Reviewer 1

This manuscript presents a valuable and timely study by utilizing multi-frequency Electromagnetic Induction (EMI) data from the MOSAiC expedition to assess pressure ridge properties. A particular strength is comprehensive validation against drill-hole, airborne laser scanner (ALS), and multibeam (MB) sonar data. The work usefully explores the potential of EMI to estimate both total ridge thickness and consolidated-layer (CL) thickness. The manuscript requires some revisions for clarity. Below are my specific comments.

(Specific comments are now integrated into the Author's response below.)

Author's response

Dear reviewer,

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

Our improvements here are listed point-wise in the text below. The reviewer's comments are printed in blue and our responses in purple.

Abstract: Please highlight the core innovation of multi-frequency EMI method in the abstract.

We will review the abstract and make it more focused towards the EMI method presented in this manuscript. We will list the specific channels and clarify that we use their corresponding mean values.

The conclusion section could be more concise, focusing on the advantages and limitations of the EMI method. The EMI method performs well in detecting ridges with a thickness of ≤ 10 m, but systematic biases occur in areas with high porosity or steep topography."

The conclusions are already quite concise but combine the method evaluation as well as key findings for MOSAiC ridge consolidation. We are currently not planning any follow-up study (The dataset will be made available for any follow-up inverse modeling study.) and many readers (non-experts in EMI, but product users) will appreciate other results we found, and we would like to have them highlighted also in conclusions.

Main text:

The entire paragraph in Line 62 regarding "ridge melting rate" has weak relevance to the main theme of the study and is suggested to be removed.

We will review the text about melting and reduce it, remove it and move to discussion, where we present the melt period data. The text has significance for the sesonal development of ridge total thickness.

For the paragraph in Line 73, a smoother transition from "commonly used ridge detection methods" to the "EMI method" is advised. It is recommended to add little sentences explaining the limitations of conventional techniques. This will naturally lead to highlighting the advantages of the EMI method.

We will elaborate on this method comparison and expand the section. In the paragraph currently starting in line 67, we will talk about the low sample number and then expand how EMI methods have so far mainly been just over level ice or for detection of the modal sea ice thickness. Some deformed ice fraction estimated were done as analysis of PDFs, but rarely (if at all) as individual ridges.

- Line 110: The choice of the five specific frequencies (1.5, 5.3, 18, 63, 93 kHz) needs clearer physical justification. Has a sensitivity analysis been conducted to clarify the response characteristics of each frequency to ridges with different porosities and thicknesses? This is particularly important because, the figures in the appendix, the thicknesses inverted by different frequencies vary.

The EMI method detects overall electrical conductivity, which the manuscript correlates with "macro-porosity". For the same porosity, will concentrate large porosity or scattered small porosity lead to different EMI responses? Moreover, based on drilling data from six ridges, the manuscript suggests that the porosity threshold of the consolidated layer detected by the quadrature of high-frequency EMI is approximately 20–30%. Is this threshold universal? In reality, the porosity of ridges will be lower than 20% after reconsolidation.

The suggested method can unfortunately only estimate CL and total thickness of ridges. Porosity is a variable that limited these estimates. We will review our wording regarding this in the paper and make this more clear. The suggestion of the reviewer here is unfortunately beyond the scope of this study. This is the first assessment of the multi-frequency EMI method (in peer-reviewed publication) for ridge consolidation, and it is using the empirical approach. The frequencies were preselected somehow arbitrary – as logarithmically-spaced values across the measurement interval of the instrument. No previous study was made and no frequencies recommended (to our knowledge). We will explain this in the manuscript. One of the conclusions of the paper is to recommend a more physical explanation (by using inverse model) to do what the reviewer here is suggesting. Our data sample is relatively small for further interpretation of the data empirically (statistically). Winter polar night conditions unfortunately did not permit more work, then the logistics (small team, sea ice deformation) prevented further such studies in the spring, when there was more light, more manageable temperatures and generally safer working conditions. **We will highlight the preliminarily nature of this study in the abstract**.

Section 3.1: The matching methods between EMI data and drilling, sonar, and etc. should be clearly stated at the beginning of Section 3.1. This will prevent readers from having to wait until the results section to understand how the data fusion was performed. Additionally, while the manuscript compares EMI data with positioned drilling, ALS, and MB sonar data, there are differences in spatial resolution among these data. How were these differences evaluated? What causes the local discrepancies between EMI, the thermodynamic model, and the thermistor chain data?

The matching of the data is described in Section 2.7 Co-location and bias correction of the data'. The accuracy and resolution of the ALS and MB data is given in sections 2.5 and 2.6. The discussion about the discrepancies is currently scattered in the manuscript as it was pointed out by the other reviewer and we will reorganize the manuscript better, so that the information comes more sequentially and it easier to follow. We will also reduce the number of Figures in the manuscript, so that the paper will be shorter and such information easier to follow.

After defining CL in Line 38, the definition is repeated in Line 46.

This will be reviewed and removed.

The terms “macro-porosity” and “rubble macro-porosity” are used interchangeably throughout the manuscript. What is the difference between these two terms?

In literature rubble porosity is mainly used in themodynamical models of ridge consolidation. Keel porosity is a more widely used term that can characterize ridge consolidation also during melt season (unlike rubble porosity). We will review the manuscript and consistently only use ‘porosity’ and refer to any other other terms (only if and where necessary) with sufficient explanation to the reader.

Fig. 3 & Fig. 8: The continuous color bar for snow freeboard uses similar hues to the discrete colors overlain for ridge age groups.

We will review and modify the colors used.

A sentence regarding suggestions for the future application of the EMI method could be added at the end of the conclusion section. This will extend the implications of the study and provide guidance for subsequent research in this field.

Thank you for this suggestion – we will add such a statement.