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Interactive comment on "Strain response and energy dissipation of floating saline ice under cyclic compressive stress" *by* Mingdong Wei et al.

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Summary

This manuscript presents results from lab measurements and numerical models of the strain experience saline ice undergoing periodic compressive stress. The experimental set up is novel in that allows the ice sample to be immersed in water while stress is exerted by a electrohydraulic cylinder. This allows a vertical temperature profile to be maintained through the sample, which is more representative of sea ice conditions found outside the laboratory. The authors find measurable differences in the cumulative strain response between "wet" and "dry" experiments for all frequencies of periodic loading, which particularly significant differences at low frequencies. At a loading pe-

riod of 1000s, the "dry" ice samples showed only 24% of the energy density dissipation as the "wet" samples. Using a dislocationbased model initially developed by co-author Cole and others, the authors are able to qualitatively and quantitatively reproduce the experimental results by assuming a significantly lower elastic modulus (E0), a higher dislocation density (ðİIJŇ), and a higher dislocation relaxation strength (ðİŻ£ðİŘůd) for "wet" ice than "dry" ice.

Overall, the manuscript is well written the figures are clear and well labeled. The measured difference in strain response between "dry" and "wet" experiments suggests the non-isothermal temperature profile of immersed ice under a cold atmosphere has a significant effect on mechanical behavior, which should be considered in future experiments. However, I feel that some additional explanation is required regarding the methods used to determine the values of E0, ðİIJŇ and ðİŻ£ðİŘůd. I believe the manuscript would also benefit from a deeper discussion of the temperature dependence of these and other parameters used in the model. My only other significant comment concerns the usage of the term "floating" in the manuscript. These comments are described in detail below, together with a list of minor comments for specific lines of text. In sum, I believe these amount to more than just minor revisions, but I feel they should all be quite straightforward to address.

Re: We sincerely thank the reviewer for the encouragement on our work. The comments are constructive and insightful. We have modified our manuscript according to them.

Major Comments

1. Unclear derivation of model parameters

The should provide the reader with more information about the empirical method used to determine the values for E0, ðİIJŇ and ðİŻ£ðİŘůd listed in Table 4. Line 300 mentions a "trial and error" method, but it is not clear if applies just to E0 or other model parameters as well. Also, the text states on lines 295-296 that values for ðİŻ£ðİŘůgb were determined empirically, but these are not listed in Table 4 or mentioned elsewhere in the text. The authors should describe in detail the method used to determine the values for each parameter and provide an assessment of the sensitivity of the model to each parameter.

Re: We thank the reviewer for the constructive comment. More information (as listed below) about the empirical method used to determine the values for E0, dislocation density and grain boundary relaxation strength, and an assessment of the sensitivity of the model to each parameter, has been added in the revised manuscript (as listed below). Please note that dislocation relaxation strength is dependent on dislocation density (Eq. (8)); once dislocation density is determined, dislocation relaxation strength is also known. Values for grain boundary relaxation strength could be found in Table 4.

Lines 271-279, "By making the slopes of the modeled and experimental hysteresis loops for T = 10 s comparable, E0 could be determined. This is because the behavior of the specimens is mainly dominated by the un-relaxed modulus E0 when the loading frequency is high (here 0.1 Hz to 1 Hz), as indicated in Figures 13 and 14. From Eqs. (9) and (10), one can find that the strain increment under one loading cycle is dependent on the dislocation density; based on this, the dislocation density was estimated by using the experimental results of T = 100 and 500 s and the dislocation relaxation strength was then determined from Eq. (8). The grain boundary relaxation strength was determined by referring to previous work (Cole 1995) because it could be reasonably assumed constant for the ice material of interest here, and its effect on inelastic behavior of ice was significantly less than the dislocation mechanism"

Lines 383-386, "It was also found that for relatively high loading frequency (for example, 1 Hz used here), the modeled strain behavior was dominated by the un-relaxed modulus E0, not sensitive to the dislocation density or the strength of grain boundary relaxation. However, for low-frequency (0.001 Hz) cyclic loading, the modeled specimen deformation was very sensitive to the dislocation density."

2. Greater discussion of temperature dependence of mechanical behavior of saline ice

The difference in the observed strain response between wet and dry samples is attributed to the higher temperature of the wet ice, while the model results indicate that the difference is due to a lower elastic modulus, E0, and higher dislocation density, ðİIJŇ. However, the connection between these parameters and the temperature of the ice is not made clear. I recommend the authors expand their discussion to give the reader further insight into the temperature dependence of these two parameters. Also, the viscous strain rate, , is the only parameter specifically identified as having a temperature dependence (equation 10) and so I was surprised not to see greater discussion of this in the text.

Re: We thank the reviewer for the insightful comment. We have expanded our discussion to give readers further insight into the temperature dependence of E0 and dislocation density. Details can be found in the revised manuscript (as shown below).

Lines 392-396, "The analysis and modeling indicated that the physical mechanisms of deformation in both the warmer, floating specimens and the colder dry specimens were essentially the same. Warmer saline ice had a smaller modulus due to its higher liquid brine volume, which necessarily decreases the volume of the solid ice matrix (thereby reducing the bulk elastic modulus) and there is a pronounced increase in the effective dislocation density with increasing temperature (Cole and Durell, 2001; Timco and Weeks, 2010; Cole, 2020)."

3. Use of the phrase "floating ice"

I have two minor concerns with the use of the term "floating ice" in the manuscript:

a) First, I wonder whether the ice sample in the wet experiments can really be considered to be floating once the compressive stress is applied. If the water level were changed during the experiment, the sample would presumably not rise or fall. So, I wonder whether "immersed" would be a more appropriate term to use.

Re: Thanks for this comment. Before the ice specimen is compressed, it floats naturally on water. From the strain response of the ice, the change of water level can be determined to be in the magnitude of only 0.001 mm. Thus, it can be approximated that the specimen is still floating.

b) Second, in the discussion and conclusions section the phrase “floating ice” sometimes appears to be used to refer more generally to real world ice outside the laboratory. Specifically, on line 373, the phrase is used almost synonymously with “full-scale”. I recommend that the authors add additional language to clarify that the “wet” lab experiments are able to replicate the temperature profile of floating ice, but not necessarily all the other ways in which the real world differs from the lab. See also specific comments below referring to lines 298 and 350.

Re: Thanks for the recommendation. Corresponding changes have been made in the revised manuscript (as shown below). More responses can also be found below the related specific comments.

Lines 355-358, “Even if the use of floating specimens could be considered to only address the temperature profiles of in-situ floating ice (with some other environmental conditions of natural ice floes ignored). . .”

Specific comments

Lines 51-52: This statement is not strictly accurate. One the air temperature rises above freezing in spring, the ice will approach an isothermal state

Re: Thanks for pointing this out. We have modified the statement in the revised manuscript.

Line 51, “Floating ice commonly has a through-thickness temperature gradient”

Line 83: It is not necessary or accurate to refer to the “bulk” salinity of seawater. The word “bulk” can be deleted.

Re: The corresponding change has been made in the revised manuscript. Thanks.

Line 106: Sea ice literature more commonly describes this microstructure as "non-oriented columnar". For readers not familiar with the designators S2, S3, etc, I recommend the authors add some brief text explaining the relevant microstructures.

Re: Thanks for pointing this out. We have improved the text to avoid using the designators S1, S2 and S3.

For example, Line 61, “non-oriented columnar saline ice specimens”

Lines 206-207: This feature of the data could be highlighted with additional annotation. Also, the total amount of strain also seems to increase with loading period, with the exception of T=10s, which seems to yield less strain than T=1s or T=5s. Can the authors comment on this?

Re: Information on this is added to the revised manuscript.

Lines 177-180, “The total amount of strain does not strictly increase with the loading period. This may be because the loading platen and the specimen did not always achieve perfect contact immediately, causing some error in the strain measured in this initial stage of loading. Once intimate contact was achieved, the measured strain became reliable.”

Line 221: Misplaced comma after “both”

Re: The sentence has been modified. Thanks.

Line 194, “both the strain increment per cycle and the area of one hysteresis loop”

Line 283: there should be a citation here for the value of Ω for unaligned S2 saline ice.

Re: Thanks for pointing this out. The corresponding change has been made in the revised manuscript. Line 256, “$\Omega = 1/\pi \approx 0.32$ for a horizontal specimen made of unaligned columnar ice (Cole, 1995)”

Line 293: For clarity, I recommend adding “(Rho)” after “dislocation density”

Re: The corresponding change has been made in the revised manuscript. Thanks.

Line 267, “the dislocation density (Rho)”

Line 298: Are the authors referring to field or lab measurements of floating ice here? Please also refer to general comment 1b above

Re: Originally, the statement referred to field measurements of floating ice. Since more detailed information on how the parameter values were determined has now been provided, some redundant descriptions, including the sentence discussed in this comment, have been deleted from the revised manuscript.

Line 316: Should “modes” be “models”?

Re: Apologies for the typo. The error has been corrected in the revised manuscript.

Line 350: A citation would be appropriate here. Additionally, it would be helpful to clarify whether the authors are refering to lab- or field-scale observations of the elastic modulus of floating ice (see also comment 1b above).

Re: Corresponding changes have been made in the revised manuscript.

Line 332, “. . .as would be expected based on full-scale observations on the effect of temperature on this property (Timco and Weeks 2010. . .”

Lines 381-383: By using the phrase “the water and the related through-thickness temperature gradient”, the authors appear to suggest that water itself (and not just the resulting change in temperature profile) exerts some influence on the elastic modulus of the ice. Further clarification of this statement is needed.

Re: Thanks for pointing this out. We have modified the manuscript to make its main idea clearer by removing unnecessary and potentially misleading descriptions. The phrase mentioned here is deleted from the manuscript.

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