The Cryosphere Discussions

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Comment on tc-2021-209

Ted Maksym (Referee)

Referee 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-RC2, 2022

Note this comment is provided by the handling editor in lieu of a second referee comment. Based on this assessment and the prior comment, the authors should respond to these comments first. You only need to provide detailed responses to substantive comments and do not need to prepare a revised manuscript at this time. If the major comments can be adequately addressed, you will be asked to submit a revised manuscript.

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.

- 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.
- 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).

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.

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.

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.

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

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.

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.

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

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.

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.

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.

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.

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.

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 dure to brine volumes, etc, which you have not properly captured (see next point).

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 Vb ~4%. This puts all your values for Youngs 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.

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

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

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