Responses to comments by reviewer of manuscript tc-2020-300 "Behavior of Saline Ice under Cyclic Flexural Loading"

We sincerely thank anonymous referee for valuable comments/suggestions on our work. The comments are constructive and insightful. We have modified our manuscript according to them. Please, see all the responses in red.

Comments from Referee # 1

General comments:

The experimental program is well described in the paper and the experiments themselves were well executed. The experiments uncovered an interesting and potentially important aspect of saline ice behavior and the authors are to be congratulated for developing this interesting line of work on the flexural behavior of ice. The graphics adequately portray the findings and the specimen images, particularly regarding the brine drainage features, are effective. The writing in general is clear and concise, with a few exceptions noted below. I believe that the paper presents valuable information and should ultimately go forward to publication after certain shortcomings are addressed as detailed in the following.

Although I applaud the experimental effort, I have serious issues with certain assumptions and related conclusions that are put forth. The most significant issue is the discounting of microcracking as a viable damage mechanism in their test material despite the observation of prolific acoustic activity of the type that is generally associated with microcracking. Instead, a vaguely described AE mechanism due to liquid brine movement is put forth with no quantitative development. Additionally, it is concluded that the mechanism for cyclic-loading-induced strengthening is the same in both FW and saline ice without any valid proof beyond the rough similarity of the slope of the strengthening effect. In my view, these are fatal flaws in the manuscript as submitted. That being said, both relate to the interpretation of the findings and not to the actual results. Thus, it should be a relatively simple matter to address these issues in a revision. I strongly urge the authors to make the necessary changes.

Other improvements that would significantly strengthen the paper are: 1) include profiles of the physical properties of the high- and low-salinity ice sheets and specimen specific salinities, 2) include information on the extreme fiber strain values at failure, and 3) include stress-strain data plots to illustrate basic constitutive behavior. The former measurements should be made as a matter of course and the constitutive information would serve the dual purpose of helping to relate the present effort to other work and the constitutive behavior would help inform the authors' understanding of potential underling mechanisms. I feel strongly that the above items are critical to a fuller understanding of the experimental results.

Mandatory changes:

1. Line 103: It would seem that the whitish features under discussion here are brine drainage features that consist of tubes that are typically filled with very fine-grained ice, rather than a collection of discrete brine inclusions as suggested in the text. Some clarification on this point is in order. These features constitute regions of weakness, however, and the observed tendency for cracks to run through these regions is in line observations in the cited literature.

We changed this sentence and added that interconnected brine pockets can be filled with very fine-grained ice as suggested.

2. Lines 114-118: Since this paragraph addresses, albeit in a qualitative manner, the experimental results, it belongs in the results or discussion section.

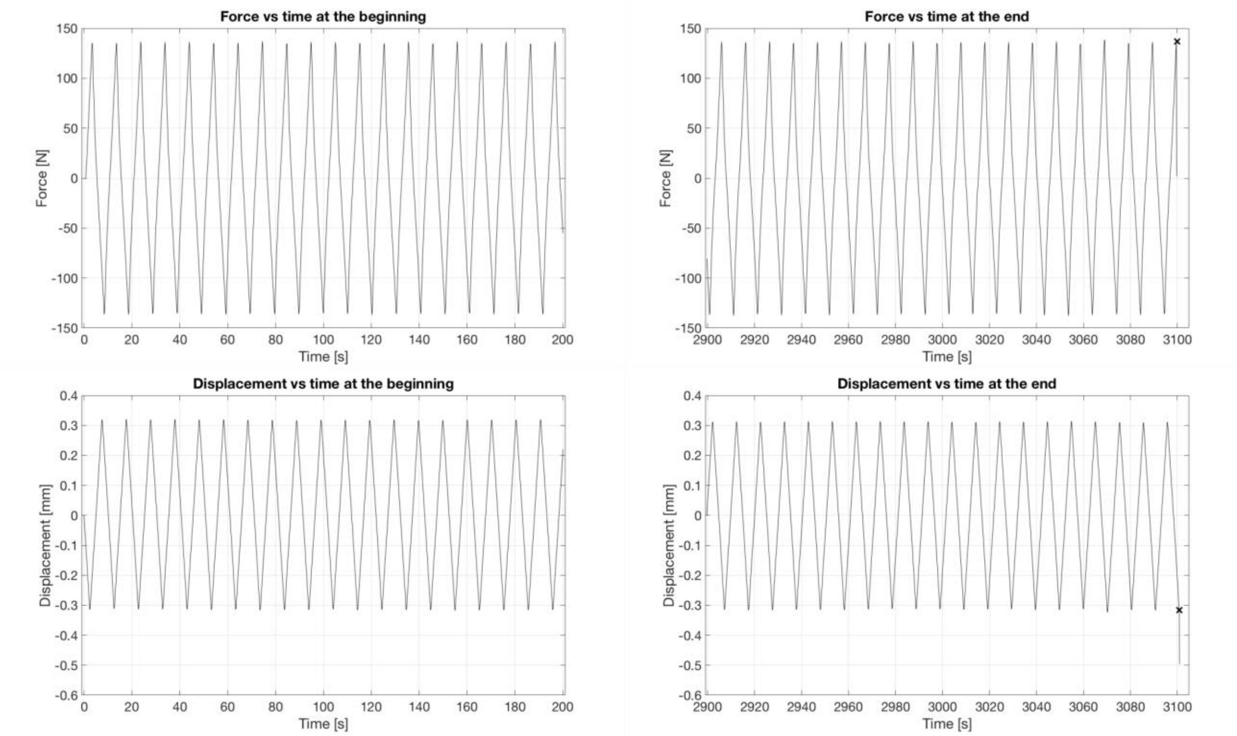
We moved these sentences to the Results section.

3. Line 130: State the range of temperature variation about your -10C set point. Was the interior or surface temperature of any of the specimens monitored?

We added ±0.5°C variation in cold room temperature. We also monitored the surface temperature of a few specimens before and during cycling with an infrared digital thermometer whose readings were also within ±0.5C.

4. Lines 150-152: I find the use of "little evidence" to be unacceptably vague in this context. The two obvious quantities to check for changes in mechanical properties are the effective modulus over a load cycle and the hysteresis loop area. Were both of these quantities examined for the early and late stages of cyclic and were both found not to vary systematically?

We changed this sentence in the following way: "*Figure 6 shows measurements of load and of displacement versus time at the beginning and near the end of cycling before specimen failure of a lower-salinity specimen (3.0±0.9 ppt). The measurements detected no softening. (According to Bažant et al. (1984) softening is a decline of stress at increasing strain or, in our case, an increase of strain during cycling at constant stress amplitude during the tests). The absence of detectable softening during cycling of the saline ice is reminiscent of the absence of softening during the cycling of freshwater ice (Iliescu et al., 2017; Murdza et al., 2020b).*". We also added a load-time and displacement-time plots, new Figure 6, at the beginning and at the end of cycling (where end of cycling in this figure corresponds to premature fatigue failure). This result was reproducible and obtained systematically**.** The area of the hysteresis loop did not change over the time of cycling.



5. Lines 163-166: This is an odd way to start the results section. I suggest that a comparison with other results be moved to the discussion section.

We agree and moved this part to the discussion section.

6. Line 177: Regarding the use of Eq.2, why use the 1967 expression for brine volume when it would be preferred to use the newer relationship presented in Cox and Weeks (1983)?

We compared values of brine volume fraction obtained through Eq2 in the manuscript and using expression provided in Cox and Weeks (1983) as suggested for a few specimens. The results obtained were similar and not significantly different. Therefore, we think that both methods can be used. The reason why we used an expression by Frankenstein and Garner (1967) is because it does not require measurements of density. Unfortunately, we did not measure density for some of our specimens; in addition, we believe that density measurements may introduce an extra error in brine fraction calculations.

7. Line 186: Do you mean "larger brine volumes" here?

By larger volumes we meant larger specimen size. We corrected the text accordingly.

8. Lines 230-237: Although the analysis focuses on failure stress levels, the matter of extreme fiber strain at failure can provide critical information and should not be ignored. The observation that FW ice can be cyclically loaded beyond its quasistatic failure strength with much greater consistency than saline ice suggests that the cyclic loading conditions can more readily generate the necessary failure strain in saline ice than in FW ice. Consequently, I believe that it would be

useful for the authors to put more thought on this matter. A few stress-strain plots of the early- and late-stage cyclic response are certainly called for here.

As noted in comment 4 above, we added load-time and displacement-time plots of the early- and late-stage (right before failure) cyclic response as suggested above (since the specimen dimensions were not changing during cycling, the load and displacement are equivalent to stress and strain). The results revealed that strain amplitude did not increase over time; moreover, during the last cycle before premature fatigue failure the strain amplitude also remained the same as at the beginning of cycling.

9.Line 253: Re the sentence beginning with “No systematic trend. . .” Drop the word “also” and correct the verb situation. Possibly say “was plotted”.

We corrected the sentence by removing the word “also” and replaced “plotted” with “was plotted”.

10.Line 269: Use “Microstructural” rather than “Experimental” in this section title.

We have used the term “Microstructural” instead of “Experimental”.

11. Lines 284-295: The conclusion that microcracks did not occur in these experiments because they were not observed in the post-test thin sectioning is, in my view, incorrect and fatally detracts from the values of this paper. In my own experience on this topic, it is clear that microcracks occur extensively in saline and sea ice but they are not readily observed under a microscope because they immediately fill up with liquid brine upon formation. This results in a loss of contrast and renders them all but invisible. In addition, given the complex nature of the microstructure in saline ice, it may be difficult to distinguish between cracks and grown-in defects, and microfracture of the ligaments between brine inclusions can occur without much visual evidence. More to the point, the AE signal characteristics in saline and sea ice are virtually the same as observed in FW ice when microcracks occur. A useful technique in this context is to shine a light through the specimen during straining and take a video of the result. What you will see are flashes of light (reflections from the forming crack faces) that immediately disappear as brine is drawn into the crack. Not coincidentally, these events correspond to concurrently monitored AE activity. This exercise will also yield information on the general location of the microcracks. In light of the above considerations, I see virtually no support for the introduction of a new and novel AE source mechanism as described in the paper.

It would be beneficial for the authors to broaden their thinking about the tensile failure process in saline vs FW ice. The thinking in the paper seems to be predicated on there being some discrete and readily observable microcracking event that causes failure – as is typically seen in FW ice. In that case, tensile failures are generally controlled by crack nucleation with only minor precursor events. Alternatively, the prolific flaw structure in saline ice allows for damage accumulation due to a great number of microcracks in regions of microstructural weakness. The brine drainage features in the test specimens constitute regions of high porosity and thus provide favorable sites for the concentration of such damage. Failure occurs when one of these sites can no longer support the applied stress and a macrocrack emerges from the damage zone &

propagates. Strain data from these experiments should shed light on this matter – especially in comparison with their FW ice data, which presumably show considerably less straining that the saline ice experiments.

In view of the reviewer's suggestions, we modified Section 3.6 Acoustic Emissions in the following way:

There are four possible sources of the noise detected. One is from microcracking. We imagine that microcracks form in regions of mechanical weakness which results in accumulation of damage that we detected via the AE method. Specifically, the brine drainage whitish features discussed above in the test specimens constitute regions of high porosity and thus provide favorable sites for the concentration of such damage. Failure may occur when one of these sites can no longer support the applied stress and a microcrack emerges from the damage zone and propagates. It is possible that newly formed microcracks are stable until a critical length is reached (Cannon et al., 1990; Schulson et al., 1991), at which point the crack growth ensues. The reason that microcracks were not observed under the optical microscope may be because they filled up with liquid brine upon formation which results in a loss of contrast. A second possible explanation for the acoustic emissions is the motion and friction of very fine particles of ice which may have been entrapped inside brine drainage features, as mentioned above. A third possibility is microcracking along grain boundaries due to grain boundary sliding (Elvin and Shyam Sunder, 1996; Goldsby and Kohlstedt, 1997; Mulmule and Dempsey, 1997; Schulson et al., 1997; Weiss and Schulson, 2000). The fourth possible explanation—consistent with the non-history dependence of the hit rate (new Figure 13) - is a kind of water-hammer effect in which brine entrapped within pockets impacts the wall, first in one direction and then another. None of these possibilities can be evaluated based upon the limits of the present observations. We refrain, therefore, from further speculation on this point.

12.Line 333-337: The similarity in the slope of the cyclic loading effect between FW and saline ice does not support the conclusion that both are the result of the same mechanism. The vast differences in the microstructures of the two ice types militates against such a simple approach. What I suspect is much more likely is that cyclic loading, when conducted at appropriate stress levels, acts to blunt the effectiveness of stress concentrations within both materials, but by dissimilar mechanisms. Some of the FW data with which I am familiar show clear evidence of a process known as dislocation punch-out, in which dislocations are produced in large numbers from grain boundary triple points and ledges during a load cycle. They act to relieve the local stress level sufficiently to prevent crack nucleation. In some cases, the dislocations thus produced can collapse back to their sources upon unloading. It would be worth examining your FW ice data for evidence of this process. On the other hand, due to the inherent weakness of the saline ice microstructure, the same sort of microstructural stress relief most likely occurs through localized damage via microcracking. In this way, two completely difference processes can ultimately have similar effects on the flexural strength.

Regarding the suggested strengthening mechanism via localized damage, we added the following sentences (lines 333-346): *"However, we should point out that there is a possibility for a different strengthening mechanism. Due to the inherent weakness of the saline ice microstructure, the microstructural stress relief may occur through localized damage via microcracking mentioned above. More research, however, is needed to examine this hypothesis."*. We agree that this mechanism (hypothesis) is possible, although we do not have any strong evidence/observations that support this argument as the correct mechanism. We still think, however, that the fact that the strengthening slopes for both fresh and saline ice are similar hints that the strengthening mechanism may be similar in the two materials.

13.Line 344: Please become acquainted with the literature on grain boundary sliding in various ice types. For polycrystalline ice (FW, saline or naturally occurring sea ice) of grain sizes over about 2 mm, the extent of grain boundary sliding is remarkably consistent. Consequently, it is difficult to invoke variations in that process in the argument being made here.

We reviewed the literature and, therefore, removed this argument from the manuscript.

14.Line 361-362: This statement may be reasonable for small-scale laboratory experiments, but I had personally witnessed incremental crack growth under cyclic loading in full-scale field experiments on sea ice. Thus, it would be prudent to qualify this statement as applicable to the scale of your experiments.

At the end of this sentence we added *"under the conditions of our experiments"*.

15.Line 380: Suggest changing "upon" to "subsequent to".

We corrected the wording as suggested.

16.Line 385: This conclusion is based on weak assumptions and cannot logically be drawn from the experimental observations. Objectively, the best one can say given the lack of definitive proof of the underlying failure mechanism in saline ice, is that the increase in flexural strength of FW and saline ice attributable to pre-failure load cycling is roughly equivalent.

We modified this conclusion as suggested.

17.With regard to figures:

Figure 11: The y-axis label should be "Cumulative hits" if that's the quantity being plotted.
Figure 12: The y-axis label needs units.

We made changes respectively.

Suggested changes:

18.Line 19: This first sentence needs a citation or two. One or two of the standard texts on fatigue & failure of materials in general should suffice.

We added a few citations.

19. Line 90: For practical considerations, it is understandable that the skeletal layer was discarded from the specimens, but the resulting material will not be representative of in-situ conditions viz-a-viz the flaw structure. A comment on the potential effects of removing this material would thus be appropriate.

We added the following clarification: *"For practical considerations, the bottom, skeletal layer of ice of about 7-10 cm was discarded as it was slushy and weak; we also believe that the skeletal layer in nature does not play a significant role in supporting the load during loading*".

20. Line 92: Regarding Figure 1 and the grain sizes reported in Table 1: It is clear from the vertical thin sections that the grain size increases significantly with depth, as is usually the case in congelation ice. Consequently, the reported grain sizes should be associated with a specific depth in the ice sheet. Moreover, since the specimens came from a range of depths in the parent ice sheets, grain size likely varied considerably across the specimen population & values should be reported.

We mentioned in the text (lines 108-109) the grain size variation along the depth of ice pucks. We also added to the text (lines 99-100) that the top layer with small grain size was not used for the specimen preparation "because it was seeded and its grain size was considerably smaller and its microstructure thus different from the rest of the ice puck".

21.Lines 121-123: In my view, storing the ice blocks on their sides is not going to inhibit brine drainage to any appreciable extent. Until the ice is cold enough to close off brine drainage pathways, brine will drain out downward or sideways and storing it in other than its growth orientation runs the risk of establishing brine inclusions with orientations that do not occur naturally. The general practice is to store specimens in their growth orientation and keep them as cold as possible. On this point, it will be important to state how long the specimens were in storage.

We added that we stored ice for time periods ranging from 1 to 10 weeks. We will consider this suggestion for the future experiments.

22.Lines 139-143: It would be helpful in this paragraph to include the peak values of the extreme fiber stress and strain associated with the range in applied stress rates. Additionally, the description indicates that the test machine was operated in either displacement control (of the loading piston) or strain control (from a specimen-mounted transducer). Please specify which method was used.

We added (lines 149-150) peak values of the extreme fiber stress and strain associated with the range in applied stress rates. Regarding displacement vs strain control, it says in the text (line 137) that constant displacement rate was used, i.e. displacement rate of an actuator was constant during cycling.

23.Lines 154-161: For the Type II tests, was the stress level increased on the fly or was the loading stopped while the settings were changed?

The loading was stopped for ~15 sec to change settings. This point is noted in the revised manuscript.

24.Lines 225-228: Note that the approximate threshold of 0.4 MPa for the cyclic loading effects agrees well with observations of the onset of significant AE activity under cyclic loading of sea ice cores presented in the Cole and Dempsey (2006) paper that is cited.

We added the following sentence: *"Interestingly, this apparent threshold is similar in magnitude to the stress that marks the onset of significant AE activity under cyclic loading of sea ice cores (Cole and Dempsey, 2006)".*