Year-round assessment of sea ice pressure ridges by multi-frequency electromagnetic induction sounding
by Polona Itkin and others

Submitted to The Cryosphere

Review by Andy Mahoney
April 10, 2026

Summary
This manuscript presents an interesting timeseries of multifrequency EM sounding observations of pressure ridges surrounding the MOSAiC expedition. The data presented show great promise for advancing our understanding of both the thermodynamic evolution of ridge keels and the effect of macroporosity on EM sounding of sea ice. I very much look forward to seeing this work published, but I fear the manuscript is currently in a somewhat unpolished state and relies too much on key details and results that are presented in other papers. It is clear that the authors are highly familiar with the adjacent literature, but I feel that more details from these other sources should be provided so that the manuscript can better stand alone as a separate publication. At the same time, the paper presents some results that feel tangential to the key findings presented. Hence, if length becomes an issue, I think the authors could omit some of this material to make room for greater discussion of details that are currently delegated to citations of other literature. I also find that readability of the text is made more difficult by a somewhat unorthodox structure that deviates from the more typical practice of using separate, clearly defined sections for the methods, results, and discussion. I recognize that this may sound like strong criticism, but I do not want to discourage the authors as I believe all my concerns can be addressed quite readily. I have provided more details for my principal concerns below and where possible I have tried to make constructive guidance for addressing them.

Major Comments
1. Over reliance on material published elsewhere
A number of the manuscript’s findings rely on drill-based measurements of macroporosity within the ridge keel and the identification of the consolidated layer (CL). However, the text devotes just 4 lines to explain the underlying methodology and reports the associated results without uncertainty estimates or, in some cases, any clear source (e.g., macroporosities reported on line 308). I understand that the relevant details are provided in recent work by Salganik et al, but I feel the reader should still be given enough information to understand the method and results necessary for this work without having to read another paper.
Similarly, I feel the brief description of the thermistor chains would likely only be meaningful to a reader already familiar with the SIMBA instrument and no data from these instruments are presented in the text. Instead, the thermistor results are reported entirely through citation of other work. If the thermistor data are important for supporting the thermodynamic modelling then I feel they should be given more space within the manuscript.
Lastly, I would also like to see brief explanations of how the ALS and multibeam data were tied to local sea level to derive freeboard and draft, respectively, and the associated uncertainties. These details may be provided in the paper cited in the text, but I feel they should be included here for completeness.

2. Unclear presentation of EM thickness results
Figure 5 presents some of the most important results in the manuscript, but I find it very difficult to understand. Based on what is written in the text, I had expected that panel (a) would show in-phase EMI thickness results and panel (b) would show results from the quadrature component. The caption suggests these are distinguished by dots and crosses, respectively, but the plots in both

panels show both dots and crosses, making it appear that both plots show the same set of EMI thickness measurements. However, the vertical positions of the data points in panel (a) do not match those in panel (b), suggesting they are not showing the same EMI data. I'm therefore at a loss to understand what should be one of the paper's key figures. Some of this might be fixed with decluttering of unnecessary data points and a clearer legend and caption, but some attention may also be needed to ensure the correct data are being plotted.

3. No explicit explanation of EMI-derived CL thickness
I cannot find any place in the text where the method for determining CL thickness from EMI data is explicitly described. The text on line 298 states "the CL thickness aligns mainly with the quadrature values of the highest frequencies (18, 60, and 98 kHz)", but there are no specific details regarding how this applied to derive CL thickness from EMI data. A dedicated reader might infer from the preceding paragraph that CL thickness is determined from the average EMI thickness values derived from the 18, 60 and 98 kHz quadrature data, but I would like to see a clear statement of this in the text, together with a quantitative discussion of uncertainty.

4. Unnecessary presentation of spatial ridge distribution
The locations and spatial distribution of ridges discussed are not crucial information for supporting the manuscript's key findings. I also feel the figures presenting this information are somewhat cluttered and difficult to interpret. Hence, if space becomes an issue, I suggest that Figures 3, 4, and 8 and associated text could be omitted or moved to an appendix with minimal impact to the findings.

5. Recommended restructuring of method, results, and discussion
This may be partly a matter of style, but I encourage the authors to consider reorganization of the text to improve readability, particularly for the benefit of readers who may be encountering this subject matter for the first time. Specifically, I recommend a cleaner separation of content between the Methods, Results, and Discussion section. In its current form, section 2 presents the reader with both methods and results, while section 3 is a combined "Results and Discussion" section. There are even important drill hole results presented in the Introduction section. As a consequence, I found myself having to go back and forth re-reading sections multiple times to follow the chain of reasoning and evidence. Some further use of subheadings to break-up lengthy sections of discussion into discrete topics may also help.

(The minor comments provided by you are now listed at the end of our response and addressed individually there.)

Author's response

Dear Andy,

Thank you very much for reading and reviewing our manuscript! Your comments and suggestions are highly constructive and will be taken into account to improve this paper.

Your remark that the co-authors feel very at home in the MOSAiC results and did not do a good enough job in assuring the paper is a true stand-alone work is well received, and we will revise the manuscript accordingly! We will expand the methods descriptions by providing more text from the cited literature to make it easier for the reads to comprehend. To address the problem with the manuscript length, we will move some of the figures to the supplementary material. We would like to keep the current paper section structure, but some of the text will be moved between sections as you and the other reviewer suggest.

Here is the list of specific modifications we will do to address your major comments:

1. We will provide a more extensive summary of the MOSAiC-related work previously published by Salganik et al. (several papers). We will also provide an improved description of the thermistor chain data as they are relevant for understanding of the consolidated layer retrieval results by EMI and for the thermodynamic modeling. We will also include information about the uncertainties of the ALS data, based on the published literature (e.g. Hutter et al, 2023b give freeboard uncertainty of around 10 cm). The MB sonar data uncertainties are large and in this paper the drafts were manually corrected (shifted uniformly). We will communicate that better in the Methods section.
2. Figure 5 does not show in-phase retrievals on panel a) and quadrature retrievals on panel b), but instead total sea ice thickness (from drill holes) compared to all EMI retrievals (in-phase and quadrature) on panel a) and consolidated layer thickness (from drill holes) compared to all EMI retrievals (in-phase and quadrature) on panel b). In other words, the EMI data is the same (y-axis), but the drill hole data is different (x-axis). This will be now better explained in the caption. We believe that all EMI data should be displayed on both panels, so that the reader can understand why we decided on the averages of specific channels. We will also try to make the figure more readable (colors, markers, sizes) and adjust the legend (add crosses markers there too).
3. The total and CL thickness from EMI is estimated exactly as the reviewer induced. We will improve this text to make this clearer (provide an equation) and gather the description of uncertainty that can be deduced from Figure 5 and is discussed elsewhere in the text.
4. You are right. We believe these figures are necessary to demonstrate that our method is useful in field data interpretation, however the figures can be entirely or partially moved to the supplemented materials.
5. We will carefully review the entire manuscript, so that it can be read sequentially and to reduce the need to move back and re-read the sections. We will also address all the minor comments individually and this will also improve structures and sequence of relevant information. Although we will add some information as described under the first major comment response, the overall manuscript length will be reduced.

Minor comments (directly copied from the review by Andy Mahoney with our response printed in purple).

Line 194-195: Does this 10 m accuracy refer to absolute position accuracy, or the accuracy with which you are able to co-locate measurements? The latter seems more important to me in this context, but 10 m seems too high to be useful for co-locating the different measurements described here, all of which have footprints much smaller than this.

This is our pessimistic estimate of how far two collocated datasets may be. While the accuracy of the ALS relative positioning is certainly within 1m, the MB draft is more difficult to assess. The EMI footprint is about 4x the total sea ice thickness (24 m for a 6-m-deep ridge and 32m for an 8-m-deep ridge). This means that 10-m is well inside this estimate. A more optimistic estimate would be that we estimated the EM draft location within 1-2m. We will revise this ‘10 m accuracy’ to 1 m accuracy for ALS and 1-10 m accuracy for MB in the next version of the manuscript.

Line 209: As it is stated, I find this assumption of constant “ridge porosity” a little confusing given the attention paid to ridge consolidation in this paper. Would it be more accurate to state “… and we assume the macroporosity of the unconsolidated rubble remains constant …”?

This refers to statement: ‘We determine ridge porosity from drilling measurements and assume it remains constant for each location of the two ridges.’

Here we should refer to rubble macroporosity, instead of the ridge porosity, so the revised statement will be:

'We determine rubble macroporosity from drilling measurements and assume it remains constant for each location of the two ridges.'

Figures 3 & 4: I find both of these figures quite cluttered and the information they provide on the spatial distribution of ridges does not seem critical to the discussion. Could they simply be omitted, along with much of this section, or moved to an appendix?

Although this section appears much like cruise-report style, it provides basic maps with names of the locations and is important to be kept in the manuscript. Our hope is that this dataset can be used by others in an inverse modeling study. We can consider moving this and other similar sections to the supplementary material. We will consider technical possibilities to do so (references to naming of location etc.).

Lines 250-273: These lines contain most of the relevant information found in this section and could be better placed into a table

This is a good idea. We will prepare a table.

Lines 290-293: I do not recommend using the same term for two different properties, even with a qualifying statement like that used here. Could the authors instead use a new term to describe CL thickness plus freeboard? Perhaps something as simple as "CL+" ?

This is also a good idea. We will reformulate it.

Lines 308-310: It took me longer than necessary to realize there was a missing cross-reference to Figure 2, in part because Figure 2 is found within the Introduction, rather than the results, where I would have expected it.

The missing reference is in fact the Salganik et al, 2023 paper and Figure 2. We will modify the text accordingly and explain how we arrived at these values.

Lines 319-320: I'm afraid I don't find this argument to be well supported by the results shown. This may be helped by redrawing Figure 5 to better show the relationships described. I think it would also be helpful to present the frequency sensitivity more clearly so that the reader can better understand how the difference between low- and high-frequency responses related to thickness of unconsolidated ice in the keel.

Figure 5 and its captions will be improved as described under major comment #2.

Figure 8: I don't find this figure very useful. It is difficult determine changes in consolidation over time because the regions useful comparison are too small. Figure 9 is much more suitable for this purpose. Since Figure 8 is only referenced once in the text and the spatial distribution of the ridge keels is not critical to any key finding, I suggest this figure could be omitted.

We will review this figure and attempt to improve the colormap. In any case, the figure will be omitted or put into supplementary materials.