

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-61

Anonymous Referee #1

Referee comment on "Winter growth and tidal variability of the sub-ice platelet layer observed with electromagnetic induction soundings" by Gemma M. Brett et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-61-RC1>, 2021

Several findings are reported here using applications of EMI soundings to investigate Subice platelet layers in McMurdo Sound. 1. Ground-based surveys with concurrent drill hole and snow depth measurements on large spatial scales. 2. Small spatial scale surveys conducted over several days with high resolution. and 3. Time series measurements using a fixed point EMI to map variations of SIPL at various time scales. The authors then relate each of these sets on SIPL properties to driving forces. Previous studies have measured the spatial distributions (only) in late spring while this study made winter survey measurements and time series measurements at a fixed site.

The authors' conclude that the variability observed in the sub-ice platelet layer indicates that a combination of the tides, wind-driven polynya activity and the presence of multi-year ice influences the circulation of Ice Shelf Water in the upper surface ocean and consequently sub-ice platelet layer formation over a range of timescales.

General Comment.

I found that the structure of the paper, jumping from the three different types of measurements was laborious to follow. This was especially true when three separate sets of results are presented with quite different aims and then are not discussed until after all the results are presented so there is a cognitive break in this reader's ability to follow a discussion and interpretation from a set of results that occurred several pages before. My recommendation is to revise the paper into a shorter version covering the ground-based spatial surveys (large and possibly small spatial scales) and leave out the time series measurements at a fixed point and the detailed tides dependence calculation, which are inconclusive after a lengthy analysis. These single point time series measurements (possibly coupled with the small-scale surveys?) may form a second paper. The findings from the ground surveys on large spatial scales are worthy of publication.

Some detailed comments follow.

Detailed Comments

Figure 1. The color bar scales with only end points indicated, gave no indication of the intermediate scales color values, which appear to be also nonlinear? The middle one is also reversed from the other two, with blue being the thickest ice, while blues are near the thinnest snow depths and SIPL thicknesses. Recommend providing some intermediate values on the color bar scales rather than just zero and maximum values and redoing the snow depth so that thin and thick colors are the same as the other two rather than reversed.

Figure 2. Lacks labeling of the blue contours relating to SIPL thickness. The caption reads that these are in 0.10m steps from 0 to 10m which would correspond to a hundred blue lines a number much greater than the number of lines displayed unless there are ~50 lines packed in near the bottom? I assume the scales goes from 0 at the bottom to 10m at the top but the spacing at 0.10m is not clear. Possibly place a few numbers on the blue lines to indicate the scale.

Line s 165-170 While the experimental data gives SIPL conductivity values of 800mSm⁻¹ for First year Ice, and 1000mSm⁻¹ for Second year ice, the authors choose neither of the above for the forward modeling (900 mSm⁻¹)? Are the forward modeling results significantly different for 800 or 1000 than they are for 900? Would it be better to show two modeling runs for the two actual measured conductivities than for an intermediate not observed value?

Pg.9, Figure 3c. The color of the dots are hard to compare with the EMI trace they correspond to for year as they appear (to me) as dark blue which has no parallel EMI trace color in 3c. Which year are those drill hole dots for SIPL thickness taken? Especially these seemingly dark blue (?)dots, they don't seem to correspond very well to any of the measured EMI Traces, especially in the left hand side? Can you explain?

Figure 6. The lack of horizontal time scales on b and c lead to some confusion with the spatial scale on Fig 6a. I don't see the merit in putting the spatial scale figure a in with the time scale figures b, c, d. Suggest breaking this into two figures, one with 6a only to show the location of the time series within the broader spatial scales and another figure with b, c, d with better labeling on each for the time scales.

Fig 6d While on different days, the drill hole SIPL thickness(3Nov) and EMI thicknesses measure from 4 Nov on seem quite different? Can you comment?

Line 291 Measured conductivity of 900mSm⁻¹ was derived from drill hole measurements but differed from the 800 (FYI) or 1000(SYI) discussed above?

Line 295 In the figure looks like EMI is underestimated for sea ice relative to drill holes and SIPL is overestimated for EMI compared to drill holes? Text indicates EMI overestimated for both.

Lines 333-335-337 reads "The magnitude of change in *I*, *Q* and SIPL thickness at the WSN over the tidal range of each flood (positive from trough to peak) and ebb (negative from peak to trough) spring and neap tide was quantified and is shown in Fig. 8 with linear fits applied. Opposing trends were observed in *Q* and *I* with the tides (Fig. 8a). *I* decreased on ebb tides and increased on flood tides. In contrast, *Q* 335 increased on ebb tides and decreased on flood tides." This was investigated on pgs 15 and 16 with forward modeling parameters and attributed to variations in seawater conductivity as opposed to SIPL conductivity. It's supposedly discussed in Section 4.5 relating to seawater salinity and temperature. But the beginning of the Discussion section (4.1) jumps back to the Spatial surveys data. I find the interleaving of two or three quite different studies, the large scale spatial surveys, the small-scale surveys with a time components, and the single site high temporal scale measurements extremely confusing to follow coherently.

Section 4.5 Discussion following the results referred to above. Here after rejection of the two effects modeled on pgs 15 and 16 of changes in seawater conductivity and SIPL conductivity, we now find that both of these are probably not the cause. Instead a new discussion is injected where the variations of the thickness of the SIPL is examined and related to other observations of the SIPL using underwater video. Again this discussion is inconclusive as to whether this third cause is responsible. Makes for a lot of difficult reading to arrive at a somewhat null result?