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Interactive comment on "Elastic-viscoplastic characterization of S2 columnar freshwater ice" *by* Iman E. Gharamti et al.

Anonymous Referee #2

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This article reports the experimental results from load controlled mode I fracture tests on columnar freshwater S2 ice at -2 C. To characterize the load-displacement behavior observed in the experiments, the authors use Schapery's constitutive model and establish its ability to describe the crack mouth opening displacements under creep and cyclic-recovery load sequences. The novelty/merit of this work relies on the uniqueness of the experiments, which are conducted on floating 3 m x 6 m rectangular plates in the Ice Tank at Aalto University. To my knowledge, very few laboratories in the world have the capability to conduct such experiments and so this type of experimental data is hardly available in the literature. From this perspective, I think the article is worthy of publication, however, I think the paper needs revision before it can be published. Below I provide my detailed comments on the manuscript.

Introduction

Page 1, line 14 – The eventual goal of these experiments seems to be to better describe or model ice loads during sea ice floe interactions with structures in the Arctic. As the authors mention the deformation modes are quite complex during ice-structure interaction, but there is no literature cited on this work. For example, Claude Daley at MUN has done experimental and modeling work on this using ice indentation experiments on structures, which seems to be the most relevant mechanism for transfer of ice loads onto structures. One would rarely imagine a floating ice flow to be subjected to mode I tension. Please add more description to the introduction about what experimental data is available, what motivated the current experimental study and why mode I fracture experiments are relevant in the context of ice-structure interaction.

Page 2, line 26 – Why has it become increasingly important to use time-dependent constitutive modeling. When was it less important? Perhaps, the authors are referring to recent drastic changes in the Arctic sea ice. The sentence here is rather vague.

Page 2, line 30 – While it is true that ice sheet and glacier modelers use viscous creep law, the terms long term and short term are vaguely defined. As my research has found, sometime a few hours is all that takes for viscous behavior to dominate, which is not really that long term. Please explain clearly that short time scales you mean are seconds or minutes or hours

Page 3, lines 65 to 70 – The study's aims are noted here. However, there is an important discussion missing here about viscoelastic fracture mechanics. The concept of fracture toughness or critical stress intensity factor is only well defined for linear elastic solids or elasto-plastic solids with small scale yielding. The authors should state and explain the definitions. Of the apparent fracture toughness K_Q and the loading rate Kˆdot, and why they are relevant quantify to ice mechanical behavior. What are the specific assumptions made about the ice viscoelastic behavior. Refer to any experiments and modeling studies in the literature that establish the theory of fracture in time-dependent materials.

Creep-recovery fracture experiments

Page 3, lines 75 to 85 – The scale of these experiments is truly impressive, however, referring to my previous why is mode I fracture relevant for ice-structure interaction. Aren't sea ice floes breaking up due to compression and plate buckling processes. Please explain the motivation for these experiments and how it can be used in large-scale modeling of ice-floe structure interaction. For example, will this study provide necessary parameters for discrete element modeling of sea ice-structure or ice-ship interaction.

Page 3, line 85 – The top surface temperature is noted as -2 C, but in Figure 2a the temperature below the surface is around -0.3 C. I am confused, please explain.

Page 3, line 86 – Please provide some more description of the experimental setup, ice growth etc as we still do not have access to your paper in press. Why does the grain size increase with depth? Also, how realistic is this for sea ice as opposed to stagnate lake ice with no waves.

Page 3, line 90 – The experiments report the load values and peak loads. However, it would be useful from a modeling perspective to get crack initiation stress. Is it possible that this sort of information can be extracted and reported from experiments. This will make the paper's results useful to those modeling sea ice-structure interaction.

Page 4, line 95 – How do the applied load rates and load levels related to real ice floes. A bit more justification is needed to establish the rationale for testing.

Page 4, line 103 – I am failing to understand the purpose of creep loadings. If the creep loads were kept small so that no damage nucleates and with recovery periods, there should not affect. In fact, this is what is observed with the results.

Page 4, line 108 – Once again how do these cyclic load levels and loading rates related to the physical setting. Are these in any way representative of the ocean wave loads

on sea ice floes?

Nonlinear time-dependent modeling

Page 5, Equation 2 – Replacing the stress and strain with load and displacement is valid only for linear behavior. Has Schapery's model used with load and displacement before in any literature?

Page 7, line 195 – What is the purpose of the modeling and parameter estimation. I ask this because I work in ice fracture modeling and cannot really see how these experiments can improve the fracture models.

Results and discussion

Page 7, line 204 – How is the weight function approach applied? Numerical evaluation of integrals with weight function approach can lead to inconsistencies. Why not use the displacement correlation method directly using COD and CMOD and NCOD?

Page 8, line 213 – Figure 5a needs more explanation. In viscoelastic materials, the peak load increases with loading rate. Please define precisely what Kˆdot is and why the peak load decreases as you increase Kˆdot. Also, defined what you mean by failure load. Is it the same as peak load? If so, then just use one terminology consistently.

Page 8, line 228 – What are the differences in the post-peak load curves that should be identified. Is it the oscillatory nature of load displacement curves in cyclic sequences? A better explanation would be useful.

Page 8, line 235 – The authors state "It is clear from Figs. 7b and 8b . . ." How is it clear? The writing style is a bit confusing.

Page 9, line 243 – I only know of the Maxwell model and the generalized Maxwell model. What is a simple Maxwell model?

Page 9, lines 247 to 259 – This whole paragraph should be written as a separate discussion section. Based on my recollection the experiments of Sinha and Cole involved compression loads and not tension loads, and there were not really on pre-cracks ice slabs. This lead to the question on why delayed elastic effect was not there? However, it is not clear why this is even an important question in the context of ice-structure interaction.

Page 267 – The statement "When the specimen dimensions are several meters, apparently viscoelasticity is not an important deformation component" is poorly explained. Also, what is the consequence of this finding? Is the author suggestion that one can just use elastic model for sea ice-interaction? Is there any relevance of these results for floating ice shelves, which are much larger than ice floes?

Conclusion

Overall, I am not clear on what the broader purpose of the paper is? Why did the author's select the specimen size and loading rates they used. Why specifically test creep/cyclic recovery? How is this work relevant to the motivation mentioned in the first paragraph of the introduction – interaction of ice floes with structure. How to use the data and findings of this paper in any future modeling analysis. A comprehensive revision of this article is needed and I recommend including a discussion section to address the implications of this research.

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