**Strain response and energy dissipation of floating saline ice under cyclic compressive stress**

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***Review***

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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 period of 1000s, the "dry" ice samples showed only 24% of the energy density dissipation as the "wet" samples. Using a dislocation-based 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 ($\rho$), and a higher dislocation relaxation strength ($\delta D^{d}$) 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 $E_0$, $\rho$ and $\delta D^{d}$. 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.

## 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 $E_0$, $\rho$ and $\delta D^{d}$ listed in Table 4. Line 300 mentions a "trial and error" method, but it is not clear if applies just to $E_0$ or other model parameters as well. Also, the text states on lines 295-296 that values for $\delta D^{gb}$ 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.

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, $E_0$, and higher dislocation density, $\rho$. 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, $\dot{\varepsilon}_v$, 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.

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.
   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.

**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

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

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.

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?

Line 221: Misplaced comma after "both"

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

Line 293: For clarity, I recommend adding "($\rho$)" after "dislocation density"

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

Line 316: Should "modes" be "models"?

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).

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.