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Interactive comment on "2020 Larsen C Ice Shelf surface melt is a 40-year record high" *by* Suzanne Bevan et al.

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Response to Reviewer 2.

We thank the reviewer for the careful and complimentary review of the manuscript. We have addressed the comments below and agree that the manuscript will be much improved accordingly.

Comments (by line number):

15: The introduction is extremely brief and context, timeliness and wider importance is lacking. You should mention by way of context here that inland ice acceleration and discharge is expected in the event of a collapse of Larsen C, although the glaciers are not thought to be highly buttressed and therefore the sea-level response is likely to be modest (Furst et al., 2016 doi:10.1038/nclimate2912; Schannwell et al., 2018 doi:10.5194/tc-12-2307-2018).

Reply

We will improve on the context in the introduction by adding the following text to the end of the first paragraph.

‘If LCIS were to disintegrate, following the collapse of Prince Gustav and Larsen A ice shelves in 1995 (Rott et al., 1996) and Larsen B ice shelf in 2002 (Rott et al., 2002), modelling studies suggest that the dynamic response of the inland ice might be limited owing to the small amount of buttressing generated by LCIS (Furst et al., 2016), and the potential sea-level contribution of the order of millimetres over the next two centuries (Schannwell et al., 2018). However, removal of ice shelves has consequences other than sea-level rise with potential impacts on ocean circulation and biodiversity (Siegert et al., 2019).’

54: if you’re going to state that the threshold determination is ‘carefully considered’, it may be worth detailing that consideration, rather than just stating it’s what has been done previously.

Reply

Accepted, we will change this sentence to read: We record the presence of melt water at a pixel location when the backscatter is more than 2.7 dB below the previous winter (June, July, August) mean. This threshold was used in previous studies using SIR ASCAT data (C band, 5.255 GHz) (Ashcraft and Long, 2006; Bevan et al., 2018) and was based on empirical comparisons with QuikSCAT derived melt (Ashcraft and Long, 2006). We have no reason to suspect that Level 1 ASCAT data would require a different threshold, and melt patterns and variability have anyway been found to be insensitive to changing the threshold between 2 and 4 dB (Wismann, 2000).

57: these acquisition times differ from those of QSCAT, can you discuss discrepancies in melt detection between QSCAT image acquisition and ASCAT during their cross overperiod?

Reply

Rather than repeat the discussion of differences between the products, and reasons for the differences, we will refer the reader more explicitly to an earlier paper by including the following sentence in the Methods and Data section: ‘Bevan et al. (2018) discussed the impact of acquisition time of day with respect to differences between SIR QuikSCAT and ASCAT melt detection on LCIS. The 2008/2009 year of overlap showed that MI based on the QuikSCAT morning data was 3.3×106 melt days km2 and MI based on ASCAT was 3.4×106 melt days km2.’

99: Odd to draw comparison on first line of results with data presented by another paper. Readers may look to your figure 6 here, erroneously. This sentence describes results that are not shown in this paper.

Reply

This is a good point and we will include a map of the 2017/2018 melt based on SIR ASCAT data to make for an easier comparison.

101: how intense? How much greater?

Reply

We have added the phrase ‘with up to 139 melt days in the south-west inlets’ to the end of the sentence.

142: Would you not expect an ENSO teleconnection to be important to western Antarctic Peninsula temperatures, as per previously published work? This doesn’t seem to be mentioned here (main focus is on the IOD), but it ought to be discussed to be able to relate this paper to findings in previous literature.

Reply

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This point was also raised by Reviewer 1 and is an important one. We will add some text to the Methods and Data section, to the Results section and to the Discussion section. Please see our response to Reviewer 1 for more details.

164: but station measurements are not shown in Figure 5... Is this referenceable, or could you plot them in an additional Appendix figure (on inset within a figure)?

Reply

We were referring to the station temperatures reported in the introduction. We will remove the reference to them from this sentence.

Reply

170-171: this is speculative rather than proven, I think. If proven, please provide a reference, if not then amend text to 'may also reduce'.

Reply

The effect of persistent subsurface meltwater on melt detection was demonstrated in Bevan et al. (2018) so we have included the reference here.

192: Perhaps a final summary statement on the importance of large-scale atmospheric influences on ice shelves for stability, environmental change, sea-level rise implications?

Reply

We think our final paragraph of the Conclusions already covers this but we will add a final paragraph to the Discussion: Increasingly strong SAM (Arblaster and Meehl, 2006) and more extreme positive IODs (Cai et al., 2014) predicted under high greenhouse gas emission scenarios may act in opposition with regard to AP warming as they decrease or increase sea ice over the Weddell Sea, respectively. As we have revealed in this study the implications for eastern AP ice-shelf melt are further complicated by southern-hemisphere SSWs, and it is not yet understood how global warming will af-

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fect the occurrence or intensity of SSWs in either hemisphere, indeed there is not yet a consensus on the best way to define an SSW (Butler et al.,2015).

Reply

Figure 1: Does the SAR backscatter image need a colour scale?

Reply

We will add one.

Figure 2: For ease of viewing could you instead differentiate the colour scales in Figure 4 to grey, blue, red (instead of dark grey, light grey, blue)? And is there a reason why the January bar is dark grey? Is this related or not to the 2019/2020 (dark grey, also) curve?

Reply

Yes, we will change the dark grey data line to red. The January vertical bar is only to mark the calendar year change, changing the colour scale will resolve this ambiguity thank you.

Figure A2: Given averaging periods were 'shifted slightly to maximize signals', what effect would you see from different averaging periods?

Reply

This was also a point raised by Reviewer 1 and we answered that the averaging period can be justified because of the abrupt downward expression of the SSW on about 17 Oct, causing SAM to go negative. So, it is based on what actually happened and in that way helps to maximize the signal. We will add a note on this point to the caption of Fig. A2.

Additional references

Arblaster, J. M. and Meehl, G. A.: Contributions of External Forcings

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