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Discussions

Interactive comment on "Ice bridges and ridges in the Maxwell-EB sea ice rheology" *by* Véronique Dansereau et al.

Anonymous Referee #2

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Review: Ice bridges and ridges in the Maxwell-EB sea ice rheology

Summary & General remarks:

The paper presents simulations of ice bridges with the Maxwell-EB rheology in idealised and realistic geometries. The authors evaluate the model by analysing the simulated stress states associated with the formation and persistence of ice bridges. In different experiments, they study the effect of weaker ice in recent years on the flow storage and the sea ice export. Finally, power-law scaling of the ice thickness distribution produced by ridging ice at the margins of the bridges is presented. The paper is well written and presents interesting, new results that contribute to the current research on sea ice rheology. Nevertheless, these results generated in this idealised experiments need to be discussed in further detail to back all conclusions that authors

make. This issue lead to the "fair" grade for review criteria 2 (C). In addition, the readability of the model description could be improved. I therefore recommend a major revision on the manuscript.

Major comments:

There are many statements that require more discussion and clarification. In some places strong statements are made that not supported by the results of the model simulations. These statements need either be toned down, or sufficient evidence should be provided (which is probably not possible without additional experiments, especially when reference to VP models is made.) For instance, the impact of the simplified model equations, geometry, wind forcing and no thermodynamics is discussed too little, especially when comparing to observations made outside of a channel. The model set-up for the analysis of PDFs of ice thickness facilitates ridging with a high amount of coastal boundaries, low cohesion parameter and strong wind forcing (and a presumably wrong source term in Equation (7)), which is not further discussed. In the experiments with reduced mechanical strength a comparison to observations would be even possible: Are a collapse of ice bridges and no flow storage observed in the recent years? Especially, regarding the comparison to VP-models more caution is required. If no reference VP simulation is performed, the differences of the referred studies need to be considered, as most of the studies do not agree in resolution or region used in the experiments presented here. For example, it is not clear, if VP models at the same resolution would not lead to the same type of "ridging" behavior; as long as it is not clear, it shouldn't be claimed. The detailed overview which points need further discussion are listed below in the minor comments.

In Section 3, a sea-ice model using the Maxwell-EB rheology is outlined. However, due to the reference to individual equations in Dansereau, et al. (2015) the readability is reduced. A summary of the model equations cited from Dansereau et al. (2015) in an appendix would assist the reader. In addition, if you would include the simplifications that are made in your experiments into the model description, it is directly clear to the reader what equations are solved in your model. For example, the full momentum equations are given in Equation (3); one can easily miss the fact that many terms are not solved for (p10, l.26-27). Here the reduced equations could replace Equation (3), since this is all that is used in the manuscript.

Minor comments:

page 1,l.7: "in" -> into

page 1,l.8: "Strait" -> strait

page 1,l.9: "different dynamical behaviours" -> various dynamical behaviour

page 2, l.26: "to" -> and?

page 2, l.32: "This rheological framework typically does not account for (uniaxial or biaxial) tensile strength." Not quite true, there is uniaxial tensile strength in Hibler elliptical yield curve (see your figure 2), but there's usually no isotropic/biaxial tensile strength; adding tensile strength is another option for the VP model and has been used to improve simulations of land fast ice (Lemieux et al 2016, Olason, 2016). Could be mentioned in this context, too.

page 2, l. 30-32: Dumont also used EVP

page 4, l. 13-14: That's a speculation. It would be nicer to actually show that this can work or not work with VP models at high resolution (better than 4km grid spacing).

page 4, l.13: "…(e.g., see Fig. 1b and Sodhi,1977)" wrong citation

page 4, l.20: Section 2.2 "Ice ridges". First paragraph is relevant for the following analysis of the ice thickness distribution. The last three paragraphs can be shortened or discarded, as neither a VP-model nor an ITD is used later on.

page 5, l.17: wrong citation, I guess Hibler, 1980 is meant?

page 5, l.19: "badly" -> poorly

page 6, l. 5: "details" -> detail

page 6, l. 5: "recall" -> repeat?

page 6, l.15: This definition is unfortunate and unintuitive. 0 should be "undamaged" (zero damage) and 1 should "completely damaged" if d is called "damage". d feels more like "integrity" for the material, but it's just terminology …

page 6, l. 22: Isn't Equation (2) $\sigma_t = -2C[(\mu^2+1)^{1/2}+\mu]^{-1}$? At least in Danserau et al. (2016) Equation (7),(8) and (10) are not consistent and I guess the exponent -1 is missing in Equation (10).

page 6, l. 27-28: Unclear, if it represents refreezing of leads, how can it be independent of pure thermodynamics, please explain/rewrite/elaborate

page 8, l.9: Equation (3); Please also state the simplified version of the equation that is actually used by the model, to prevent misunderstandings.

page 8, l. 21-22: Mechanical redistribution in your formulations is represented by the divergence term, see next comment about Equation (7).

page 9, l. 1, Equation (7) That is wrong: h is defined as the mean thickness (per grid cell area), so something like a volume of ice in the cell. The ice volume does not change if $A > 1$ is reset to $A = 1$, but the (mean) thickness of the thick ice $h_{thick} = h/A$ is increased, and there is no extra contribution to S_h. (See also Schulkes (1995), JGR.) This should be corrected in the text and also in the model, if the model actually implements this extra (spurious) tendency in Equation (4).

page 9, l. 4-5, Equation (8) and (9): Just for clarity, a dependence on the thickness as in Hibler (1979) is not needed, as the internal stress is used in the momentum equation?

page 9, l. 8: "widely" -> is widely

page 9, l. 22: "(Kwok et al., 2010))" -> (Kwok et al., 2010) page 10, l.11: “northerly, wind stress” -> not sure about this: northerly winds, but the stress is acting towards the south, should be made clearer I think.

page 10, l.24: “transport of the cohesion, C” there are no sources and sinks of cohesion? How realistic is that? Please comment and elaborate the cohesion equation in Dansereau et al. (2016), this is not discussed.

page 10, l.32: What is the reference for the Young’s modulus? Same as for the Poisson’s ratio?

page 11, l.5-8: The physical role of healing is unclear and needs to be explained better. It is clearly connected to the thermodynamics (in contrast to earlier statements in the manuscript) . . . Please elaborate . . .

page 12, l.7: “location of ice bridges is not prescribed” only through the random field of cohesion (the spatial pattern should be shown somewhere). I would like to see simulations with uniform cohesion; the model geometry should be irregular enough to make the model develop ice bridges, etc.

page 12, l.16 (and elsewhere): “(see (Dansereau et al., 2016))” -> (see Dansereau et al., 2016).

page 12, l.18: For the claims made in the introduction and background sections, VP simulations at this resolution are absolutely required (have not been done to my knowledge). Please tone done the statements in the appropriate places.

page 13, l.1-2: please state the range of Δx

page 13, l.16-17: “(see Dansereau et al. (2016))” -> (see Dansereau et al., 2016)

page 13, l.19-20: I think this statement requires, that you have tried a fully implicit scheme and compared the results. Have you? If not, this statement is no really supported by anything and should be changed.

page 14, l.8-9: Please say, how much the “drift velocity on the order of that associated

with strictly elastic deformations within an undamaged ice cover.” really is (in m/s or cm/s or whatever) so that others can compare.

page 14, l.11: “relatively undamaged” -> rephrase to “stagnant ice with low damage” or similar

page 14, l. 20-21: “the width of the distribution of C impacts the rate of propagation of the damage, with the propagation being more progressive for a larger distribution.” Since the cohesion appears to be an important parameter, it would be useful to add more information about the choice of C, i.e. the actual distribution of C that is generated (page 11, ll.10) in case the reader would like to reproduce the results.

page 15, l.2: “differs” -> differ

page 15, l.23: “(see Fig. 4b and 4c, panel 3)” Should be Fig. 5b and 5c. . .

page 15, l.23-26: “This is an important point, as standard viscous-plastic sea ice models do not account for pure uniaxial or biaxial tensile strength and hence would not be able to reproduce the formation of a stable ice arch with self-obstruction to flow under the conditions simulated here.” I don’t agree: (1) From the figure, the location of the arch is not visible if you mean it is defined by the location of black elements. (2) the details of the yield curve (Figure 2) should not matter, one can tune the elliptic yield curve to resemble the Mohr-Coulomb and tensile failure criteria (see Figure 1 in Lemieux et al, 2016). (3) even without isotropic/biaxial tensile strength, Dumont could simulate arches with VP rheology, so do Losch and Danilov (2012) in similar idealized simulations, even with “a standard VP model” for order 1000 days. (4) why do VP models not account for pure uniaxial tensile strength? I think that this statement needs to change.

page 16, l.24: In this comparison (Figure 8), one might ask why the specific failure curves where chosen differently for the model, when there are estimates for the parameters available ($\sigma_c = 250$, and $\mu = 0.9$). Should be discussed somewhere.

page 17, l.3: “later” -> more recent ?

page 17, l.16: “According with”-> In line with

page 17, l.21: "“differentiated” does not sound right, rephrase if necessary

page 17, l.29-31: “However, in all of the weaker ice cover scenarios (2002-2008 period and/or 30 summer), none of the ice arches formed near the exit of Kane Basin nor secondary arches formed elsewhere sustain the applied wind forcing and all ice bridges eventually collapse.” Is there a similar behaviour in observations in this period? Please add a comment.

page 18, l.7: “widely different dynamical behaviours” -> a wide range of dynamical behaviour

page 18, l.7-9: The big question remains: how do you determine the appropriate cohesion? It appears to be vital parameter, similar to P* in Hibler’s VP model.

page 19, l.5-6: “A Lagrangian model would perhaps be more suitable to simulate the edge of the detached ice”; or a better advection scheme with less numerical diffusion (i.e. higher order basis functions in your finite element method)

page 19, l.13-14: “Nevertheless, at all times the simulated probability density function is strongly asymmetric, consistent with thickness distributions estimated for sea ice with little history of melting (e.g., Haas, 2009).” Please discuss in how far this special experimental geometry with many coastlines and the low C_{min} is suitable to compare to observations made for open ocean Arctic sea ice as described in Haas (2009).

page 19, l.18: This term (7) is not correct and should not be used. See e.g. Schulkes (1995), JGR, for correct equations and a nice explanation of ridging in general.

page 19, l. 23: “Fig. 11b” -> Fig. 10b 10b

page 20, l. 2-3: “In coupled thermodynamic and dynamic models, a high density of leads is expected to impact the simulated heat fluxes between the atmosphere, the ice and the ocean (Smith et al., 1990).” This is not really a conclusion, but part of a

discussion.

page 20, l. 11-13: “the presence of land fast ice along. . .” This has hardly been discussed and comes as a surprise. Needs more attention in Section 5 if you want to keep this conclusion

page 20, l. 24: “a process that is known to be underestimated in VP models using a two-level scheme” This is new to me. At correspondingly high resolution I would expect a VP model to behave in a similar manner, see also Losch and Danilov (2012), Fig6. which shows very similar ice thickness distribution in a similar channel experiment.

page 20, l.26-28: See above, I don’t think, that you can say this, because you’d have to show that the same model configuration with a VP model would not have your thickness distribution. I am pretty sure that you would get a similar result.

page 21, l. 14: “later” -> recent

page 21, l. 33: “Haas, C.: Dynamics Versus Thermodynamics: The Sea Ice Thickness Distribution, p. 638, Wiley-Blackwell, 2009.” Please correct citation as book chapter in Sea Ice (eds D. N. Thomas and G. S. Dieckmann)

page 22, l.10: “III, W. D. H.: Modeling a Variable Thickness Sea Ice Cover,. . .” -> wrong name

page 25, l.27: “Weiss, J. and Dansereau, V.: Linking scales in sea ice mechanics, Philosophical Transactions A, pp. –, doi:10.1098/XXXX, 2016.” Is this a submitted manuscript? If so it is not properly cited.

page 26, Figure 5: What is C_{min} in this simulations? Did you consider to show a sea ice concentration plot for the idealized experiments as well? That would help to see the arches directly.

page 29, Figure 8: An indication of the probability of single stress states using a colormap or transparency would be helpful, to get an impression how frequently biaxial tensile states (and all other stress states) occur.

page 31, Figure 10: Why are the PDFs for Δx = 4km and 8km given at t=5days, whereas the other results are shown for t=3days?

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