Response to the Comments of Reviewer2

An interesting paper, congratulations with all the good field work.

Response: We thank the reviewer for his recognition of our work. The comments are detailed and constructive, based on which we have revised the manuscript carefully. Please find our responses to individual comments below.

General comments

Comment: I suggest you try to keep the result to *your own results only*. The comparison to others and discussion on why fit better in the Discussion section. For example sub-section 3.2.1 is almost only comparison with others and discussions on why. Put this content in the Discussion section.

Response: Thanks. A new section *4.1 Comparisons with previous studies* will be added in the Discussion section, and the comparisons to other studies on flexural strength, effective modulus, and compressive strength are to be moved to this section.

1. Introduction

Comment: OK, perhaps also refer to Strub-Klein and Høyland (2012).

Response: Their work will be cited.

2. In-situ sampling and laboratory experiments

2.1. In situ sampling

Comment: Ice temperature profile during field work? I suggest you move this information from section 4.2 into the *In-situ sampling* section.

Response: Corrected accordingly.

Comment: What was the air temperature during field work? Do you have a air temperature history a few weeks back?

Response:

(1) During the field tests, the air temperature varied from –2.6 to 1.8ºC with an average of –0.8±0.9ºC. We will add the information in the new version.

(2) There is a weather station at the Zhongshan station. Since the field work site was not far away the Zhongshan station, so the air temperature recorded by the weather station is used. The figure below shows the air temperature in the two months

before field work. A rise in the air temperature occurred after 15 October 2019 (UTC) from below –10ºC to above –10ºC. This information and figure will also be added.

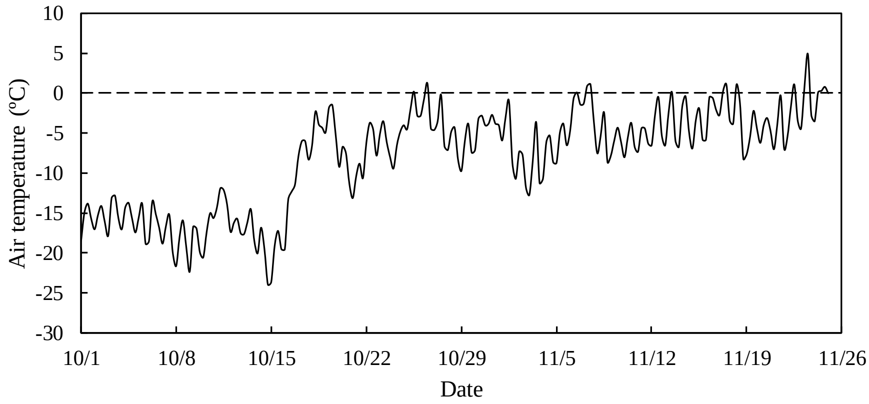

Figure The air temperature from October 1 to November 24 2019 at Zhongshan station

Comment: How long time did the field work take? Or how long was the ice exposed to the air temperatures and possibly solar radiation?

Response: Approximately 2 hours after lifting onto the deck, part of the ice block was cut and machined into samples, and the bending tests were completed. During the tests, the air temperature varied from –2.6 to 1.8ºC with an average of –0.8±0.9ºC, and it was overcast with low solar radiation. We will add the above information in the revised manuscript.

2.4. Bending tests

Comment: Elastic modulus. Could you explain how you derived these? Equation 2 only give a force and a displacement. But, there must be some kind of $\Delta F/\Delta\delta$? There are several ways to do this, one may search for the steepest part of the curve, use some kind of average etc.

Response: If the load is applied on the midspan of a simply supported beam, according to simple elastic beam theory, the midspan deflection of beam is

$$\delta = \frac{Fl^3}{4bh^3E}$$

where δ is midspan deflection, F is force at failure, E is Elastic modulus, l is span between supports, b and h are section width and height of the beam.

In the ice bending tests, with an assumption that the beam is perfectly elastic, the Eleatic modulus can be then derived using Eq. (2) in our paper if δ is known. The equation has

been suggested by IAHR Section on Ice Problems (Schwarz et al., 1981) and adopted by other reports (Karulina et al., 2019; Kermani et al., 2008). As sea ice is not turly elastic, and the derived modulus is termed effective modulus (Timco and Frederking, 2010). We will give a much clearer explanation on the equation.
Additionally, combined with the third reviewer's comment, the term of *E* will be changed to effective elastic modulus.

2.5. Compression tests

Comment: Measure of displacement. I assume this is the position of the loaded plate and not the compression of the ice sample? I don't know your machine, but usually there is some elasticity in the machine that gives a somewhat lower compression of the sample than what is given by the displacement of the loaded plate.

Response: Yes, the measured displacement is the position of the loading plate. The deformation of the test machine results in a lower compression of the ice sample than the displacement of the loaded plate. Therefore, the true stain rate of the ice sample is less than the nominal strain rate of the test (Timco and Frederking, 1984), and the latter was used in this paper. We used a universal testing machine to measure sea ice uniaxial compressive strength. The accurate stiffness of the machine is not measured. The machine is equipped with a portal frame with four columns supporting the upper beams and the working plate, all of which are made of welded steel plates. So, it is expected to be rigid enough and produce a minor effect on the compressive tests.
More detailed information on our test machine and the effect of stiffness on strain rate will be added in the revised manuscript.

Comment: Equation 3. Perhaps use *Fmax*?

Response: Corrected accordingly.

2.6. Uncertainty analysis

Comment: The numbers here could be used to give a reasonable amount of numbers in the dervied properties.

Response: This section will be deleted in the new manuscript because we found that we had missed the precondition of the error propagation that the direct measured variables used to derive indirect variables must be independent. While the force applied and sample dimensions are actually correlated.

3. Results

3.1. Crystal structure

Comment: Where is the water line in Figure 4?

Response: The accurate ice freeboard was not measured in field as our focus was paid on snow and ice thickness. The freeboard was derived from a video recording the ice side surface by comparing with the snow thickness, and it was 19 cm approximately. We will add the information in text and figure.

3.2. Flexural strength

Comment: The values of flexural strength are given with a lot of number. But, if you consider an uncertainty of 0.002 and a value of about 700 it should be sufficient to give numbers like 511 kPA, 846 kPa etc.

Response: As the respond to previous comment, the original uncertainty analysis will be deleted due to wrong understanding. While we agree the reviewer that the strength value with integral number is enough considering the accuracy of force sensor and caliper. Therefore, we will correct the values of flexural strength.

Comment: Why not give flexural strength of snow ice also here?

Response: We will give this information in the revised manuscript.

Comment: As explained above I suggest to move the content of sub-section 3.2.1 (Congelation ice) to Discussion.

Response: Corrected accordingly.

Comment: Line 192. *the region specific*. I don’t like this explanation at all. The ice does not know where it is, it only knows which physical conditions it has been exposed to. It is OK if you cannot explain why things are different, but do not blindly blame Geography!

Response: Thanks for your comment. We will delete the original explanation and rephrase it according to your comment below.

Comment: Differences to Timco and O’Brien (1994). T&B give some kind of upper limit and this means that almost any set of experiments will give lower average values. In other words it is natural that you find lower values.

Response: Discussion will be added following your suggestion in the revised manuscript.

Comment: Differences to Karulina et al. (2019). Here your results are higher and there are some obvious differences that should be discussed. Firstly, Karulina et al. (2019) tested in field, secondly they tested larger beams larger beams. It could be that their beams had more weaknesses than yours. You prepared the beams carefully in the lab and these two facts may help to explain. Also the different testing methods may have contributed.

Response: We will revise the discussion based on your comments as below.

The differences may be attributed to several facts. The first is that our tests were conducted in the laboratory while the tests in Karulina et al. (2019) were performed in field, so that our samples were able to be prepared in caution. On the other hand, the ice samples in Karulina et al. (2019) were much larger and contained more potential weaknesses than ours. Besides, the flexural strength in Karulina et al. (2019) was derived using cantilever beams, and stress concentrations at the root of beam resulted in low strength.

Comment: It is interesting and new that you investigate the flexural strength in relation to grain size and platelet spacing. Very nice.

Response: Thanks for the reviewer's approval on this work.

Comment: Figure 5. It is interesting to note that the slope was more or less equal for the columnar and mixed ice, in spite of different strengths. And that the peak deformation was equal for the snow ice and the mixed ice in spite of very different strengths. Was this coincidental?

Response: We have checked all our data and found that:

(1) The mean slopes of force varying with deformation are similar between columnar ice (512 ± 246 N·mm^{-1}) and mixed ice (625 ± 178 N·mm^{-1}), and both of them were higher than snow ice (157 ± 46 N·mm^{-1})

(2) The mean peak deflections of samples at failure are similar between snow ice (0.31 ± 0.11 mm) and mixed ice (0.41 ± 0.21 mm).

We will add the above information in the revised manuscript.

3.3. Effective modulus

Comment: As explained above you need to explain how you found the effective elastic modulus (*E*).

Response: Thanks, please see the above respond where we have made a detailed explanation.

Comment: *E* is a function of force, displacement and time. The more time a tests takes the more important becomes the viscous (or delayed viscous) deformation. The time-dependent deformation is know to be a function of salinity(brine volume). Did Karulina et al. (2019) load with the same load/displacement rate as you did? If they loaded more slowly it may explain why they found E = *f* (b*rinevolume*)?

Response:

(1) Based on the first reviewer's comment, we have calculated the strain rate of our bending tests, and stain rate ranged between 10^{-5} to 10^{-3} s^{-1}.

(2) The strain rate in Karulina et al. (2019) varied from 10^{-4} to 10^{-3} s^{-1}. So, our tests were performed at a similar or even a bit slower rate than Karulina et al. (2019). The time-dependent deformation seems not the reason that *E* is the function of brine volume in Karulina et al. (2019) while not in this paper. We will also add the above discussion in the revised manuscript.

3.4. Uniaxial compressive strength

3.4.1. Congelation ice

Comment: There is much more available published data on uni-axial strength and it is good to see that your results are more or less in line with what we think we know from before.

Response: Yes, many previously published studies have reported similar results as given in our paper. In the revised manuscript, we will cite more references and give a brief statement on these similarities.

(1) For stress-strain curves, Bonath et al. (2019) and Arakawa and Maeno (1997) are commented and cited.

(2) For the effect of sea ice porosity on compressive strength, Moslet (2007) is commented and cited.

(3) For the failure modes compressed under different directions, Gold (1997), Kuehn and Schulson (1994), and Sinha (1988) are cited.

(4) For the effect of stain rate on compressive strength and corresponding fitting

equations, Arakawa and Maeno (1997), Schulson (2001), Timco and Frederking (1990), and Høyland (2007) are cited.

(5) For the comparisons between horizontally- and vertically loaded strength, Strub-Klein and Høyland (2012) is cited.

3.4.2. Mixed and snow ice

Comment: Any comment on physical properties of the snow ice? You do not report densities or porosities. Why? If the ice was too porous to shape samples properly, please say so. Did you have any impression from visual observation? Was the ice more porous or why was it weaker?

Response:

(1) The mean density of snow-ice samples in bending test was 0.55±0.01 g·cm^{-3}, and that in compression test was 0.61±0.13 g·cm^{-3}. The snow-ice samples could be distinguished easily by their white appearance and light weight.

(2) As snow ice was not formed by congelation of sea water, the equations proposed by Cox and Weeks (1983) were not able to be used to determine porosity. From visual observation, the snow ice was much porous than the underlying congelation ice. The snow ice was compacted than new snow on top; therefore, snow ice can still be machined into regular shape.

We will add the above information in the revised manuscript.

4. Discussion

4.1. Ratios between strengths

Comment: You could also compare with Moslet (2007) and Strub-Klein and Høyland (2012), they also report vertical / horizontal uni-axial compression strengths. I don't think you can claim that you have found the unique ratios between uni-axial compression in vertical direction, the same in horizontal direction and flexural strengths. Moslet (2007) argues that this is a function of ice temperature among other things.

Response: We will compare our ratios with those reported in Moslet (2007) and Strub-Klein and Høyland (2012) in the revised manuscript. It was found that the ratio of vertically to horizontally loaded uniaxial compressive strength in our tests was independent of porosity, and the average was 3.1±0.9. The ratio of vertically loaded uniaxial compressive strength to flexural strength decreased with increasing porosity, and it reached 8.0 for sea ice with small porosity and 4.0 for sea ice with large porosity approximately. The average ratio was 7.4±1.9. Moslet (2007) reported a ratio between vertically and horizontally loaded strength of columnar ice of 1.3 for cold ice (< −10ºC) and of 4–5 for warm ice. Strub-Klein and Høyland (2012) reported low vertical-to-horizontal strength ratios of 1.4–1.8 for granular and columnar ice because the sampling ice cover had been broken and recrystallized.

4.2. Comparison between field and lab

Comment: This discussion should be linked to the comparison with Karulina et al. (2019). One important aspect I suggest you think about is cooling and then heating of the sea ice. We have tested relatively warm ice in-situ, then sampled cooled down (-15C) and stored (some weeks or some months), and finally heated again and tested. The samples that were cooled down and heated again were clearly stronger than the in-situ ice even if the temperature was the same! I think this is an important, and not understand mechanism in ice mechanics that should be studied, it may explain why SYI and Old Ice are both stronger than FYI even for comparable temperatures, and porosities.

Response: We will discuss more in the Section 4.2 based on your comment.

(1) The sea ice flexural strength of our field measurements was 718.6±47.6 kPa. Karulina et al. (2019) reported a range of sea ice flexural strength of 109–415 kPa by performing full-scale tests in the Arctic regions. As stated before, the differences in sample preparation, size effects, and test techniques could produce different strength values between their and our tests.

(2) As the reviewer commented, the thermal cycling of an ice sample would influence its mechanical behavior (Høyland, 2007). The sea ice block with warm in situ temperature was extracted and then cooled down for storage followed by heating again for final tests conducted in laboratory. While the samples that were cooled down and heated again are stronger than the in-situ ice even for comparable porosities. It is not clear why this is the case, but it is a possible reason that the strength estimated using equation derived from laboratory tests exceeds on site measured strength.

Reference

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