

The Cryosphere Discuss., author comment AC2  
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## Reply on RC2

Sebastian Skatulla et al.

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Author comment on "Physical and mechanical properties of winter first-year ice in the Antarctic marginal ice zone along the Good Hope Line" by Sebastian Skatulla et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-209-AC2>, 2022

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This study reports physical and mechanical properties of sea ice from a very few floes in the marginal ice zone. While the dataset is small, and the number of floes selected possibly too small to know if they are representative, there are two aspects that make these measurements notable. First, there are almost no prior observations of mechanical properties for Antarctic sea ice, and I believe these are the first for the MIZ. That alone makes these valuable. Second, while such few cores for the physical properties do not add much to the fairly extensive prior observations from the literature, they are unique in that so many cores were taken from a few pancake floes. For these reasons, there is enough here to be worth publishing.

There are a few important areas where the manuscript can be improved. I have two main suggestions; one on the presentation, and the second is technical.

■

The measurements are not placed in sufficient context and the **importance of the results is not discussed in much detail**. The **introduction is somewhat short** and does not provide enough background on prior work. It would also be helpful to provide **more background on prior similar mechanical measurements up front** to help highlight the lack of such measurements in the MIZ, and to provide more background explaining **how your measurements can inform sea ice mechanical models**. This may be less clear for large-scale ice rheology, but I think your measurements are more directly relevant for things like rafting and ridging, and particularly for pancake-pancake interactions, wave propagation, and fracture of pancakes. You could also **summarize/reference prior observations of sea ice properties made in the MIZ**. In addition to the ones you mention, numerous cores in the MIZ were taken on various **Jeffries cruises between 1994-2000**, and most recently **Ackley et al., 2020**. Finally, more discussion of the potential importance and impact of your results in the conclusion would help.

*A: Agreed. As the paper also reports on physical sea ice properties, the authors will elaborate more on related measurements and observations in literature in the introduction. Furthermore, it will be emphasized that data on the mechanical properties of*

Antarctic sea ice is scarce and that there is a complete lack with regards to elastic properties of Antarctic **winter** first-year sea ice. As suggested, the need and relevance of mechanical ice properties will be highlighted, in particular, to inform detailed small scale sea-ice dynamics models.

■

The interpretation of the **results with respect to the brine volume**, which will be significantly **different between in situ and when measured in the cold lab**, is suspect, and not sufficiently discussed. This point is critical, as I believe some of the results (figure 13) are then incorrect (see specific comments below). The potential impact of the results could also be better described.

*A: Direct brine volume measurements are difficult in-situ and the indirect determination based on existing empirical relations (Frankenstein and Garner 1967 or Cox and Weeks 1983) is generally hampered by brine drainage after sea ice extraction/coring as well as inaccuracies of onboard density measurements. For this reason, the relation by Frankenstein and Garner 1967 was used which does not depend on ice density but is solely based on ice temperature and bulk salinity – the former was determined in situ and the latter within the hour after collection. Whereas brine drainage for pack ice mostly effects its bottom layer, it can be expected that the high porosity of pancake ice floes leads to more significant drainage effects. These aspects and the uncertainty introduced by them for some of the results will be further elaborated on in the manuscript.*

■

Unfortunately, the **differences among cores for different locations** within the individual pancakes is not explored. I would urge the authors to explore this, both for the salinity and mechanical properties, if possible, as there have been almost no descriptions of such variations before (I believe one or two papers by Wadhams has some information on spatial variations in salinity).

*A: The authors agree that a detailed analysis of the distribution of properties within a single pancake ice floe would be very important to investigate. Unfortunately, there was a large interest from different groups part of the expedition to study a wide spectrum of pancake ice floe properties, e.g. temperature, salinity, trace metal, biomatter inclusions, elasticity, strength, crystallography etc. Accordingly, only 3 cores per floe could be allocated for each, salinity, elastic and compression strength properties. As such, it was not possible to confidently report on the actual distribution of the mechanical properties within a single pancake ice floe. Instead, as the pancake ice floes were collected at the same location, data from all pancake ice floes were combined to obtain a larger dataset for the statistical analysis.*

Specific comments:

Line 17 – I normally think of “sea ice properties” as intrinsic, and so not something climate models would predict (e.g., models don’t predict ice strength). I think you mean things like extent here.

*A: Correct, the authors were referring to predicted sea ice properties in terms of extent, concentration and thickness as affected by drift and thermodynamics. This will be clarified in the manuscript.*

Line 20-22 – sea ice mechanics is not my core area of expertise, but I think this is too strong a statement. While there is considerable debate over issues of scale in sea ice mechanical properties, the large-scale emergent properties do have some physical meaning. i.e.  $P^*$  is still related to compressive strength, and I believe Schulson (JGR, 2004) has argued that fracture patterns are similar across scales and suggests that mechanical properties may be scale independent. Rheologies do have empirical parameters, but many do have physical meaning (some, however, are numerical conveniences). You might also add a couple refs here, as there are a variety of sea ice rheologies.

*A: The scale dependency of sea ice deformation has been a field of active research until now. Differences between Arctic and Antarctic sea ice dynamics further add complexity to this. Squire 2018 summarized that meso and large-scale sea ice dynamics models are phenomenological approaches and their parameters do not represent actual physical ice properties. They are only fitted average values of a large enough statistically representative region containing varying sea ice types. Moreover, these averaged values also incorporate a whole range of mechanical phenomena in combination such as floe collision dynamics, form drag of interstitial grease ice on pancake ice floes, skin drag (see e.g. another paper by the authors on detailed small-scale modelling of pancake ice floe dynamics in the MIZ: Marquart et al. 2021).*

*The authors tend to agree with the conclusions drawn by Squire based on their own observations during the SCALE Winter Cruises to the Antarctic MIZ (2017 and 2019) where they experienced a very heterogeneous and mobile sea ice composition comprising a mixture of solid pancake ice floes and fluid-like grease ice, varying ice concentration, leads with open water etc.*

*The scale-dependency of sea ice deformation has been also discussed by Rampal et al. 2008 and Dansereau et al. 2016 where it was concluded that it cannot be exclusively described by a viscous-plastic rheology, e.g. sea ice drift on the scale of less than 10 km is only accurate to some degree and fails to reproduce sea ice deformation at finer scales.*

*Also, brittle sea ice mechanics and fracture is linked to a wide range of spatial and temporal scales and highly intermittent requiring coupling and tailored scaling laws to link these scales (see Weiss and Dansereau 2017).*

*The issue of scale-dependency sea ice models will be further elaborated on with additional literature references in the manuscript.*

*Dansereau, V.; Weiss, J.; Saramito, P.; Lattes, P. A Maxwell elasto-brittle rheology for sea ice modelling. *Cryosphere* 2016, 10, 1339–1359, doi:10.5194/tc-10-1339-2016.*

*Marquart, R.; Bogaers, A.; Skatulla, S.; Alberello, A.; Toffoli, A.; Schwarz, C. and Vichi, M., 2021. A computational fluid dynamics model for the small-scale dynamics of wave, ice floe and interstitial grease ice interaction. *Fluids*, 6(5), p.176.*

*Rampal, P.; Weiss, J.; Marsan, D.; Lindsay, R.; Stern, H. Scaling properties of sea ice deformation from buoy dispersion analysis. *J. Geophys. Res. Ocean.* 2008, 113, doi:10.1029/2007JC004143.*

*Squire, V. A. A fresh look at how ocean waves and sea ice interact, *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences* 376 (2129) (2018) 20170342.*

*Weiss, J.; Dansereau, V. Linking scales in sea ice mechanics, *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences* 375 (2086) (2017)*

20150352.

Figure 1 – This is a bit hard to read because of the color choices. At least highlight the two sites where cores were taken and reported in this paper with a different color. You might consider alongside this figure a location map that shows where this was in relation to the rest of the Weddell Sea region.

*A: An improved figure will be included which also shows the sampling stations on a large-scale map.*

Line 71-75 – Can you define what is meant by young ice here? Is it the WMO definition?

*A: With reference to WMO (code 3739), ice age ID 3 applies for the station where the pancake ice was collected (predominantly new and/or young ice with some first-year ice) and ice age ID 5 applies for the station where consolidated pack ice was collected (all thin first-year ice (30 - 70 cm thick). This clarification will be added to the manuscript.*

Lines 85-90 – only a point on style here, but listing the tools is a bit of an odd way to present this. Normally, you'd mention the specific tools when their use is mentioned in the following paragraph.

*A: Agreed. The manuscript will be changed as suggested.*

Line 121-124 – Measuring salinity for young, warm ice is challenging, because the brine drains so quickly. Normally, one would measure salinity immediately upon sampling. It is not clear how much time passed between taking the core and cutting on the band saw. In any case, you should note that some salt may have drained, so your salinities may in some case be low. This could impact density measurements as well.

*A: The processing time of core samples used to determine the bulk salinity of ice was less than an hour which will be clarified in the manuscript.*

*The authors observed substantial initial drainage at the ice bottom during pancake ice floe lifting from the ocean and consolidated pack ice core extraction, but no visible brine loss due to gravity drainage or expulsion was observed during the subsequent core processing in the cold lab aboard the ship. The latter is also in line with investigations by Notz et al. 2009 where brine expulsion effects were found to be negligible and gravity drainage requiring an ice thickness of significantly more than a core diameter to result in a supercritical Rayleigh number (the cores were kept in horizontal position during transport, storage and processing in the cold lab). In summary, the salinity measurements and subsequent density computation have been affected brine loss. This fact will be stressed in the manuscript.*

*Notz, D. and Worster, M.G., 2009. Desalination processes of sea ice revisited. Journal of Geophysical Research: Oceans, 114(C5).*

Line 125 – note it is the absolute value of the temperature that is used in (1).

*A: Will be noted in the manuscript.*

Line 137 – what is a thermal macrotome? Thin sections are normally done on a sliding microtome, so some description of the instrument would be useful here if it is different.

*A: The authors used a thermal macrotome to produce thin sections for cross-polarisation viewing which they constructed themselves. This device uses the concept of heat being passed through a nickel-chrome wire that slices ice into thin sections. It is able to cut 10*

*cm-long and 9 cm-wide core segments into slices of 1 mm or less. More details of this custom-made macrotome will be provided in the manuscript.*

Section 3.1 – It would help to compare these salinity profiles to prior observations from the MIZ. For example Eicken, 1992 is the most extensive study on salinity profiles in Antarctic sea ice, and defined several canonical shapes; how do yours compare? Also, see Tison et al. 2020, who report salinities and brine volumes for young ice in the Ross Sea, with several very high salinities reported.

*A: Agreed, a comparison will be added to the discussion section of the manuscript to provide more context.*

For bulk density, please provide more details on how the measurements were made. This is a difficult measurement to make accurately based on weighing cut pieces (and may be affected by brine drainage). You state you have some implausible values; doesn't this imply that your confidence intervals should be broader in your figures 8 and 9? i.e. the error is not just to scatter in the fit, but also due to potential inaccuracies in the measurement. They look too small for pancake ice. How do these densities compare to prior observations? Density is a quite useful property to know, and few have been reported in the Antarctic, so it would be quite useful to have a more careful estimate of your confidence in these numbers in case someone uses them.

*A: The density measurements are indeed highly problematic when done aboard a ship in the winter MIZ close to the open ocean. Also, the errors in the determination of specimen dimensions and volume from cored specimens due to flaws in terms of shape and surface composition as well as drainage play a role and lead to unavoidable inaccuracies computing the density. More details will be provided in the manuscript as to how the measurements of specimen dimensions and mass were obtained using a Ross tape measure and a hand-held scale, respectively.*

*The graphical representation of the 90<sup>th</sup> percentile confidence intervals have been computed with the help of the Seaborn package (<https://seaborn.pydata.org/>) and have been double-checked again. The relatively low pancake ice density would be plausible when considering drainage. For the consolidate pack ice density, another issue is the relatively small sample size which results in unrealistic values for the top part in particular.*

*These issues will be emphasized more, in particular that the density measurements in general have been affected by the aforementioned inaccuracies.*

Figures 6&7 – it might be more useful to show temperature, salinity, and brine volume alongside each other in each figure, as this might help the interested reader in understanding the mechanical results. This is just a presentation style, so up to you.

*A: The authors thank the reviewer for this suggestion. Indeed different combinations were tried, because of the high number of measurements and the large ranges involved. The figures for the different cores and stations are quite small. Merging all 3 properties into 1 graph led to decreased legibility. The authors therefore would prefer to maintain the current layout.*

Line 226-229 – this is misleading. First snow ice is often orbicular granular as well (which is why it is difficult to identify by morphology alone). It is superimposed ice that is clearly polygonal. Second, this is not the primary means of snow ice formation (though perhaps it is in the MIZ, where it is dynamic and snow is not deep). Snow ice is usually thought of as forming from seawater percolation up through the ice when under sufficient snow loading.

*A: It was indeed not possible from our analysis to differentiate between ice formed from frazil ice as opposed to meteoric ice and the specific ratio of both. Isotopic analyses have been delayed during the pandemic and they are planned for this year. We therefore only refer to it as granular ice textures. The authors agree with regards to the origin of snow ice formation and textural ambiguities of granular ice. This sentence will be rephrased accordingly in the revised manuscript.*

Figure 11 and 12 – what is the purpose of plotting these versus depth? It doesn't look like there is any significant relationship, and is certainly more due to brine volumes, etc, which you have not properly captured (see next point).

*A: For the small-scale modelling of the mechanical behaviour of an ice floe (e.g. inelastic floe collision), the detailed spatial distribution of mechanical properties such as elastic and strength properties is required. Therefore, their distribution not only in horizontal directions (as requested before by the reviewer) but also in the vertical direction is of interest. Furthermore, there is an interlink between the vertical textural, porosity and mechanical properties which helps to cross-reference and validate these properties. A comment will be added to the manuscript that the detailed vertical distribution of mechanical properties will aid small-scale modelling of sea ice deformation.*

Line 282-286, figure 13, 16, and elsewhere – It is not clear if you have plotted against the brine volume in situ (i.e. based on the initial core temperature and salinity) or in the lab (where the cores would have cooled somewhere close to the -10C storage temperature by the time these tests were performed). Based on the values plotted, It seems like it is the former for Figure 13, but the latter for Figure 16. This is important, because your cores were generally much warmer when sampled. If it is the former, then it is probably quite misleading. Ice with salinity of 7 ppt at -10C will have  $V_b \sim 4\%$ . This puts all your values for Young's modulus well below Langleben and Pounder's. But more importantly, it affects your interpretation of the actual properties of the ice in the ocean, because the brine volumes will be much higher there in most cases. This needs to be explained and discussed at some length.

*A: The temperature measurements were done in-situ immediately after core extraction, whereas the bulk salinity measurements were obtained from melted core segments cut within an hour after extraction in the cold lab at -10 degrees Celsius. As for the bulk salinity, only drainage but not temperature affect its magnitude. As mentioned before, the authors only observed substantial initial drainage during ice collection. In this sense, the physical properties temperature, salinity and thus, relative brine volume (computed from temperature and bulk salinity values) are relatively close to in-situ conditions but have been affected by drainage at extraction. As such, the actual relative brine volume before ice extraction from the ocean can be assumed to have higher magnitudes.*

*On the other hand, the mechanical properties were obtained in the cold lab at -10 degrees within 2 to 3 hours after ice core extraction, i.e. the mechanical properties relate to sea ice at -10 degrees Celsius whereas relative brine volume to the in-situ temperature distribution. In this sense, the reviewer is correct that there is an ambiguity in the mechanical properties plotted vs. brine volume shown in Figs. 13 (Young's Modulus) and 16 (uniaxial compression strength). Therefore, the relative brine volume distribution for all cores will be recomputed using the relation by Langleben and Pounder 1963 with -10 degrees Celsius so that they directly correspond to the ice conditions when the mechanical testing was done. The graphs in Figs. 13 and 16 will be subsequently updated in the revised manuscript.*

Line 293 – This statement is inconsistent with line 303, which states that Kivamaa and Kosloff did such measurements in the Weddell Sea.

*A: Only Urabe and Inoue (1988) obtained Antarctic sea ice strength measurement during winter. Kivimaa and Kosloff (1994) also obtained Antarctic sea ice strength measurements, but during spring. In this sense, only Urabe and Inoue as well as ourselves obtained **winter** Antarctic sea ice strength measurements. This difference and unique feature of the obtained data will be better explained in the revised manuscript.*

Conclusion – can you elaborate on your results in terms of what they might imply for sea ice mechanical modelling, etc?

*A: Agreed. It will be added to the conclusion that the obtained data on mechanical properties of pancake ice floes in the Antarctic winter MIZ will help to parameterize realistic small-scale sea ice dynamics models with respect to aspects concerning the the influence of pancake ice floe deformation on the inelastic collision restitution and fracture due to collision. Both phenomena are expected to influence sea ice formation, sea ice drift and wave attenuation.*

*Furthermore, the obtained measurements of anisotropic elastic material properties of consolidated pack ice, in particular, are unique, because they also comprise their directional dependency in vertical and horizontal directions, respectively. This aspect has been usually considered insignificant in literature and is not reported on. Where applicable, the complete set of anisotropic elastic material properties, however, is required when realistically modelling sea ice deformation.*

Appendix B1 – something is wrong here. I just see a bunch of gray bars, and no sections.

*A: Appendix B contains schematics of the core segmentation used for the elastic and uniaxial strength testing. There might have been a problem with the file upload. This will be corrected in the revised manuscript.*