) criticized by both Reviewer 1 and 2, and second, because it is only broadly relevant to the topic of this paper. Also, the first part of section 2, providing a general introduction to the two models, has been moved to section 1, according to the suggestion of Reviewer 1. See the revised manuscript for the new version of the text, and my replies to individual comments below.

3. I added a new figure, showing top and side views of two grains connected with a bond, in order to illustrate the model components and definitions of the model properties in Section 2.1.

REPLY TO THE COMMENTS OF REVIEWER No. 1 (F. Montiel)

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.

Yes, this is beyond the scope of this paper – see point 1. above.

I don't understand the comment on the "small water depth". The water depth doesn't have to be small! The NHWAVE model does not make any assumptions about water depth. Moreover, instead of constant thickness of σ-layers, the water column can be divided into σ-layers logarithmically, so that if the water depth is large, the layers can be very thin close to the surface and thicker towards the bottom, reflecting the structure

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of the deep-water wave.

NHWAVE is a very flexible, nonlinear hydrodynamic model – and all its advantages can be easily used in coupled wave–ice simulations. In particular, there is no assumption regarding irrotational motion (as many wave–ice interaction models assume) or the form of the wave forcing – it does not have to be a sum of sine waves, any nonperiodic time series can be used as well. Additionally, NHWAVE can handle different types of boundary conditions (vertical, at the bottom etc.).

If the sea ice module is concerned, the manuscript discusses the limitations of the present version (e.g., the lack of motion in the horizontal plane; Section 2) and the plans of the future development (Section 4). In the revised manuscript, I extended the last paragraph of Section 4 so that it provides more details relevant to your questions. I also added some comments on (extremely high) computational costs of this modelling approach.

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.

Regarding the first part of this comment: The first paragraph of section 1 contains a general introduction (with topics listed by the Reviewer); the second and third paragraphs describe the literature related to SEA ICE BREAKING, not to sea ice–wave interactions in general. Yes, problems other than ice breaking are mentioned, but only two sentences are used for that (I admit, they are rather long). In my opinion, it's the first part of the introduction that can be argued to be not very relevant to the content of this paper, not paragraphs two and three. In particular, the quotes from Squire (1995) are crucial for the results presented in this paper – and they are part of the second paragraph, criticized by the Reviewer, who suggests expanding the first paragraph and reducing the following ones. Whereas I fully agree that the introduction should be made more "fit to purpose", my understanding of how this should be done seems to be opposite to that suggested by the Reviewer – see my general comment No. 2 above.

I followed the suggestion to move the first part of Section 2 to Section 1. I combined it with the text of the next to the last paragraph.

If making the introduction more "fit to purpose" is concerned, I added information on how ice breaking is treated in the existing parameterizations used in large-scale models, so that it is clear that breaking in these models is parameterized, not simulated explicitly. Please see the new introduction for the changes/additions that have been made. 4

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.

But the models in question parameterize the effects of breaking, they do not simulate breaking itself! I state this very clearly both in the new introduction and in the discussion section. In fact, the recent paper by the Reviewer (Montiel and Squire 2017) is the only one in which ice breaking by waves is in the focus of the analysis and in which no a priori assumptions on the FSD resulting from breaking is made! I added a reference to that paper to the revized introduction.

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 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.

I understand these objections, and I myself spent some time considering the advanteges and disadvantages of these two alternatives – adjusting the model description to the configuration used in the paper, or keeping it more general in order to illustrate a wider range of applications of the model. I decided to keep the model description more general. I don't think that introducing grain and bond indices in the sea ice equations introduces significant complications to the model description or make it less clear – especially that many grain and bond variables (instantaneous forces, stresses, velocities,

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etc.) need an index anyway, as they vary from grain to grain and from bond to bond.

As for the shear stress in the bonds: I don't see a good reason for making any a priori assumptions regarding its amplitude. I think it is more valuable to have the relative "importance" of different stress components as a result of the simulations rather than to start with simply disregarding some components. If the shear stress was simply set to zero, one could ask how do I know that it does not contribute to the breaking process. . .

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.

Figures 4 and 5 (or, according to the new numbering in the revized manuscript, 5 and 6) are related to the first, major stress maximum close to the ice edge. I think that the knowledge of the stress variability along the whole floe is not necessary to the understanding of how the value and location of this major stress maximum changes with ice thickness or incomming wavelength. I find the version you propose, with a description of the stress variability across ice floes followed by the analysis of the stress maximum, equally logical and justified. I decided to keep the first version, only because I don't see any important reasons to change it.

However, I made a number of small modifications to this part of the text, following your detailed comments (see further).

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 nonlinearities 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

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its cause is not discussed. In any case, a discussion of this phenomenon is needed.

See my general comment on attenuation in the first part of this document.

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.

Yes, I agree that the effects of scattering might be important and are hard to estimate.

I added a comment on multiple scattering to the text in the second paragraph of section 3.3 (previously 3.2). I also made a comment that the model is capable of simulating multiple scattering, but not in the configuration that was used in this paper – see the modified manuscript.

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".

Changed as suggested.

2. page 1, line 3: I do not think quotes are appropriate for joints or in all subsequent instances throughout the manuscript.

That's true, the quotes have been removed.

3. page 1, lines 4 and 5: I think "part" should be replaced by "module" for consistency.

Changed as suggested.

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.

The sentence says it is "A defining characteristic", not "THE difining characteristic". It doesn't suggest that it is the only one. Although the term MIZ is used in a different way by different authors (as the second part of the sentence in question says), I don't think anyone would claim that the influence of waves is not an inherent feature of MIZ.

5. page 1, line 15: I suggest to replace "ice cover" by "ice-covered ocean" or "sea ice cover" to be more precise.

Changed as suggested.

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.

The phrase "in polar and subpolar regions" has been extended to "in polar and subpolar regions of the northern hemisphere".

7. page 1, line 23: I suggest to replace "show" by "suggest".

Changed as suggested.

8. page 1, line 25: "continuum models" should be defined.

I added the text "(i.e., those in which ice is treated as a continuous mass rather than as discrete particles)" to this sentence.

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).

Do you mean the paper by Collins et al. titled "In situ measurements of an energetic wave event in the Arctic marginal ice zone"? This one is a case study in which a SWAN model is used that does not take into account any ice-related effects. No parameterizations of wave-ice interactions are proposed. The papers of Li et al. (2015), Ardhuin et al. (2016) and Rogers et al. (2016) present very interesting and valuable results on wave propagation in sea ice, but they use the existing parameterizations that are implemented in the Wavewatch model – and this is what I should have cited, I think. I added a reference to Tolman and the WAVEWATCH III® Development Group (2014) to the revised text, as well as a reference to Bennetts et al. (2017).

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.

This sentence is a statement about the TEMPORAL RESOLUTION of remote sensing data from polar regions. I'm not an expert in remote sensing, but to the best of my knowledge, temporal resolution of satellite data at high latitudes remains a problem. In their recent review paper by Ardhuin et al. (2017, Ocean Science Discuss.), they write that "Ice concentration is the only parameter that is well monitored near the ice edge" (section 4.2). I slightly modified the next sentence so that the advances in remote sensing methods are briefly mentioned (see text).

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

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wave height is not a process, but merely an effect of the processes governing the propagation of waves in the MIZ.

I agree that the statement is unfortunate. I rewrote it. I also removed the text following it, as already mentioned in my comments above.

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.

This text (the whole paragraph) has been removed (see my general comments above).

13. page 2, lines 12 and 13: replace "a storm event" by "a field experiment" and "in height" by "in significant wave height".

This text has been removed.

14. page 2, line 14: the observations of those breakup events were reported in Kohout et al. (2016, Deep Sea Research Part II).

This text has been removed.

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.

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This text has been removed.

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.

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I extended the sentence to: "basic mechanisms of wave-induced ice breaking, related to the presence of secondary ice-coupled waves affecting the wave envelope close to the ice edge and rapidly decaying away from the edge."

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.

In Williams et al. (2013) the relationship is not a "consequence of the model", as you suggest, but it is assumed! At the beginning of their section 3.2.2 they write: "If it [the ice] breaks, the maximum floe size is set to $D_{max} = \max(\lambda_w/2, D_{min})$". The statement is correct.

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.

All elastic plate models of sea ice are based on this assumption. My statement is almost a direct quote from Fox and Squire's paper ("elasticity is justified because of the oscillatory nature of the problem which does not allow anelastic processes to act in any significant way.").

19. page 3, line 27: replace "module" by "modules".

The phrase has been changed to "the sea ice module and the wave module".

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.

I added "(see further Section 2.2.1)" at the end of this sentence.

21. page 3, line 31: replace "(moving) ice" by "floating ice".

Changed as suggested.

22. figure 1: I find some aspects of the figure to be slightly misleading. More specifi-

cally, 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.

As for the first part of this comment: the wave module DOES solve the hydrodynamic equations within the ice as well. This is the core idea – and the strength – of immersed boundary methods, especially in applications in which the submerged objects are moving relative to the grid of the hydrodynamic model (so that "wet" and "dry" grid cells change from time step to time step). Solving the Poisson equation for pressure is computationally expensive, and therefore it is beneficial if the coeffcient matrix does not have to be rebuilt at each time step, but can be built only once instead. In immersed boundary methods, the influence of boundaries is handled through the source terms – see the papers I'm citing for details – and the values of pressure are determined in each grid point, independently of it being "wet" or "dry" at the given time instance. But of course, the points inside submerged bodies are not considered when the modeling results are analyzed. I added a short information on that to the caption of Fig. 1.

As for the remaining parts of this comment: As I wrote in the first, general part of my reply, I decided to keep the description of the model (and consequently also the Figure illustrating it) general. I also added a figure illustrating two grains connected with a bond, with relevant variables etc.

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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.

As I understand it, at 100% ice concentration the ice cover may drift as a whole, but the oscillatory surge motion (i.e., different horizontal velocity of neighboring ice floes) would require ridging or rafting to occur in areas of convergence. Last year I took part in a laboratory experiment of (broken) ice motion on waves, and we recorded almost perfectly vertical motion of the ice, with vanishing horizontal velocity component – as long as the ice was compact.

I rephrased slightly the first part of this sentence, so that it is easier to read.

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.

I added a short comment that "the governing equations are the mass and momentum conservation equations".

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

Corrected.

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.

I moved the sentences introducing these terms ("Finally, the first terms on the right-hand-side of (13) and (14) denote the net moment of forces and the net vertical force, respectively, from the wave motion underneath the ice. They are calculated by integrating the contribution from waves over the wetted surface of the grains. Their detailed formulation is given further in Section 2.2.3.") to the paragraph directly following Eqs. (11)–(17).

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.

The torque, being a product of force and position vector, is different for grains of different sizes, even if the force transmitted by the bond connecting those grains is the same. I think this sentence is clear.

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

In fact, essentially all "brittle" materials are not prefectly brittle and exhibit a certain level of softening when the stress acting on them exceeds their strength, i.e., the stress does not drop to zero instantaneously, but within a certain time $t > 0$. As I noted in the revised text, some DEM models take this softening into account in order to obtain more realistic breaking behavior. I cited a review paper by Lisjak and Grasselli (2014).

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.

I suppose you mean Eqs. (25) and (26). The contribution of pressure acting on the vertical surfaces of end grains is crucial for the horizontal momentum balance of those grains – which is not solved in this version of the model, as it is assumed that $u \equiv 0$. Of course, as the grains move on the water surface, their tilt θ_i is different from zero, so that the contribution of pressure acting on vertical grain surfaces to the *vertical* force is not exactly zero. It is proportional to $\sin \theta_i$ and to the surface area of the vertical wall of the grain, whereas the pressure-related force acting on the lower surface (given by Eq. 28) is proportional to $\cos \theta_i$ and surface area of that surface, i.e., it is much larger. But of course the contribution from vertical surfaces can be easily added to the formulae (25) and (26).

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

It's a mistake, thank you for pointing this out! It should be Δx instead. It has been corrected in the revised manuscript.

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)?

See my general comments at the beginning of this document.

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

I think it amounts to the same thing in this context, but I changed it as suggested.

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.

I modified this fragment so that it includes more details about the content of Fig. 3.

As for attenuation, see my general comment No. 1.

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 50m 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.

I added the text "the stress saturates to a value specific for the given ice thickness", to make it clear that *for a given ice thickness* the value of stress does not change up from a certain floe size. I also changed "two wavelengths" to "one to two wavelengths", as this is where the curves in Fig. 4c,d become approximately horizontal.

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.

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I added this information to the revised text.

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.

Yes, I meant the location of $\sigma_{t,max}$. I corrected this.

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).

I added this reference to the text and modified it so that it is clear that both wave reflection and the response of the ice itself are responsible for this effect.

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

Changed as suggested.

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

As I understand it, they are physical, and they are in fact analogous to modulations of the wave envelope at the upwave end of a floe, close to the ice edge (that are observed and predicted by models).

There is a paper by Yoon et al. (2014), in which they analyze experimentally and numerically the motion of floating plates on waves. They observe (and predict with their model) an increase of the amplitude of the vertical motion at the downwave end of the plate (see e.g. their Figs. 7a, 11a, 12b – for the plates without hinges). Figs. 3 and 4 in Kohout et al. (2007) show a similar pattern. As in both papers the model is compared with laboratory experiments, the range of the parameters considered (wavelength, floe size, etc.) is limited by the setup of the experiment. In particular, the ratio of the plate length to the wavelength is relatively small, and the effects of attenuation are much

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weaker, so that there is no smooth region between the fluctuations at the upwave and downwave end of the plates (like in our case with $L_o = 250$ and $L_o = 500$ m, Fig.6a), but the overall pattern is similar. I added references to Yoon et al. and Kohout et al. to the revised manuscript.

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.

As Fig.6b shows, the response of floes smaller than half the wavelength is almost exactly symmetrical, with maximum stress in the middle of the floe. When the floe length increases beyond half the wavelength, the symmetry gradually vanishes, but at first no second maximum is present – see the curve for $L_o = 25$ m (the wavelength in this figure is 42 m). I added this information to the modified text.

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?

Take a closer look at Figs. 7b and 9b; for example, in 7b, two or three breaking events (pink dots) take place within one wave period (graphically, within one inclined yellow stripe representing a wave propagating into the ice).

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

I wrote it explicitly: "In thinner ice..."

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

I added the text "(one of widely used idealized models of wave energy spectra, suitable for a wide range of wind and fetch conditions)" to this sentence.

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.

The amplitude is important only in the sense that it must exceed certain threshold. Breaking occurs as soon as the stress exceeds the strength of the ice – it cannot reach higher values. See also my comments below (No. 45 and 50).

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

I agree that taken alone it is quite obvious – but it should be read together with result (ii): the wave characteristics decide *if* the ice break, but not *where* it breaks.

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.

Ice breaking probability is mentioned in the introduction in relation to the work of Vaughan and Squire (2011), but you are right, this term has not been properly defined. I think it is clear in the context of the paragraph in which it is used, but to avoid confusion I changed it to a simple statement: "... and thus breaking is most likely to occur."

47. page 11, line 22: replace "might be not realistic" by "might not be realistic".

I wrote "might be unrealistic" instead.

48. page 11, lines 27 and 28: I think the author should also mention multiple scattering here, as discussed in my earlier comment.

This part of the discussion has been largely modifed/rewritten – see the revized manuscript.

49. page 11, lines 27-29: can the author include a reference to support the statement in brackets?

It is hard to include just one (or even a few) references, as the great majority of studies of FSD in MIZ demonstrate the existence of wide, heavy-tailed FSDs. I added reference to Toyota el el. 2011 and 2016, as they are recent and themselves contain a lot of references to papers on FSD.

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.

The larger the wave amplitude the smaller the floes... If one consideres a whole range of breaking mechanisms that contribute to breaking and therefore influence the FSD (floe–floe collisions, overwash, rafting, etc.) then certainly: yes. But one should keep in mind how the parameterizations of wave-induced breaking for large-scale sea ice models work: it is assumed that the FSD has a prescribed shape (e.g., a power law with a certain slope) and *only the maximum floe size L_{max} is allowed to vary.* L_{max} is related to bending stresses (all floes larger than L_{max} break due to bending). All other breaking mechanisms that I mentioned above and that – as you suggest – are wave amplitude dependent, are included in the power-law part of the FSD, as it is assumed that they produce scale-invariant pdfs of floe sizes.

The results of the present model (and the earlier ones by Squire and colleagues) suggest that L_{max} should be a function of ice properties, not wavelength. If bending stresses are concerned, the only relevant thing is whether the wave amplitude exceeds a threshold required for breaking or not. This is what I had in mind when I wrote this sentence ("the information on incoming waves is used to determine whether breaking of ice takes place"). But I agree that the wave amplitude (or rather: steepness) has influence on FSD as a whole, and presumably modifies the exponent of the distribution.

In the revised manuscript, I modified/extended the statement in question in order to make it more clear.

REPLY TO THE COMMENTS OF REVIEWER No. 2

2.1. p2: 'For example, the functional form describing the rate of change of wave height with distance from the ice edge is far from established...' Meylan et al (2014) found exponential decay from this dataset when the individual frequencies are considered instead of the significant wave height. Li et al (2015) were able to qualitatively produce the linear decay of the significant wave height with Wavewatch 3 – proposing that the nonlinear source term S_{nl} was the reason, as it moves energy to lower frequencies which are attenuated more slowly.

As I wrote in the general comments above, I decided to remove this text from the revised manuscript.

2.2. p8: please give the equations for the free-slip boundary conditions.

Yes, thank you for pointing this out. The free-slip boundary conditions at the bottom were given in Eq. (6), and they should be written together with Eqs. (21)-(22) and (23)-(24) as well (for the horizontal and vertical velocity components, respectively). I have added the missing formulae, and rearranged the order of equations so that they directly correspond to (5)-(7) in Section 2.2.1.

2.3. equation (7): Ma et al (2012) has the total derivative of w in the bottom condition (dw/dt). Maybe this is a typo? Similarly, perhaps the pressure condition at the lower surface of the ice (21) should be checked?

Yes, the first mistake is a typo, the second one – an effect of copying/pasting this formula... Thank you for noticing it!

2.4. p6: How are the waves generated?

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The last sentence in section 2.2.1 says: "In the model applications presented in this work, sponge layers are applied at the left and right boundary, and waves are generated inside the model domain (Ma et al., 2014)." Both things – sponge layers and internal wave generation – are working exactly as described in the NHWAVE technical

documentation and in papers describing the model. These parts of the NHWAVE code are unaffected by the coupling with the ice module. In the revised manuscript, I added a short comment to the above sentence to make it more clear.

2.5. §3.1 Stress variability in continuous ice
(i) The locations of $\sigma_{t,max}$ seem to occur about 10m away from the ice edge, which seems very small, eg. Squire et al (1995): "Anecdotally it appears that incoming waves and swell cause a fracture line to develop a few tens of meters back from the ice margin and parallel to it". Was there any attempt to tune the wave number in the ice to a realistic value, eg. for a thin elastic plate? Perhaps these numbers would increase if such tuning were done.
(ii) Presumably the damping of the waves is due to some damping in the bonds, although this is not mentioned anywhere. Perhaps if this was removed it could be useful to see how much of the attenuation is numerical and how much is physical. The reflection and transmission coefficients could also be compared to linear models for a semi-infinite elastic plate. It would also be interesting to see if this affects the conclusions about the location of $\sigma_{t,max}$ being independent of wavelength or not.

See my general comments on the damping of wave energy in the model and on the choice of the model parameters.

2.6. §3.2 Breaking of uniform ice by regular waves
(i) It would be interesting to see fig 8 without damping in the bonds and also the time evolution of the wave amplitude to see if there is more or less attenuation after breaking – on one hand the floes produced are small compared to the wavelength but on the other hand there are many of them and perhaps multiple scattering could do something.

Again: see my previous comments. Also, I think that Fig. 9 (previous Fig. 8) at least partly answers your question, as it compares the wave amplitude in simulations without and with breaking.

REPLY TO THE COMMENTS OF REVIEWER No. 3 (A. Kulchitsky

2.1.1. My main concern about this work is the lack of verification of the approach and the code. For example, does discretization step Δx change the outcome of the computations? It is unclear until some scaling tests are done. I would suggest to use different Δx with the same wave input and compare the results. This can be corrected within this paper by running some tests with Δx being a half of the Δx used in the paper and comparing the results. Maybe a reference that the final results do not change with Δx within the numerical accuracy would be sufficient.

I fully agree that some comments on the influence of the model resolution on the results should be added to the manuscript. For the simulations presented in this work, the ratio of Δx to the length of the waves is crucial and should not exceed a certain limiting value (this is true for standalone NHWAVE model as well). Similarly, the number of σ-layers, N_l, must be sufficient to capture the vertical structure of the waves. Before running the model for the cases described in the manuscript, several "verification runs" were conducted. In computations without ice, the wave model with prescribed Δx and N_l was run for a set of wavelengths in order to estimate the shortest wave that can propagate in the model without artificial damping related to insufficient model resolution (we expect zero attenuation, because the dissipation was turned off). The shortest waves considered in the "proper" simulatons are longer than the limiting wavelength found in this way.

As for the coupled waves+ice simulations, the situation is more complex. In all DEM models, the macroscopic properties of the modelled material are sensitive not only to microscopic properties of bonds connecting the grains, but also to the grain size distribution. This is well known, and selecting grain size is a standard part of the process of calibration of a DEM model to a particular situation – see, e.g., Potyondy and Cundall, Int. J. Rock Mech. Mining Sci., 2004; Koyama and Jing, Engng Analysis with Bound. Elem., 2007; there many other papers on the subject. Generally, changing the size of grains changes the macroscopic behavior of the modelled material, or, in

other words, the grain size is not a free parameter that can be varied without affecting the properties of the material. In our case, if Δx is changed, the ice response to waves changes. Or, *vice versa*, in order to have the same ice response with different Δx, the model parameters have to be adjusted. In the study on which I am working on right now, the model is going to be applied to a laboratory setting, in which a continuous ice sheet was broken by regular waves. Calibration and validation will be a crucial element of the modelling process. In the work presented in this manuscript, however, no attempt to reproduce any particular real-world situation was made, and therefore the role of Δx is not analyzed.

I added information on that to the first part of section 3 of the revised manuscript.

2.1.2. I have some concerns regarding 2D approach to the problem of breaking an ice floe. It is just hard to imagine how to interpret the results of a single ice flow breaking in long (y-dimension) but narrow (x-dimension) fragments as the results of simulations show. Actual floe would break along x dimension as well such that it will be easier to break later. I suppose 2D approach is still applicable for testing in channels. This issue is not to be corrected and just brought to the discussion at this point.

Yes, the fact that the model has only one horizontal dimension is an important limitation. Even though many observations show that the shape of ice floes detached from the edge of a continuous ice sheet by swell waves can be very elongated, i.e., the floes' size in the wave propagation direction is much smaller than their size in the perpendicular direction, obviously sea ice breaking by waves takes place in two horizontal dimensions. This fact is related to nonuniformities of the sea ice properties as well as to the directionality of the incomming waves (both the mean wave direction, which doesn't have to be perpendicular to the ice edge, and directional spreading). These effects cannot be taken into account in the present model, but the 2D-V formulation described in this manuscript should be treated as a "proof of concept" for a more general model based on NHWAVE and DESIgn.

2.2.1. I struggled to understand the 2.1 and 2.2 sections with just Figure 1. It would be very useful to add dimensions, indices and more notes to the figure. In this case the reader would see what Δx, l_i, etc. are. Maybe a single cell scheme with forces and torques shown, as well as velocities, constraints ($u_i = 0$) and bonds.

As already mentioned in my general comments, I added a new figure showing a sketch of two grains connected with a bond, with relevant dimensions of these objects etc. I hope this makes the description of the model components in section 2 easier to understand. I also added the axes (x,z) to Fig.1.

2.2.2. Sec. 2.1, p. 5, 0-5. "All bonds are cuboid". Should it say "All bonds are elastic"?

No. This statement refers to the shape of the bonds, not their material properties.

2.2.3. Equations (13) and (14) include terms for the torques and forces imposed from water ($M_{wv,i}$ and $F_{wv,i}$) that are only explained 2 pages later in the coupling section. They should be defined after the equations as well.

These terms are explained in Section 2.2.2: "Finally, the first terms on the right-hand-side of (13) and (14) denote the net moment of forces and the net vertical force, respectively, from the wave motion underneath the ice. They are calculated by integrating the contribution from waves over the wetted surface of the grains. Their detailed formulation is given further in Section 2.2.3." In the revised manuscript, I moved this sentence to the paragraph that directly follows Eqs. (11)–(17). I agree that it should have been there from the start.

2.2.4. I have some questions regarding "classical beam theory" equations (18)-(20). First, the length of the bonds is defined generally as $l_{b,i} = \lambda\Delta x$ (page 5, 0-5). Please, verify that $l_{b,i} = h_{b,i}$ for these computations. Moreover, I suggest to introduce a picture showing all the stresses on the bonds.

No, $l_{b,i}$ does not equal $h_{b,i}$. The first is the bond dimension in the x-direction, the second – in the z-direction. Thus, $h_{b,i}$ corresponds to ice thickness and, as the text

in the first part of Section 3 says, in the simulations presented in the manuscript the thickness of grains and bonds was equal.

In equations (18)-(20), the forces are divided by the bond cross-sectional area, but as the width of the bonds (and grains, for that matter) equals 1, only $h_{b,i}$ is left.

As for the stresses acting on the bonds, the new Fig. 2 shows the locations of the maximum compressive and tensile stresses acting on the bond and resulting from relative rotation marked in red.

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Wave-induced stress and breaking of sea ice in a coupled hydrodynamic–discrete-element wave–ice model

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Abstract. In this paper, a coupled sea ice–wave model is developed and used to analyze wave-induced stress and breaking in sea ice for a range of wave and ice conditions. The sea ice module is a discrete-element bonded-particle model, in which ice is represented as cuboid "grains" floating on the water surface that can be connected to their neighbors by elastic joints. The joints may break if instantaneous stresses acting on them exceed their strength. The wave module is based on an open-source version of the Non-Hydrostatic WAVE model (NHWAVE). The two modules are coupled with proper boundary conditions for pressure and velocity, exchanged at every time step. In the present version, the model operates in two dimensions (one vertical and one horizontal) and is suitable for simulating compact ice in which heave and pitch motion dominates over surge. In a series of simulations with varying sea ice properties and incoming wavelength it is shown that wave-induced stress reaches maximum values at a certain distance from the ice edge. The value of maximum stress depends on both ice properties and characteristics of incoming waves, but, crucially for ice breaking, the location at which the maximum occurs does not change with the incoming wavelength. Consequently, both regular and random (Jonswap spectrum) waves break the ice into floes with almost identical sizes. The width of the zone of broken ice depends on ice strength and wave attenuation rates in the ice.

1 Introduction

Interactions between sea ice and waves are a defining characteristic of the marginal ice zone (MIZ), loosely defined as a region of the sea ice cover adjacent to the ice edge and directly influenced by the neighboring open ocean. In recent years, as the sea ice extent in polar and subpolar regions of the northern hemisphere decreases and thick, multi-year ice is replaced with thinner, weaker seasonal ice, conditions typical for MIZ (ice concentration lower than 90%, small floe sizes, patchy distribution of floes on the sea surface, etc.) tend to occur over larger and larger areas. There is a growing observational and modeling evidence that wave–ice interactions play an important role in the observed expansion of MIZ and negative trends in sea ice extent (see, e.g., Asplin et al., 2012, 2014; Thomson and Rogers, 2014; Thomson et al., 2016). Thin, fragmented sea ice is susceptible to further breaking and, depending on ambient weather and oceanic conditions, melting, which facilitates faster ice drift, decrease in ice concentration, increase in wind fetch, and thus creates more favorable conditions for wave propagation and generation, leading to still stronger fragmentation. These – and many other – feedbacks suggest that it is crucial to include (the effects of) wave–ice interactions in numerical ocean–sea ice–atmosphere models in order to be able to reliably reproduce the observed processes and forecast future changes on both synoptic and climate scales. Parameterizations of wave–ice interactions for large-scale,

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continuum models (i.e., those in which ice is treated as a continuous mass rather than as discrete particles) are crucial for further development of those models. However, although appreciable effort has been made in that direction in recent years (Dumont et al., 2011; Doble and Bidlot, 2013; Squire et al., 2013; Williams et al., 2013, 2017; Tolman and the WAVEWATCH III$^\circledR$ Development Group, 2014; Bennetts et al., 2017), our understanding of many aspects of wave–ice interactions is still too limited

5 to allow formulating such parameterizations, especially those suitable for a wide range of conditions. Strong fragmentation of the ice into many small floes, and highly energetic environment due to the presence of waves make the MIZ a very difficult, demanding location for field work. Due to low temporal resolution of satellite data in polar regions, they provide only snapshots of sea ice conditions, making it difficult or impossible to infer details of processes acting on time scales comparable with a typical wave period. Therefore, in spite of recent advances in remote sensing techniques to monitor waves in the MIZ, the

10 amount of observational data necessary for validation of numerical models remains very limited. Consequently, many crucial processes and their large-scale effects are only poorly understood. As the overview of the relevant literature in the following paragraphs clearly shows, one of them sea ice breaking by waves and the resulting floe-size distribution (FSD) – the main subject of this paper.

Review papers by Squire et al. (1995) and Squire (2007, 2011) provide a good overview of the state-of-the-art research
15 related to wave–ice interactions. Problems studied in this context include, but are not limited to: wave propagation, attenuation and scattering by various ice types, e.g., continuous ice sheets, broken compact ice, (groups of) individual ice floes, and inhomogeneities like pressure ridges, cracks etc.; motion of ice floes (and other floating objects, including very large floating structures) on waves and wave-induced floe collisions; sea ice breaking by waves. Considering relatively rich literature on wave propagation in sea ice and wave-induced motion of ice floes/sheets (see, e.g., Squire, 1983; Liu and Mollo-Christensen, 1988;
20 Shen and Ackley, 1991; Meylan and Squire, 1994; Meylan, 2002; Wang and Shen, 2011; Montiel et al., 2012, 2016; Sutherland and Rabault, 2016, and references there, as this list is by far not complete), the number of studies on sea ice breaking by waves is remarkably limited and – as Squire et al. (1995) aptly put it – they are to a large degree based on "anecdotal evidence". In a series of papers published in 1980s, V. Squire analyzed wave propagation in continuous, land-fast ice and basic mechanisms of wave-induced ice breaking, related to the presence of secondary ice-coupled waves affecting the wave envelope close to
25 the ice edge and rapidly decaying away from the edge (see, e.g., Squire, 1984a, b). In their review paper, Squire et al. (1995) describe qualitatively the process of breaking of land-fast ice by swell waves, in which elongated, parallel strips of ice are progressively separated from the initially continuous ice sheet. They write that "the width of the strips, and hence the diameter of the floes created by the process, is remarkably consistent and appears in the sparse evidence available to be rather insensitive to the spectral structure of the sea, but highly dependent on ice thickness." Consistently, their modeling results showed that
30 the location of maximum flexural strain in the ice relative to the ice edge depends mainly on ice thickness rather than wave period. Notwithstanding these conclusions, a close relationship between the incoming wavelength and floe sizes produced by breaking is usually assumed, as for example in the above-mentioned parameterizations by Williams et al. (2013), Bennetts et al. (2017) and others. It is worth stressing that these models do not directly simulate the sea ice breaking process. Instead, they simulate the *effects* of breaking by testing if the conditions are favorable for breaking (criteria based on the wave height
35 and thus strain that the ice experiences) and, if these conditions are fulfilled, by modifying the maximum floe size $L_{\max}$

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according to certain, prescribed rules. The shape of the FSD for floe sizes $L_o < L_{\max}$ is prescribed as well, so that $L_{\max}$ is the only variable parameter characterizing the FSD. In other words, these models are suitable for analyzing the consequences of wave-induced breaking of sea ice (i.e., the influence of the evolving FSD on ice dynamics and/or thermodynamics), given the assumed relationships between the FSD and the wave forcing. Thus, as with any parameterization, our understanding of the processes involved decides upon the validity and accuracy of the modelling results.

Since the pioneering works described above, few studies have been devoted specifically to the analysis of sea ice breaking by waves. In a modeling study of ice motion on waves, Meylan and Squire (1994) analyzed flexural strain variability in ice floes of different sizes and thicknesses. Langhorne et al. (1998) analyzed experimentally and numerically the fatigue behavior of first-year sea ice subject to repeated bending stress and demonstrated that the time history of strain acting on the ice is crucial for predicting its breaking. In a subsequent work, Langhorne et al. (2001) extended their earlier work to estimate lifetime of landfast ice subject to waves with given characteristics. Based on ship observations of ice breaking during a strong-wave event in the Barents Sea, Collins et al. (2015) analyzed the role of nonlinear wave processes and the resulting strong modulation of wave amplitude in ice breaking, in accordance with much earlier observations and theoretical results of Liu and Mollo-Christensen (1988). Vaughan and Squire (2011) estimated ice breaking probabilities in the Arctic sea ice in function of the distance from the ice edge, based on the probability density functions of the sea surface curvature. This approach, employed also by Kohout and Meylan (2008), assumes a simple relationship between strain (estimated directly from the shape of the wave profile) and stress in the ice. Finally, sea ice breaking is included in the recent model of wave–sea ice interactions by Montiel and Squire (2017). In simulations of wave propagation and multiple scattering by circular ice floes in MIZ, they used strain-based floe breaking criteria and obtained approximately normal FSDs, without any *a priori* assumptions regarding their shape.

In this paper, a coupled sea ice–wave model is proposed suitable for simulating ice–wave interactions in the time domain, including computation of instantaneous stresses in ice and ice breaking. The model consists of a bonded-particle discrete-element sea ice model, similar to that of Herman (2016), and a wave model based on the code of the Non-Hydrostatic WAVE (NHWAVE) model by Ma et al. (2012, 2014). The two parts are coupled with proper boundary conditions exchanged at every time step. The type of a discrete-element model (DEM) used here, in which bonds connecting grains behave as elastic "rods", is particularly suitable for studying sea ice–wave interactions due to oscillatory nature of these processes, prohibiting inelastic effects from becoming significant (see, e.g., Fox and Squire, 1994).

Apart from providing a detailed description of the model, the main goal of this work is, first, to analyze spatiotemporal variability of wave-induced stress in ice floes with varying thickness and sizes, and second, to analyze the time evolution of breaking and the final breaking patterns produced by regular and irregular waves. The paper is structured as follows: Section 2 contains the definitions and assumptions underlying the model, followed by the description of the model equations and coupling between the wave and ice modules. The results of simulations are presented in Section 3. Finally, Section 4 provides a discussion and a summary.

3

2 Model description

The model consists of two parts, the sea ice module and the wave module, exchanging information at every time step. The wave part is based on the Version 2.0 of the Non-Hydrostatic WAVE (NHWAVE) model developed by Ma et al. (2012) and available at https://sites.google.com/site/gangfma/nhwave. NHWAVE solves three-dimensional incompressible Navier-Stokes equations in vertically-scaled σ-coordinates (see further Section 2.2.1). For the purpose of this work, NHWAVE has been extended to allow non-free surface boundary conditions under the (floating) ice, as described in detail further in Section 2.2.3. The second component is a discrete-element bonded-particle sea ice model. It is based on similar ideas and assumptions as the DESIgn model by Herman (2016), with certain modifications crucial for representing ice motion and bending on the oscillating sea surface (in DESIgn, which is essentially two-dimensional in the horizontal plane, these effects are treated in a very rudimentary way, with a number of unrealistic assumptions).

Recently, Ma et al. (2016) and Orzech et al. (2016) implemented in NHWAVE equations for floating objects and other solid "obstacles". Their method is based on immersed boundary techniques (Mittal and Iaccarino, 2005; Ha et al., 2014), suitable for modeling interactions between fully or partially submerged solid bodies (fixed or moving) and the surrounding fluid. The algorithms of Orzech et al. (2016) are not yet included in the publicly available version of NHWAVE (although the code does contain basic treatment of fixed obstacles); the present model, developed independently, shares many features with their approach, but due to a number of assumptions related to the shape and the characteristics of motion of the floating objects, it is much less general, suitable for the specific configuration analyzed in this work. On the other hand, the model of Orzech et al. (2016) assumes that floating objects are rigid bodies, making it unsuitable for an analysis of ice deformation and breaking, crucial for the present study.

2.1 Definitions and assumptions

The model is two-dimensional in the xz plane. The waves are unidirectional and propagate along the x axis; the z axis is directed vertically upward, with $z = 0$ at the mean water level. The sea ice is composed of discrete elements (called grains) of cuboidal shape that are floating on the water surface and may be bonded to their neighbors with elastic bonds. The grains are rigid bodies, so that the deformation of the sea ice is accommodated only by the bonds, which may break during the simulation if stresses acting on them exceed their strength.

In the present version of the model it is assumed that the horizontal resolution of the wave model, Δx, and the sizes of the grains are adjusted, i.e., every one of the $i = 1, \ldots, N_x$ grid cells of the wave model is either ice-free or fully covered with ice (Fig. 1). Let us denote a set of indices of ice-covered cells as $\mathcal{I}_g$. All grain-related variables and equations referenced further are relevant for $i \in \mathcal{I}_g$. Similarly, as bonding is possible only between grains occupying neighboring cells, we may define a set of bond indices $\mathcal{I}_b$ so that $i \in \mathcal{I}_b$ if and only if both $i \in \mathcal{I}_g$ and $(i+1) \in \mathcal{I}_g$. (To avoid renumbering of bonds during a simulation, broken bonds are not removed from the list, but their strength is set to zero – see further.)

The grains have length $2l_i = \Delta x$, thickness h_i, and mass density ρ_i (Fig. 2). The model equations are formulated for an ice "strip" with unit width in the y direction. The position of the center of the ith grain is $[x_i, z_i]$, and the deviation of its

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orientation from the horizontal position due to rotation in the xz plane is denoted with θ_i. The motion of the grains is described by the translational velocity $[u_i, w_i]$ and the angular velocity ω_i. For each grain, the center of mass and the center of rotation are assumed identical, so that the off-diagonal elements of the mass and buoyancy matrices vanish. For rotation within the xz plane, the moment of inertia per unit grain width $I_{g,i} = \rho_i \frac{l_i h_i}{6}(h_i^2 + 4l_i^2)$. The mass per unit grain width is $m_i = 2\rho_i l_i h_i$. The assumption regarding the grains' positions relative to the wave model cells implies that $u_i \equiv 0$ and x_i is constant, which makes the model applicable only to compact sea ice in which the drift and oscillatory surge motion is insignificant. These limitations will be relaxed in the future versions.

All bonds are cuboid (Fig. 2) and their geometric properties are: thickness $h_{b,i}$ and length $l_{b,i} = \lambda(l_i + l_{i+1}) = \lambda \Delta x$, where $\lambda \in (0, 1]$ is a coefficient deciding whether the elastic deformation is distributed across the grains ($\lambda = 1$) or limited to narrow zones at the grains' boundaries ($\lambda \to 0$). As in the case of grains, it is assumed that the bonds have unit widths in the y direction. Additionally, the bonds have the following material properties: Young's modulus $E_{b,i}$, ratio of the normal to shear stiffness $\lambda_{ns,i}$; tensile strength $\sigma_{t,br,i}$; compressive strength $\sigma_{c,br,i}$, and shear strength $\tau_{br,i}$. From this set of properties, the normal and shear stiffness can be calculated: $k_{n,i} = E_{b,i}/l_{b,i}$ and $k_{t,i} = k_{n,i}/\lambda_{ns,i}$, respectively. Finally, the relevant moments of inertia (again, per unit bond width) are $I_{b,i} = \frac{1}{12}h_{b,i}^3$.

Due to the assumption of no motion along the x direction, no contact model is necessary for neighboring grains that are not bonded to each other. (If surge is taken into account, repulsive contact forces between touching grains should be implemented, e.g., the Hertzian model, as used in Herman, 2016).

In the vertical direction, the model domain is bounded by $z = -H(x)$ and $z = \eta(x,t)$, where $H(x)$ denotes the (time-independent) water depth and $\eta(x,t)$ denotes the instantaneous water surface elevation. The total instantaneous water depth is $D(x,t) = H(x) + \eta(x,t)$.

2.2 Equations and boundary conditions

2.2.1 Wave model

As already mentioned, the wave-related part of the model is based on NHWAVE. Its full description can be found in Ma et al. (2012, 2014); therefore, only a summary of the most important model features is given here. NHWAVE solves incompressible, nonhydrostatic Navier-Stokes equations in a three-dimensional domain, formulated in Cartesian horizontal coordinates and boundary-following vertical σ-coordinates, defined as:

$$\sigma = (z + H)/(H + \eta) = (z + H)/D, \tag{1}$$

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for $z \in [-H(x), \eta(x,t)]$. In the xz-space, in which the present coupled ice–wave model is formulated, the governing equations are the mass and momentum conservation equations:

$$\frac{\partial D}{\partial t} + \frac{\partial (Du)}{\partial x} + \frac{\partial \omega}{\partial \sigma} = 0, \tag{2}$$

$$\frac{\partial (Du)}{\partial t} + \frac{\partial (Du^2 + \frac{1}{2}gD^2)}{\partial x} + \frac{\partial (Du\omega)}{\partial \sigma} = gD\frac{\partial H}{\partial x} - \frac{D}{\rho}\left(\frac{\partial p}{\partial x} + \frac{\partial p}{\partial \sigma}\frac{\partial \sigma}{\partial x}\right) + DS_{\tau_x}, \tag{3}$$

$$\frac{\partial (Dw)}{\partial t} + \frac{\partial (Duw)}{\partial x} + \frac{\partial (Dw\omega)}{\partial \sigma} = -\frac{1}{\rho}\frac{\partial p}{\partial \sigma} + DS_{\tau_z}, \tag{4}$$

where g denotes acceleration due to gravity, p – the dynamic pressure, u, w are water velocity components in x and z direction, respectively, ω is the velocity component perpendicular to the σ-surfaces, and (S_{τ_x}, S_{τ_z}) are turbulent diffusion terms, assumed equal to zero in the present work. The free surface is obtained explicitly from the vertically-integrated continuity equation (2). To close the system of equations, (2)–(4) are supplemented by the Poisson equation for pressure (Ma et al., 2012; Orzech et al., 2016).

At the bottom, $z = -H$, the kinematic and free-slip boundary conditions for velocity, and the Neumann boundary condition for pressure are:

$$w = -u\frac{\partial H}{\partial x}, \tag{5}$$

$$\frac{\partial u}{\partial \sigma} = 0, \tag{6}$$

$$\frac{\partial p}{\partial \sigma} = -\rho D\frac{\mathrm{d}w}{\mathrm{d}t}. \tag{7}$$

Boundary conditions at the free surface, $z = \eta$, not covered with ice are:

$$w = \frac{\partial \eta}{\partial t} + u\frac{\partial \eta}{\partial x}, \tag{8}$$

$$\frac{\partial u}{\partial \sigma} = 0, \tag{9}$$

$$p = 0. \tag{10}$$

In the model applications presented in this work, sponge layers are applied at the left and right boundary, and waves are generated inside the model domain with a so-called internal-wavemaker technique, in which a source term is added to the model equations at the wave generation location, and the waves propagate out of this location in both directions (Ma et al., 2014).

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2.2.2 Sea ice model

The sea-ice-related part of the model can be formulated as a set of the following ordinary differential equations:

$$\frac{d\theta_i}{dt} = \omega_i, \qquad i \in \mathcal{I}_{\mathrm{g}}, \tag{11}$$

$$\frac{dz_i}{dt} = w_i, \qquad i \in \mathcal{I}_{\mathrm{g}}, \tag{12}$$

$$I_{g,i}\frac{d\omega_i}{dt} = M_{wv,i} + M_{b,i} - M_{b,i-1} + l_i(F_{t,i} - F_{t,i-1}), \qquad i \in \mathcal{I}_{\mathrm{g}}, \tag{13}$$

$$m_i\frac{dw_i}{dt} = F_{wv,i} + F_{z,i} - F_{z,i-1}, \qquad i \in \mathcal{I}_{\mathrm{g}}, \tag{14}$$

$$\frac{dM_{b,i}}{dt} = -k_{n,i}I_{b,i}(\omega_i - \omega_{i+1}), \qquad i \in \mathcal{I}_{\mathrm{b}}, \tag{15}$$

$$\frac{dF_{t,i}}{dt} = k_{t,i}h_{b,i}v_{t,i}, \qquad i \in \mathcal{I}_{\mathrm{b}}, \tag{16}$$

$$\frac{dF_{z,i}}{dt} = k_{n,i}h_{b,i}v_{z,i}, \qquad i \in \mathcal{I}_{\mathrm{b}}. \tag{17}$$

Equations (11) and (12) are definitions of the angular and translational velocities of the grains, respectively. The angular-momentum equations (13) describe changes of ω_i due to moments of forces acting on the grains. Analogously, the linear-momentum equations (14) describe changes of the vertical velocity w_i due to forces acting on the grains. The terms on the right-hand-side of (13) and (14) can be calculated from the remaining equations (15)–(17). As in all DEMs, the bonds transmit both torques and forces. Relevant in the present configuration are: bending moments $M_{b,i}$, resulting from the relative rotation (rolling) of the bonded grains in the xz plane; torques $l_iF_{t,i}$ acting on the grain boundaries due to tangential forces resulting from translational shear displacement of the grains (with velocity $v_{t,i}$); and the vertical component of the sum of normal and tangential forces, $F_{z,i}$, resulting from relative displacement of the grains (with vertical velocity $v_{z,i}$). As can be seen, in (15)–(17) linear relationships between displacement and force are assumed, which is typical for DEM models, see Herman (2016) and, for a detailed algorithm for calculating the displacements and forces in a fully 3D case, Wang (2009) and Wang and Alonso-Marroquin (2009). Finally, the first terms on the right-hand-side of (13) and (14) denote the net moment of forces and the net vertical force, respectively, from the wave motion underneath the ice. They are calculated by integrating the contribution from waves over the wetted surface of the grains. Their detailed formulation is given further in Section 2.2.3.

Note that, in a general case, although the value of $F_{t,i}$ characterizes the bond connecting two neighboring grains, the torque related to this force acting on these grains would be different if $l_i \neq l_{i+1}$. Note also that the horizontal component of the normal and tangential forces would be relevant only for horizontal displacements of the grains, which are not taken into account here.

As noted earlier, all forces and moments are formulated for a unit width of grains and bonds.

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The stresses acting on bonds are calculated according to the classical beam theory, so that:

$$\tau_i = \frac{|F_{t,i}|}{h_{b,i}}, \qquad i \in \mathcal{I}_{\mathrm{b}}, \tag{18}$$

$$\sigma_{c,i} = \frac{F_{n,i}}{h_{b,i}} + \frac{|M_{b,i}|h_{b,i}}{I_{b,i}}, \qquad i \in \mathcal{I}_{\mathrm{b}}, \tag{19}$$

$$\sigma_{t,i} = -\frac{F_{n,i}}{h_{b,i}} + \frac{|M_{b,i}|h_{b,i}}{I_{b,i}}, \qquad i \in \mathcal{I}_{\mathrm{b}}, \tag{20}$$

where $F_{n,i}$ denotes the normal force (i.e., along the bond length). The stresses are evaluated for every bond at every model time step. If at least one of the three stress components exceeds the bond strength, i.e., if $\tau_i > \tau_{br,i}$ or $\sigma_{c,i} > \sigma_{c,br,i}$ or $\sigma_{t,i} > \sigma_{t,br,i}$, the bond breaks. In bonded-particle models this is typically achieved by instantaneously setting the Young's modulus, as well as the forces and moments transmitted by this bond, to zero. This approach, based on an assumption that breaking happens infinitely fast, is well known to produce too brittle behavior, unrealistic in many materials. Some models therefore introduce a softening mechanism, ensuring that stress in broken bonds drops gradually instead of instantaneously (see, e.g., Lisjak and Grasselli, 2014). In the present model, breaking is extended in time by assuming that stresses acting on a bond that undergoes breaking drop to zero gradually over a certain time t_{br}. Numerical tests showed that $t_{br} \sim 0.1$ s is enough to remove spurious effects associated with instantaneous breaking. The influence of t_{br} on the model behavior is demonstrated in Section 3.3.

2.2.3 Sea ice–wave coupling

In the present model, the discretization of the model domain in the vertical direction is modified so that a prescribed number $N_{l,ice}$ out the total of N_l layers is used to accommodate the ice (Fig. 1). That is, the uppermost $N_{l,ice}$ layers have a constant thickness equal to $h_f/N_{l,ice}$, where h_f denotes the draft of the ice. The remaining $N_l - N_{l,ice}$ layers are divided uniformly from the bottom, $z = -H(x)$ to $z = \eta(x,t) - h_f$. Thus, the thickness of the upper model layers does not vary in time and at each time step the ice grains' boundaries coincide with boundaries of the cells of the wave model. This fact significantly simplifies the formulation of boundary conditions along the horizontal and vertical ice surfaces. At the lower surface of the ice we have:

$$w = w_i, \tag{21}$$

$$\frac{\partial u}{\partial \sigma} = 0, \tag{22}$$

$$\frac{1}{D}\frac{\partial p}{\partial \sigma} = -\rho\frac{\mathrm{d}w_i}{\mathrm{d}t}. \tag{23}$$

Analogously, at the vertical ice surfaces:

$$u = u_i, \tag{24}$$

$$\frac{\partial w}{\partial x} = 0, \tag{25}$$

$$\frac{\partial p}{\partial x} = -\rho\frac{\partial u_i}{\partial t}. \tag{26}$$

(Note that $u_i = 0$ in the present model version.) As can be seen, a free-slip condition is assumed for velocity components tangential to the ice surface.

In the immersed-boundary method, the influence of the ice on the surrounding water is taken into account by adding an additional forcing term F_{ice} to the momentum equations at the second step of the two-step second-order Runge-Kutta scheme, used in NHWAVE to numerically integrate the governing equations (Ha et al., 2014; Ma et al., 2016). By definition, $F_{ice} \neq 0$ only along the boundaries of floating/submerged objects (points marked with red crosses in Fig. 1). Details of the formulation

5 of this force can be found in Ha et al. (2014) and in references cited there. Linear interpolation of velocities close to ice boundaries is used, as recommended by Fadlun et al. (2000) and Ha et al. (2014).

To close the wave–ice interaction problem, the forcing from water to the ice has to be passed to the ice model. This forcing can be obtained by integrating the dynamic pressure p over the surface area of an submerged object. Due to the specific geometry and assumptions described in previous sections, the formulation of this forcing is relatively straightforward. As the

10 horizontal motion of the grains is not taken into account and the tilt of the grains is likely to remain small (so that $\sin\theta_i$ is close to zero and $\cos\theta_i$ close to one), contribution of pressure force and momentum acting on the vertical surfaces of end grains can be omitted. Thus, the moment $M_{wv,i}$ used in (13), and the vertical component of the wave-induced force $F_{wv,i}$ in (14) are:

$$M_{wv,i} = \int\limits_{x_i-l_i}^{x_i+l_i} p(l)\mathbf{n}_i \times \mathbf{r}_i dl, \qquad i \in \mathcal{I}_{\mathrm{g}}, \tag{27}$$

$$F_{wv,i} = \cos\theta_i \int\limits_{x_i-l_i}^{x_i+l_i} p(l) dl, \qquad i \in \mathcal{I}_{\mathrm{g}}, \tag{28}$$

15 where l denotes distance along the lower grain surface, $\mathbf{n}_i = [-\sin\theta_i, \cos\theta_i]$ is a unit vector normal to that surface, and $\mathbf{r}_i$ is a vector of length l tangential to it. Assuming linear variability of pressure between p_{i-1} and p_i, as well as between p_i and p_{i+1}, it is straightforward to evaluate the integrals in (27) and (28) to obtain:

$$M_{wv,i} = \frac{l_i^3}{3\Delta x}(p_{i+1} - p_{i-1}), \qquad i = 1, \ldots, N_g, \tag{29}$$

$$F_{wv,i} = 2l_i\left[p_i + \frac{2l_i}{8\Delta x}(p_{i+1} - 2p_i + p_{i-1})\right]\cos\theta_i, \qquad i = 1, \ldots, N_g. \tag{30}$$

20 ## 2.3 Numerical implementation

The code of the sea ice model is written as an additional module included in NHWAVE. A simplified flowchart of the coupled model is shown in Fig. 3. Due to more strict stability requirements of the sea ice part of the model, it is solved with a shorter time step $\Delta t_{\mathrm{ice}} = \gamma_t \Delta t_{\mathrm{wave}}$, with $\gamma_t < 1$. In simulations presented in this paper, $\gamma_t = 1/150$ was used. The time step of the ice model is limited by the grain size used and by mechanical ice properties, with more stiff ice (higher E_b) requiring smaller

25 Δt_{ice}.

3 Results

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3.1 Model setup

In this section, the model is applied to a series of simulations in which a single ice floe with a given thickness h_i and length L_o is moving on waves with a given open-water wavelength $L_{w,0}$. A summary of the model setting is given in Table 1. The water depth is constant $H = 10$ m, and the water column is divided into $N_l = 30$ layers. The number of "ice layers" $N_{l,ice}$ depends on the ice thickness, but is never lower than 3. The horizontal resolution of the model, i.e., the cell size of the wave model Δx and the horizontal dimensions of the grains $2l_i$, equals 0.5 m. Preliminary simulations with the standalone NHWAVE model were performed to verify whether Δx is sufficiently small and N_l sufficiently large to reproduce the shortest waves considered with satisfactory accuracy. The results showed that for the whole range of wavelengths analyzed, no significant loss of energy during propagation was observed. Thus, the attenuation present in the results described further in Sections 3.2 and 3.3 originates in the sea ice module: due to damping in the bonds they are not perfectly elastic. This undoubtedly is an undesired property of the numerical scheme used in the sea ice module; however, it has been shown in tests with artificially modified damping in bonds that it does not influence the results in terms of the floe sizes obtained – see Section 4 for a discussion.

It is also worth stressing that – as in all DEM models – the macroscopic properties of the modelled material (its strength, elastic modulus, and so on) depend not only on the microscopic properties of grains and bonds, but also on the grain size (e.g., Potyondy and Cundall, 2004; Koyama and Jing, 2007). Therefore, in simulations that are designed to reproduce the behavior and macro-properties of any particular specimen of a brittle material, grain size cannot be treated as a free model parameter, but has to be calibrated together with other parameters. In other words, the results are not insensitive to the choice of grain size. In the case of the coupled wave–ice model discussed here, they are thus not insensitive to the choice of Δx (even if this is not true for the wave model alone). As the results presented here are not calibrated to any real-world case, the influence of Δx is not further investigated. For realistic applications of the model, its parameters (Δx, λ, λ_{ns}, E_b, $\sigma_{t,br}$ and so on) can be adjusted to obtain desired macroscopic properties.

In the simulations described in Sections 3.2 and 3.3, a number of combinations of h_i, L_o and $L_{w,0}$ are considered, with the range of values 0.3–3.0 m, 5–500 m and 25–84 m, respectively. For $H = 10$ m, the range of $L_{w,0}$ corresponds to wave periods between 4.04 and 9.19 s and to kH values between 2.5 and 0.75 (where k denotes the wave number). The thickness of both grains and bonds is identical.

The simulations were performed first without ice breaking in order to analyze the spatiotemporal variability of stress in the ice, as described in Section 3.2. Subsequently, the bonds' strength was reduced to a number of values to study ice breaking pattern, analyzed in Section 3.3.

3.2 Stress variability in continuous ice

During the motion of the modelled ice floe on waves, the bonds undergo tensile, compressive and shear stress related to the relative displacement and rotation of neighboring grains. In the simulations described here, the compressive and tensile stresses had comparable amplitudes, whereas the shear stress was two–three orders of magnitude lower. All bond breaking events in

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simulations from Section 3.3 happened due to tensile failure and therefore $\sigma_{t,i}$ is analyzed here as the most relevant stress component.

Fig. 4 shows the vertical displacement of the ice and the tensile stress acting on bonds in function of time and distance from the ice edge. As can be seen in the diagrams, the amplitude of stress acting on bonds increases from zero at the ice edge

5 (where the amplitude of z_i is largest) towards a maximum value $\sigma_{t,max}$ at a certain distance from the ice edge (see the pink dot in the lower plot in Fig. 4b). Figure 5a,c shows the value of $\sigma_{t,max}$ for different combinations of ice thickness and floe lengths; the location of the stress maximum (measured relative to the ice edge) is shown in Fig. 5b,d. For a given ice thickness, the value of $\sigma_{t,max}$ increases with increasing floe size, as the floes' response changes from rigid motion (very small floes) to flexural motion (larger floes). Up from a certain floe size, equal to between one and two wavelengths, no further increase of

10 $\sigma_{t,max}$ is observed, i.e., the stress saturates to a value specific for a given ice thickness. For a given floe length, the influence of ice thickness on $\sigma_{t,max}$ is less trivial: there is a certain value of h_i for which $\sigma_{t,max}$ reaches the highest value, and for larger floes this maximum (Fig. 5a) shifts towards thicker ice. The reason for the drop of stress in very thick ice is that a lot of wave energy is reflected at the ice edge, leading to lower amplitudes within the ice itself. Moreover, thick floes are more rigid, with reduced strain and thus lower stress levels. For very small floes, $\sigma_{t,max}$ occurs in the middle of the floe and thus its location

15 is ice-thickness independent; for larger floes, location of $\sigma_{t,max}$ moves further from the ice edge with increasing ice thickness (Fig. 5d). For a given ice thickness, location of $\sigma_{t,max}$ moves away from the ice edge with increasing floe size (Fig. 5b).

Apart from the ice properties, the value and location of $\sigma_{t,max}$ are influenced by the characteristics of the incoming waves, as shown in Fig. 6 for two selected ice thicknesses and for a range of floe lengths. For a given open-water wavelength $L_{w,0}$, $\sigma_{t,max}$ increases with increasing floe length up to a certain "saturation" value (Fig. 6a,c). On the other hand, for large floes there's a

20 certain open-water wavelength producing maximum tensile stress (assuming the same incident wave amplitude). Again, this is related to both wave reflection at the ice edge and the response of the ice itself. For very short waves, strong reflection leads to lower wave amplitude within the ice; for very long waves, on the other hand, reflection and damping within the ice are weaker, but the wave steepness is small as well, leading to less intense flexural motion of the ice (see also Montiel et al., 2013). Most importantly, the location of $\sigma_{t,max}$ is almost independent of the incoming wavelength (Fig. 6b,d; note that the size of the grains,

25 and thus the effective resolution of the model, equals 0.5 m, so that the differences seen in the figures, especially in the case of $h_i = 0.5$ m, amount to just two–three grains).

For large floes, a few stress maxima with decreasing amplitude can be observed behind the main one, as shown in Fig. 7. Sufficiently far from the floe edge, the stress amplitude decreases gradually, depending on the damping rate (which depends on ice thickness and wave characteristics). At the rear side of large floes, small-amplitude ripples are observed before the stress

30 drops to zero – similar increase of the amplitude of the vertical motion of elastic plates at their downwave ends has been observed and modelled, e.g., by Kohout et al. (2007) and Yoon et al. (2014). As already mentioned, small floes ($L_o < L_{w,0}/2$) have only one stress maximum, as they undergo bending around their symmetry axis (Fig. 7b). As the floe size exceeds $L_{w,0}/2$, the symmetry gradually vanishes and the second maximum appears when L_o is close to $L_{w,0}$.

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3.3 Breaking of uniform ice by regular waves

The spatiotemporal variability of tensile stress in the ice, described above, is crucial for the evolution of ice breaking and the resulting floe-size distribution. Figure 8 illustrates how breaking of a large floe ($L_o = 500$ m) progresses from the ice edge deeper and deeper into the ice, producing small floes with lengths comparable to the distance of $\sigma_{t,\max}$ to the ice edge. An individual wave is "responsible" for a few breaking events (between one and three in the case shown in Fig. 8; up to five in other analyzed cases) and thus produces a few new ice floes. In thinner ice, the number of new cracks per wave period tends to be larger, i.e., breaking progresses into the ice faster than in stronger, thicker ice. Moreover, as can be expected, the final width of the zone of broken ice is ice-strength dependent as well and, in the cases analyzed, increases roughly linearly with decreasing bond strength (not shown). The resulting breaking pattern is not perfectly regular, but the floe-size distribution is very narrow. In the simulation presented in Fig. 8, in which the distance of $\sigma_{t,\max}$ from the ice edge equaled 8 m (yellow curve in Fig. 6b), only four floe sizes were obtained, 6.5, 7.0, 7.5 and 8.0 m, with the mode of the distribution at 7.0 m. Generally, the location of $\sigma_{t,\max}$ appears to constitute an upper bound on the size of floes detached from the edge of continuous ice, and breaking takes place not farther than a few grains in front of that limiting location.

Once the small floes break off the receding ice edge, they begin to move as almost-rigid bodies, changing their vertical position and rotating around their symmetry axis (Fig. 9). In the present model, in which the horizontal component of ice motion is not included, neighboring grains do not interact with each other if they are not bonded. Thus, a very important mechanism of wave-energy attenuation is not taken into account: floe–floe collisions. Consequently, the model produces lower attenuation rates in broken ice than in the initial continuous ice sheet (Fig. 9b). This behavior is fully consistent with the model assumptions, but not realistic. As a result, the width of the zone of broken ice is likely overestimated in the present model version. However, this drawback hardly influences the overall breaking patterns, as they are very robust to changes of the model configuration. As an example, Figure 10 shows the results of a simulation analogous to that presented in Fig. 8, but with incoming waves with a Jonswap energy spectrum (one of widely used idealized models of wave energy spectra, suitable for a wide range of wind and fetch conditions). As can be seen, even though the waves are irregular and breaking takes places in short episodes (associated with wave groups) separated by quieter periods without formation of new cracks, the final floe-size distribution is as regular as that produced by sine waves. Another important mechanism not taken into account in the present version of the model is multiple wave scattering by small ice floes detached from the ice edge. As Montiel and Squire (2017) have recently shown, scattering may lead to both destructive and constructive interference, thus contributing to local decrease or increase of the wave amplitude and strain of the ice. The net effects of these processes on the wave attenuation rates and ice breaking patterns are hard to estimate and presumably sensitive to the details of any particular configuration. (Note that the present model is capable of simulating multiple scattering, but not in the configuration used here, in which the grains of the sea ice module occupy full cells of the wave module, so that no water–ice boundary conditions are applied at the vertical walls of neighboring grains.)

Finally, it is worth noticing that the regular floe pattern described above is obtained only in simulations in which the "delayed" bond breaking mechanism, described at the end of Section 2.2.2, was activated. Figure 11 compares the results of two

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similar simulations, one with instantaneous and one with "delayed" bond breaking. If breaking is instantaneous, sudden drop to zero of all stress components at the broken location produces short-wave disturbance propagating out of this location in both directions (Fig. 11b). The excess stress related to that disturbance, combined with stress induced by the propagating wave, leads to rapid bond breaking in neighborhood of the initial breakage, producing very small ice floes, typically 2–3 grains in size (compare Fig. 11a to Fig. 8b). If, to the contrary, the drop of stress during bond breaking is extended over a time period of just less than 0.1 s, it is sufficient to suppress the amplitude of the breaking-induced disturbance to insignificant levels (Fig. 11c). Consequently, no additional breaking takes place around the initial crack.

4 Discussion and conclusions

In this paper, a coupled wave–ice model was used to analyze wave-induced stress in sea ice and the resulting patterns of sea ice breaking. The most important results can be summarized as follows: (i) breaking of a continuous ice sheet by waves produces floes of almost equal sizes, dependent on the thickness/strength of the ice, but not on the characteristics of the incoming waves; (ii) this breaking pattern results from the fact that maximum tensile stress experienced by the ice is located at a distance from the ice edge that does not depend on incoming wavelength; (iii) the incoming wave characteristics, together with ice properties, decide upon the value of the maximum stress, thus deciding whether breaking takes place or the ice remains intact; (iv) for a given floe size, there exist ice thickness and incident wave length for which the stress reaches maximum and thus breaking is most likely to occur.

As no attempt at calibrating the model against observational data was made, the numbers obtained as a result of the simulations might be unrealistic. Also, as has been already mentioned in the previous section, there are a number of mechanisms of wave-energy dissipation that are not included in the present version of the model (floe–floe collisions, ice–water friction, multiple scattering by the floes already broken off the ice edge, etc.). However, these facts do not affect the general conclusions formulated above. The present results agree with the findings of Squire et al. (1995), described in the introduction, and provide another evidence – obtained with a very different model than that of Squire and colleagues – in favor of the hypothesis that it is the ice itself (its thickness and strength) and not the incident waves that decide upon the dominating floe size in MIZ, at least during the initial stages of ice breaking (at later stages, many other factors lead to further fragmentation of ice floes, producing wide, heavy-tailed floe-size distributions typically observed in inner parts of MIZ; see, e.g., Toyota et al., 2011, 2016, and references there). In particular, it is worth stressing that in terms of the floe size resulting from breaking, the results are not sensitive to the modelled attenuation rates of wave energy (which, as already mentioned in Section 3.1, has been demonstrated in model runs with artificially modified damping in bonds connecting grains). Breaking takes place within a narrow zone of enhanced strain close to the edge of the yet unbroken ice. Again, this is consistent with the observational and modelling results of Squire (1984b); Squire et al. (1995), who found that breaking is likely only within a region where the secondary ice-coupled waves contribute to the increased vertical deflection and thus strain of the ice. As the amplitude of these waves decays very fast with the distance from the ice edge, so does the probability of breaking, independently of the attenuation coefficient of the gradually decaying propagating wave.

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If further research confirms these results, it will have important consequences for formulating parameterizations of wave–ice interactions for large-scale sea ice models, so that the information on incoming waves (especially wave steepness) is used to determine whether breaking of ice takes place, but the maximum floe size $L_{\max}$ is estimated based on ice properties themselves. (Note that, as already mentioned in the introduction, in most parameterizations $L_{\max}$ is the only variable parameter describing the FSD; the shape of the FSD for $L_o < L_{\max}$ is assumed to be a power law with a prescribed exponent. Note also that besides bending, a number of other wave-related processes may contribute to floe breaking and thus to shaping the FSD, including floe–floe collisions, overwash, rafting, etc. These processes are dependent on wave steepness, and thus amplitude; presumably, they modify the slope of the FSD, although no observational data exist that would allow to formulate this dependence as a functional relationship.)

The model presented in this paper is undergoing further development as part of a research project currently in progress. In the new version, horizontal ice motion and ice contact mechanics will be implemented (by adapting algorithms from the DESIgn model; see Herman, 2016), enabling to run the model to study floe–floe collisions and situations with significant drift and/or surge motion of ice. At later stages of the project, it is planned to extend the model to two horizontal dimensions (the NHWAVE model is three-dimensional, and significant parts of the sea ice module have already been coded for two horizontal dimensions as well). This will make it possible to analyze how the directional with of the energy spectra of incomming waves, as well as the angle between the wave propagation direction and the ice edge affect the results obtained in this study. It is also worth noticing that the code of the model can be easily extended by, e.g., replacing the free-slip boundary conditions for velocity at the wetted surface of the ice with other types of boundary conditions, or by including wind or other processes already implemented in NHWAVE. It should be stressed that NHWAVE is a very general hydrodynamic model that can be applied to a wide range of conditions: it does not make any assumptions regarding the irrotationality of the flow (as many sea ice–wave interaction models do) or the type of the wave forcing. It also accepts a number of types of boundary conditions, handles drying and flooding of grid cells, etc. All these functionalities can be used in coupled wave–ice simulations, making it a very flexible tool suitable for a wide range of conditions. A serious limitation, however, are very high computational costs of this modelling approach. This makes the model suitable for analyzing details of selected processes – like in this paper – rather than for practically-oriented applications in sea ice and wave hindcasting and forecasting.

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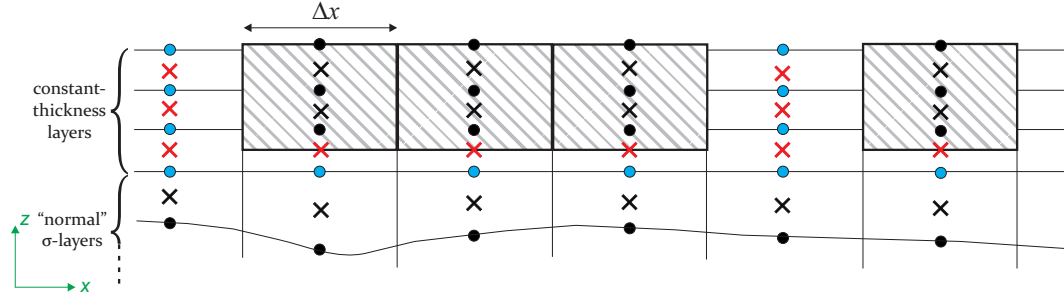
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Figure 1. Sketch of the grid organization and spatial arrangement of variables in the coupled wave–ice model, for the case of three constant-thickness uppermost layers ($N_{l,ice} = 3$) accommodating the ice 'grains' (dashed boxes). Crosses denote velocity points, dots – pressure points. Locations in which the immersed-boundary forcing is applied are shown in red, pressure points affected by the boundary – in blue (note that, in accordance with the immersed boundary method, the model equations are solved everywhere inside the model domain, independently of ice being present in a given grid cell or not). See text for more details.
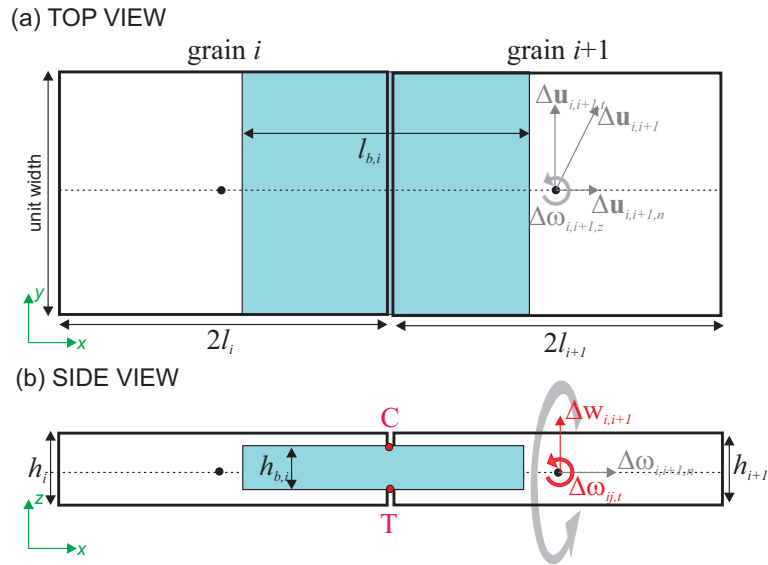
Figure 2. Top view (a) and side view (b) of two neighboring grains (white rectangles) connected with a bond (blue rectangle). Relative translational and angular velocity differences relevant for the present study are shown in red (rotation in the xz plane and vertical displacement), the remaining velocity components – in gray. Red dots with labels 'C' and 'T' in (b) mark the locations of maximum compressive and tensile stress, respectively, acting on the bond if the relative rotation is directed as shown in red.

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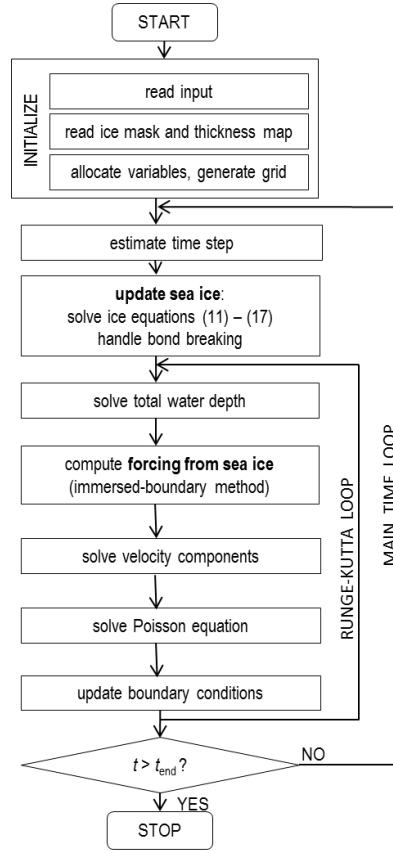
Figure 3. Simplified flowchart of the coupled wave–ice model.
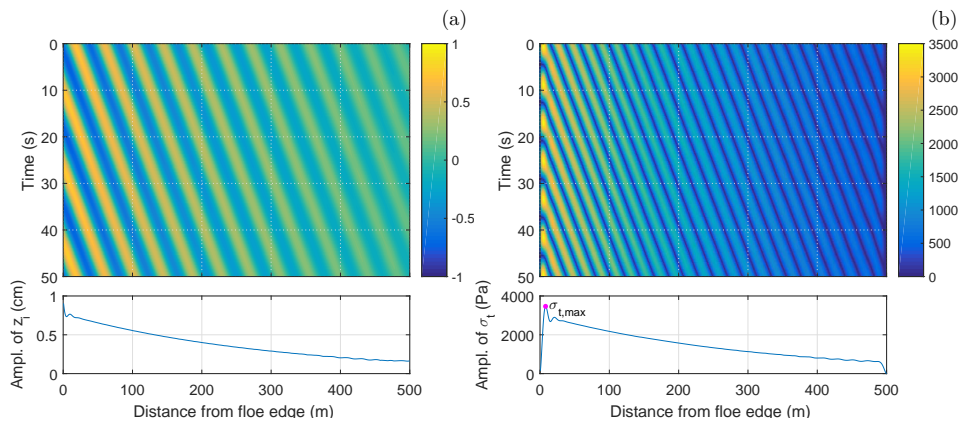

Figure 4. Simulated space–time variability of the ice vertical displacement z_i (a) and the tensile stress σ_t (b) for an ice floe with length $L_o = 500$ m; ice thickness $h_i = 0.5$ m, open-water wavelength $L_{w,0} = 42$ m. Lower diagrams show the amplitude of z_i and σ_t in function of the distance from the ice edge. Magenta dot in (b) marks $\sigma_{t,\mathrm{max}}$.
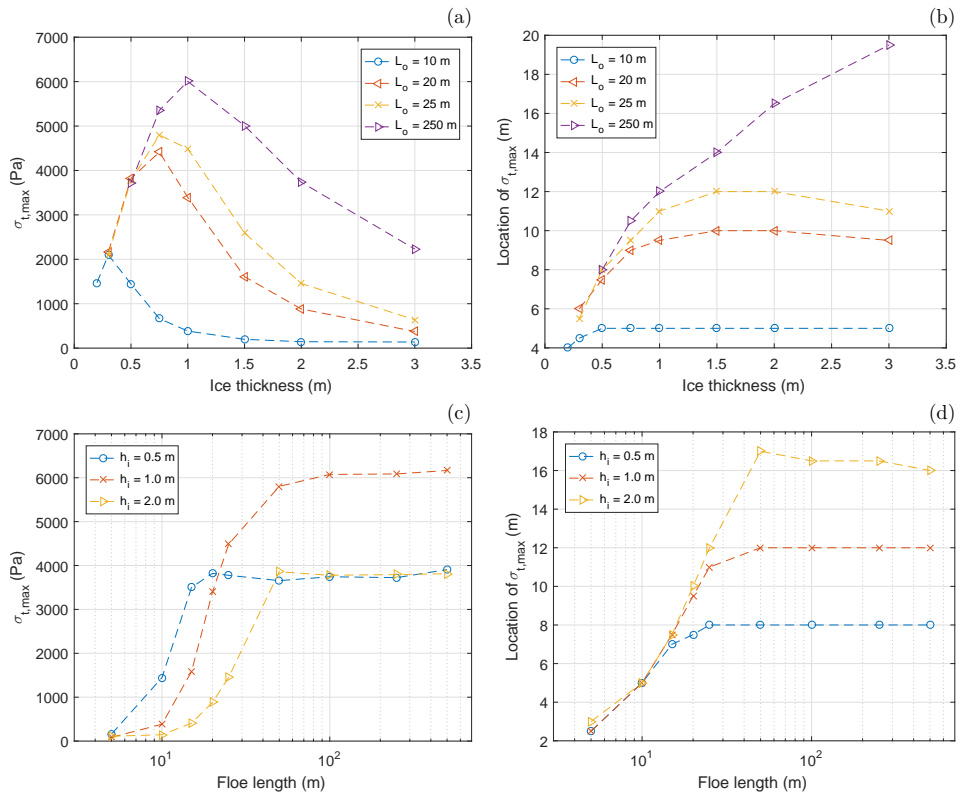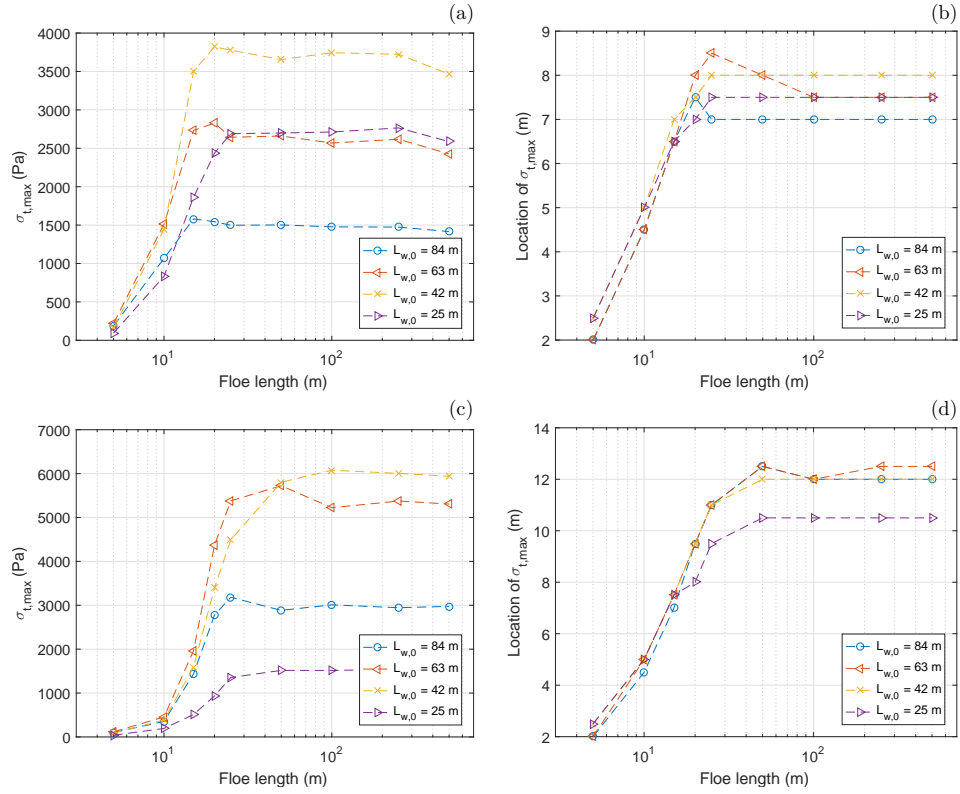
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Figure 5. Simulated maximum tensile stress $\sigma_{t,\max}$ (a,c) and location (distance from the up-wave ice edge) at which it occurs (b,d) for different ice thickness h_i and floe length L_o values; open-water wavelength $L_{w,0} = 42$ m. Note that the x-axis in (c,d) is logarithmic.

20

Figure 6. Simulated maximum tensile stress $\sigma_{t,\max}$ (a,c) and location (distance from the upwave ice edge) at which it occurs (b,d) for different floe length L_o and open-water wavelength $L_{w,0}$ values; ice thickness $h_i = 0.5$ m (a,b) and $h_i = 1.0$ m (c,d). Note that the x-axis is logarithmic.
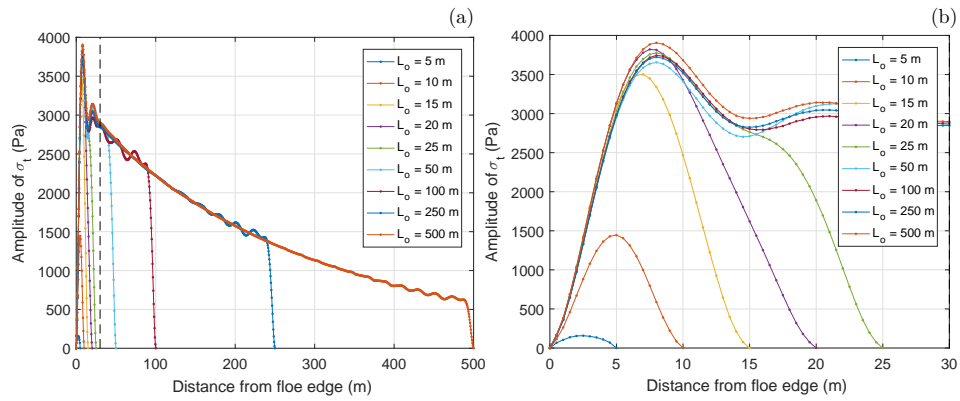

Figure 7. Amplitude of the tensile stress σ_t in function of the distance from the upwave ice edge for different floe length L_o; open-water wavelength $L_{w,0} = 42$ m, ice thickness $h_i = 0.5$ m. The plot in (b) is a close-up of the region to the left of the dashed line in (a).
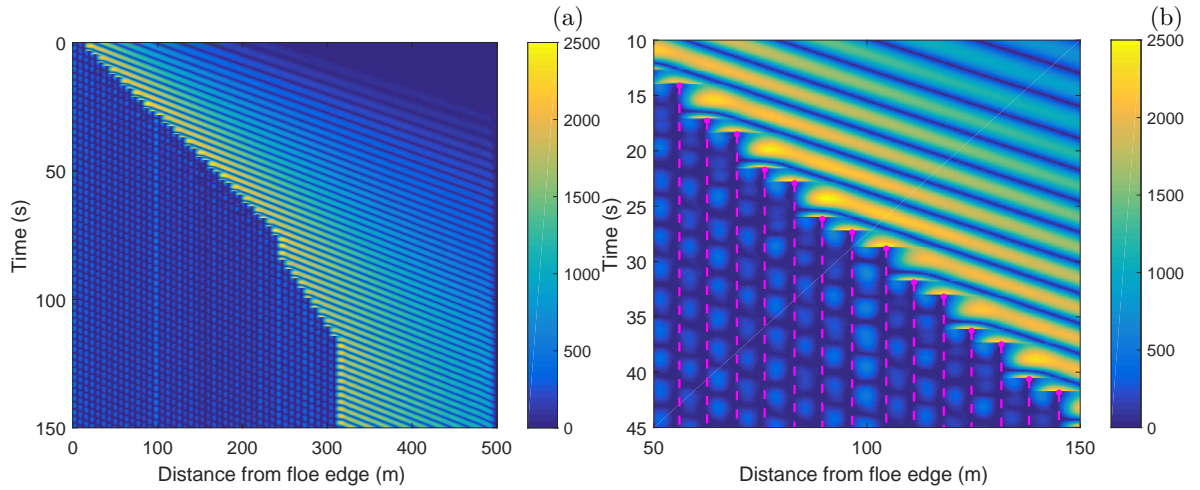
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Figure 8. Simulated space–time variability of the tensile stress σ_t for an ice floe with length $L_o = 500$ m undergoing progressive breaking; ice thickness $h_i = 0.5$ m, open-water wavelength $L_{w,0} = 42$ m, bond strength 2500 Pa. The plot in (b) is a subset of that in (a); breaking events are marked with magenta dots, broken bonds with dashed lines.
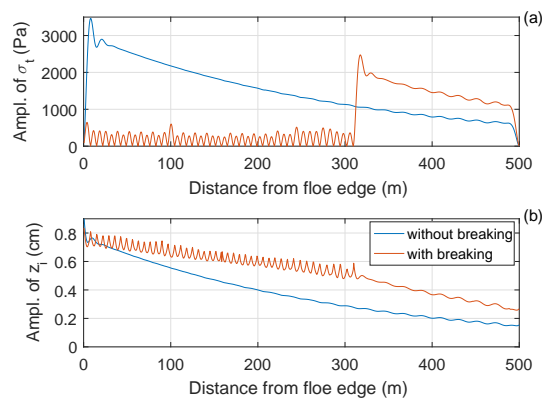

Figure 9. Amplitude of the tensile stress σ_t (a) and vertical ice displacement (b) in simulations without and with ice breaking. Floe length $L_o = 500$ m, ice thickness $h_i = 0.5$ m, open-water wavelength $L_{w,0} = 42$ m, bond strength 2500 Pa.
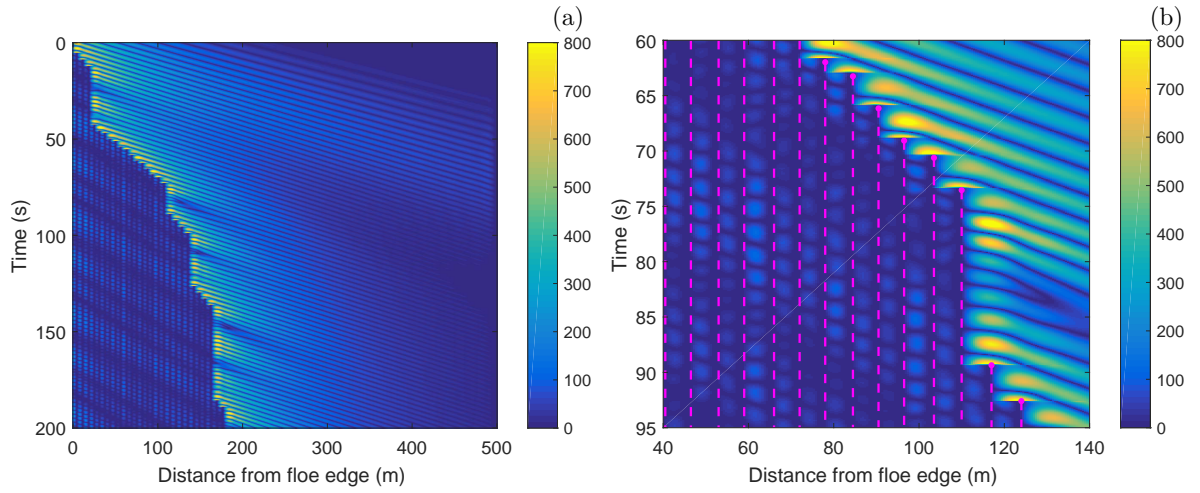
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Figure 10. As in Fig. 8, but for irregular incoming waves with Jonswap energy spectrum (wave height and peak period corresponding to those of sine waves used in simulation from Fig. 8).
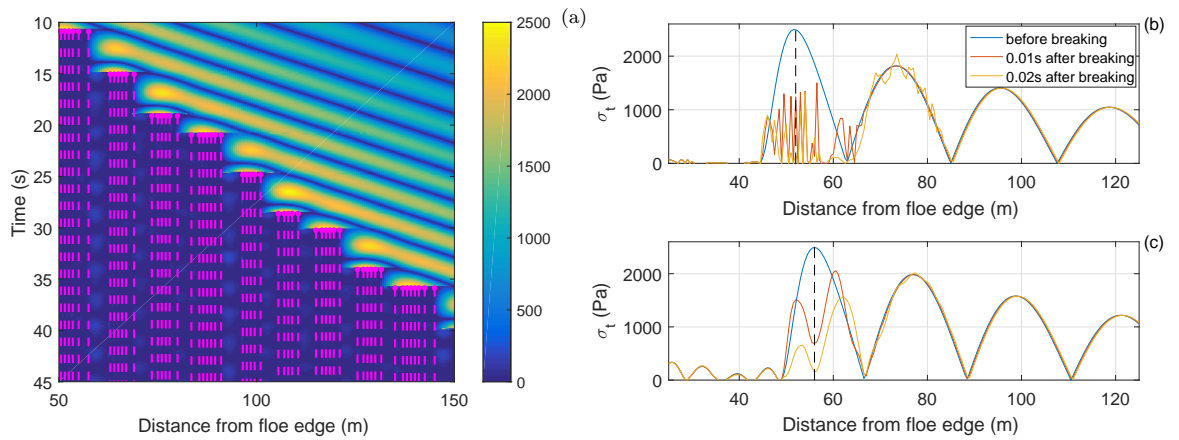

Figure 11. Comparison of the model behavior in simulations with instantaneous and "delayed" bond breaking: space–time variability of the tensile stress σ_t in a simulation analogous to that shown in Fig. 8, but with instantaneous bond breaking (a); and details of σ_t in vicinity of a selected breaking event from a simulation with instantaneous (b) and "delayed" (c) bond breaking. The curves in (b,c) show σ_t along a selected fragment of the ice floe before (blue) and shortly after (red and yellow) breaking, dashed black lines mark the location where breaking took place.

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Table 1. Model parameters used in the simulations in Section 3.

Variable	Value
Constant parameters:	
Water depth H	10 m
Basin length	1500 m
Horizontal grid size Δx	0.5 m
Number of σ-layers N_l	30
Number of "ice layers" $N_{l,ice}$	$\max\{3, 3h_i\}$
Width of sponge layers	125 m
Internal-wavemaker location	290 m
Bond length parameter λ	0.5
Normal to shear stiffness ratio λ_{ns}	1.5
Young's modulus E_b	$1.0 \cdot 10^9$ Pa
Time step ratio γ_t	150
Wave amplitude a	0.025 m
Variable parameters:	
Floe length L_o	5–500 m
Ice thickness h_i	0.3–3.0 m
Open-water wavelength $L_{w,0}$	25–84 m
Bond tensile strength $\sigma_{t,br}$	1500–3000 Pa
	(∞ in simulations without breaking)

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