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 # 2

General Comments:

The key contribution of the paper, in addition to provide new experimental data on flexural strength of saline ice under cyclic loading, is discussing fatigue and its apparent non-classical manifestation under the cyclic loading conditions of these experiments. The key finding was that cyclic flexural stressing of a saline ice beam leads to an observed increase of flexural strength. The manuscript merits publication, but more description and explanation of the tests is required first. Specific items are identified here and in the "Line" items following. The authors present a comprehensive literature review of cyclic loading in the context of the breakup of ice sheets under ocean swell. Most of the literature on sea ice has been on weakening under cyclic loading, interpreted in terms of an S-N curve and an endurance limit (cyclic stress limiting value under which failure would not occur). Fully understanding the experiments and analysis in this paper requires familiarity with the authors' previous publications on this strengthening phenomenon in freshwater ice, including several which were just published in 2020. The paper requires a more detailed description and explanation of the tests and results. Ice, being a high temperature material with relatively large grains, I would expect time and strain are critical parameters in characterizing its behaviour. Your loading periods are from 1 to 10 s, certainly providing time for delayed-elastic and plastic strains. A representative plot of force and deflection versus time should be added to show the reader whether time and strain are significant, or can be ignored.

We added plots of force and deflection vs time for short time periods at the beginning of cycling and near the end of cycling before failure. In the text, we added the following paragraph (lines 171-176): "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)".

Figure 7 shows that for the low salinity ice and Type 1 cycling at 0.35 MPa no strengthening was observed. Provide an additional figure where results show a clear example of significant strengthening for saline ice.

The goal of this figure is to show that the number of cycles imposed (once above a certain threshold in the number of cycles) does not affect ice flexural strength, not to provide any information on strengthening. We chose the stress amplitude of 0.35MPa for this purpose for simplicity as none of the specimens failed at such a low stress amplitude. The results are in line with the results obtained earlier on freshwater ice; it makes sense to conclude that beyond a certain number of cycles the actual number of cycles does not affect flexural strength at higher stress amplitudes as well. Therefore, we do not think that an additional figure, similar to Figure 7 but with a greater cycling stress amplitude, is needed. Figure 8, on the other hand, shows the strengthening of ice upon cycling. It is clear from Figure 8 that at higher stress amplitudes ice is stronger after cycling when compared with non-cycled ice.

From Figure 8 it appears a stress amplitude of 0.7 MPa or higher is required to see a strengthening beyond 0.96 MPa simple flexural strength. This means that for low salinity ice you had to go to Type 2 cycling to get strengthening. The results in Figure 8 are hard to follow, with the results of many different tests jumbled together.

Yes, this is correct that a stress amplitude of 0.7 MPa or higher is required to detect a **significant** strengthening effect. The reason why we included freshwater results in Figure 8 is to compare the ice behavior and to point out similarities for freshwater ice and saline ice of both salinities.

Similarly, Figure 9 mixes different tests without saying how many cycles were conducted before loading to failure. Add a table which provides the test results as a function of the cycle type (Type 1 or 2, and the actual program of the Type 2 cycling for that test, number of cycles, frequency, time). This would greatly improve the paper.

We added to the figure caption of Figure 9 (new Figure 10) information on how many cycles were imposed to obtain the data shown in the figure .Regarding the second part of the comment, we added the following statement (lines 163-167) where we clarify how specimens were cycled during Type II loading: *"To cycle ice samples at stress amplitudes above 0.7 MPa, we first pre-conditioned them through step-loading Type II procedure at progressively higher stress amplitude levels, i.e. we cycled specimens for ~300 times at each of the following stress amplitudes: 0.7, 0.75, 0.8, 0.85 MPa and so on either until failure occurred or until a specific value of stress amplitude set by the operator".* Frequency and time for each specimen depends slightly on its dimensions since we keep displacement rate constant (not frequency).

The purpose of 4-point loading is to create a centre section with a constant bending moment. Did the failures occur at random locations between the two inner loading cylinders? Provide some observations on failure location.

We added the following sentence (lines 181-185): *"Failure generally occurred at random locations between the two inner loading cylinders and rarely either below or slightly outside the loading cylinders. The reason for the latter location was the presence prior to flexing of a significant concentration of whitish features which served as stress concentrators and along which the failure ultimately occurred (similar to Figure 4)"*.

More of an aside, the authors may be interested in an observation in the book by D. Masterson published in 2019, "The Story of Offshore Arctic Engineering". It mentions experience in the field of moving a lightly loaded vehicle back and forth on a floating ice road before moving a greater load along it, as a means of improving the load bearing capacity of the ice road. Your work on cyclic flexural loading seems to provide an explanation for this field experience.

We added the following sentence (lines 77-79): "The strengthening of ice is of more than scientific interest, reflected, perhaps in an interesting comment of an arctic engineer who reported that builders of ice roads never trust the ice until it had been "worked in" (Masterson, 2018)".

<u>Specific Comments</u>:

Line 21; suggest adding 'and failure' to the end of the sentence.

We added *"and failure"* as suggested.

Line 35; the sentence starting with 'For instance. . .' is not clear, are you saying that the structure is being fatigued, or the ice?

We modified this sentence by adding "or damaged", in the following way: *"For instance, during ice-structure interactions the structure itself, such as a light-house, may be weakened **or damaged** to a degree that depends on the strength of the ice".* By this sentence (and paragraph) we mean that arctic infrastructure may be susceptible to ice loads and potentially can be damaged. The degree of the damage would depend on the ice properties that are affected by cyclic loading. We did not mean to discuss fatigue of the structure in this sentence.

Line 70; you mention 'recovery', what is being recovered and does that mean increasing or decreasing strength?

We modified the sentence by indicating that strength is being recovered: "*In those experiments, it was discovered that the ice flexural strength increases upon repetitive loading, followed by the recovery of the cyclic-induced increment in strength to the original non-cycled strength upon post-cycling annealing*".

Line 78; Your experiments were performed on and analysed as beams, change 'plates' to beams.

We replaced the word "plate" with "beam" through the text.

Lines 80-90; You mention that ice plates were grown in a circular tank, how deep was the tank? Did you seed the sheet? You mention melt water salinities, were the values given from the tested beams or samples from the whole ice sheet? Density of the ice beams should also be provided, that would help distinguish between brine pockets and air filled voids.

We added that the volume of the tank is 800 L, so it is possible to estimate depth knowing that the diameter is 1 m. We also added the following **(in bold type)** information about the procedure of growing the ice in the tank: *"Briefly, solutions containing 17.5 ± 0.2 ppt and 35 ± 0.2 ppt (parts per thousand, or ‰) of the commercial product "Instant Ocean" salt mixture were prepared and then frozen unidirectionally downward over a period of about 7 days* ***by using a top-placed cold plate maintained at T = –20±0.1 °C. Before bringing the cold plate into contact with the salt-water solution, the top surface of the solution was seeded with freshwater ice grains of ~ 0.3-1 mm diameter****".* Melt-water salinities mentioned above and also listed in Table 1 are salinities of the ice specimens themselves and not the salinity of the parent ice plate. Densities of the specimens for both low salinity and high salinity ice are also provided in Table 1.

Line 121; Make clear the orientation of these blocks in the original sheet, presumably the long dimension was in plane of the plate.

We added the following information **(in bold type)** to clarify the orientation of ice blocks: *"Once the ice had been grown, it was cut into blocks of dimensions ~ 10 x 30 x 20 cm^3,* ***where the longest and the shortest dimensions are in the horizontal plane of the original grown ice puck, perpendicular to the direction of growth****".*

Line 132, Figure 5; This figure indicates that deflection at the centre-point of the beam was measured with respect to the outer pair of loading cylinders of the four-point loading apparatus, this introduces an error. You should be measuring the deflection of the beam with respect to the inner pair of loading cylinders.

We note out that in our test-setup the outer pair cylinders are attached to an immobile upper part of the apparatus (and therefore do not move). The inner-pair of cylinders are connected to the actuator which moves up and down. During cycling, we obtain measurements of the **deflection of the center-point of ice with respect to immobile outer cylinders (LVDT)**. Therefore, we disagree that we introduced an error in our measurements. According to ASTM (Standard Test Methods for Flexural Properties of Materials by Four-Point Bending, D790-17 or D6272-17 for example), "Deflectometer shall be essentially free of inertia at the specified speed of testing. Deflectometer shall be in contact with the specimen at the center of the support span, the gauge being mounted stationary relative to the specimen supports". In our experiments, the deflectometer (LVDT) was indeed attached to the immobile part and was free of inertia.

Line 151; 'softening', what do you mean? Rewrite sentence to be clearer.

We re-wrote this sentence in the following way: *"Measurements of load and of displacement versus time at the beginning and near the end of cycling revealed no evidence of softening (according to Bažant et al. (1984) softening is a decline of uniaxial stress at increasing strain or, in our case, an increase of strain during cycling at constant stress amplitude) during the tests, Figure 6, similar to the case for freshwater ice (Iliescu et al., 2017; Murdza et al., 2020c).".*

Line 168; were the test beams always in the same orientation as in the original puck? Also for the simple flexural tests, or the final loading to failure after Type 1 or 2 cycling, was the top or bottom surface the one in tension?

Yes, the test beams were prepared and tested always in the same orientation. For example, in line 132 it is stated that thickness dimension of the specimens was parallel to the long axis of the columnar grains.

Specimen thickness is ~1.6 cm, while the thickness of a grown ice puck is ~30 cm; therefore, the thickness of the ice specimens is negligible when compared with the thickness of the parent ice puck. In addition, during specimen preparation (milling) both the top and the bottom surfaces of the ice specimens were prepared in the same manner. Thus, there is no difference whether top or bottom surface of the specimen was the one in tension given the ice properties do not change significantly over the specimen thickness.

Line 186; sp. 'contain'

We corrected this typo.

Line 289; would brine not fill cracks making them difficult, if not impossible, to detect visually; also if I understand the orientation of the thin section, the chance of having a crack in it would be rare.

We agree with this comment and thus added the following sentence to Section 3.6, Acoustic Emissions: *"The reason that microcracks were not observed under the optical microscope may be because they immediately filled up with liquid brine upon formation which results in a loss of contrast".*

As stated in the text, the plane of the thin sections was parallel to the long axis of the columnar grains and parallel to the direction of the greater normal stress (or long axis of the ice beams). With this orientation, we captured the top, the middle and the bottom of the specimen which was cycled; hence, if any crack occurred, it would have initiated most likely either at the top or bottom part of the thin section since those parts correspond to regions with maximum tensile stresses during cycling. Therefore, we think that we chose the correct plane of the thin sections to search for cracks . Indeed, according to the classical fatigue behavior of materials (metals for example), defects (such as brine pockets in our case) would serve as sites for crack initiation and growth. Therefore, the method that we used would have indicated crack growth/propagation had it occurred.

Line 300; could emissions also originate from grain boundary movements?

Please note that on this point we only summarized the conclusions that other authors presented, not our thoughts here. The previous authors did not discuss whether emission can also originate from grain boundary movements. However, we agree that in general acoustic emission can originate from grain boundary movements (which we also mentioned in our analysis, lines 370-371).

Line 317; 'water hammer' is usually associated with pressure waves in a fluid in a closed system, If you are proposing brine movements in pores, some further explanation of the mechanism is needed.

According to the comments of Reviewer 1 and our further thoughts on this problem, we modified this section and explained the obtained acoustic emission results differently. Please, see below:

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.

Line 331; where is the air and brine distributed, separate pockets or both in the same pocket?

We believe that air can be located separately from brine as well as exist as a mixture of brine and air.

Line 347; explain how a brine pocket or channel makes saline ice more susceptible to premature failure. Brine pockets are very rounded, have a much larger radius and lower stress concentration than for a crack.

When we state that saline is more susceptible to premature failure, we compare it with a study on freshwater ice. Indeed, in the study on freshwater ice (Murdza and others, 2020; Iliescu and others, 2017) we did not detect by the unaided eye any defects. Saline ice, however, has a lot of defects when compared with freshwater ice, although these defects, perhaps, are not as "sharp" as cracks, as pointed out by the reviewer. However, although these defects are not as "sharp", significantly more saline specimens failed prematurely during cycling when compared with the study on freshwater ice. This observation is not surprising since it is a well-known fact that porous materials are generally weaker and small cracks grow from the pores, even if pores are spherical (for example, C.G. Sammis and M.F. Ashby (1986) "The Failure of Brittle Porous Solids Under Compressive Stress States").

Line 352; this discussion of brine pockets is very subjective, careful thin sectioning could have provided more definitive information on the grain structure.

We agree that the comparison of interconnected whitish features between ice of lower and higher salinities is subjective. However, we believe that we conducted proper thin section analyses (Figure 1 in the manuscript). Based on the obtained results, there was no significant difference in grain structure in two types of ice (higher and lower salinities). The difference was rather visual which we indicate in Figure 3. This difference was also reflected in the measurements of ice salinity.

Line 376; the tests were done on beams, don't refer to them as plates.

We have made changes through the text.

Line 380; does this conclusion apply to all ice, fresh water and saline?

According to the results we obtained, it seems that the increase in flexural strength upon cycling (beyond a certain number of cycles) scales linearly with the amplitude of the applied outer-fiber stress for both laboratory-grown freshwater ice and laboratory-grown saline ice. A similar effect was also observed in the experiments on natural lake ice (Murdza, Marchenko, Schulson, Renshaw, 2021).

Line 581, Fig. 12; why a negative value of hit rate, start the ordinate at 0.0, also state the units for hit rate on this axis.

We made these corrections.