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Comment on tc-2022-127

Anonymous Referee #2

Referee comment on "Sentinel-1 detection of seasonal and perennial firn aquifers in the Antarctic Peninsula" by Lena G. Buth et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-127-RC2, 2022

This manuscript details a new method for detecting firn aquifers over the Antarctic Peninsula (AP) from Sentinel-1 synthetic aperture radar data. The detection algorithm is based on the idea that in regions with firn aquifers, summer meltwater will refreeze more slowly within the firn due to high melt and warm subsurface temperatures. This refreezing process is reflected as a rebound of the SAR surface backscatter value from low values over wet firn in the melt season, to higher values over dry firn in the refreezing season. The authors set a detection threshold that firn aquifers exist where backscatter reaches 80% of its average September value on or after the 105th day of the year. Using this threshold, they map aquifers in the 2017-2020 seasons. They compare these mappings to predictions from the IMAU-FDM firn model and show that they are in broad agreement. Based on the large interannual variations in Sentinel-1 aquifer extent, they also conclude that seasonal firn aquifers may be common on the AP.

The motivation for satellite mapping of firn aquifers in Antarctic is clear and this paper advances that effort. However, the detection utility of the algorithm is limited by a lack of validation. This is not necessarily the authors' fault – a robust validation data set for the AP that overlaps the Sentinel-1 era does not exist – but combined with the lack of uncertainty quantification in the current methods, the conclusions that can be drawn from the final mappings are, in my opinion, very limited.

Major Comments:

[1] It is not clear why the authors choose to develop a completely new Sentinel-1 firn aquifer detection algorithm for Antarctica when a reasonably robust algorithm was already developed in Brangers, et al (2020) for Greenland and validated against the OIB firn aquifer detections. What is the justification for not applying the Brangers, et al (2020) methodology to Antarctica? Of course, there will be some environmental differences between the two locations that can impact empirical thresholds, but given the lack of Antarctic validation data, the authors already have to choose essentially arbitrary

thresholds in their method, and that does not seem to be a clearly better choice than adopting the thresholds tuned for Greenland. Presumably the accumulation and melt percolation processing the control refreezing rate will not be so different on ice shelves as to make the Brangers, et al (2020) results totally useless.

At a minimum, the paper would be significantly strengthened by a clear discussion of why a new detection algorithm is needed and how this new method improves up on the previous results of Brangers, et al (2020). Ideally, a quantitative comparison between aquifer extent calculated from the Brangers method and this newly proposed method would be presented so that the reader can assess to what extent the results are consistent.

[2] How does the algorithm deal with the possibility that backscatter values do not rebound to a stable September value that is consistent from year to year? (The Muller signal seems to be an example of this.) Assuming this indicates large quantities of near-surface liquid water that did not refreeze over the winter, I would expect that you would want to either include these areas as aquifers detections (maybe as a second filter in the algorithm), or completely discard them from the data set if they seem to be indicative of something like refreezing ponds.

[3] I do not think this paper can confidently make the conclusion that firn aquifers on the AP are largely seasonal from these data. Given that Sentinel-1 is only sensitive to the upper 7m, the data only suggest that there is a large seasonal variation in how quickly the upper 7m of the snow/firn column refreeze. I am sure that there is some cutoff where the refreezing rate is so rapid that it is no longer consistent with the presence of a temperate firn layer at depth, but without understanding physically where that threshold lies, one cannot rule out that the inter-annual variation in aquifer extent may be the result of noisy data, uncertainty in the validity of the DOY80 threshold, or controlled by variations in total accumulation and melt from year to year.

I would encourage the authors to at least try to quantify how much of the inter-annual variation in aquifer extent is robust to small changes in the DOY80 threshold (greater or less than 105) or to uncertainty in where the backscatter time series crosses the 80% threshold due to radiometric uncertainty or high frequency oscillations in the backscatter time series. (I recognize that the time series has been smoothed, but the smoothing window is pretty arbitrary, so there is some implicit uncertainty associated with the choice of that window.)

Line Comments:

Line 24 – perhaps specify "refreezing in the firn" here

Line 24 – consider more than one citation here, perhaps 1-2 supporting papers for each of the mechanisms discusssed (refreezing, runoff, supraglacial storage, etc…) to better support a general point about all ice shelves, rather than just the Roi Baudouin Ice Shelf discussed in the Lenaerts (2017) paper.

Line 26 – citation? I am not sure that we have a well-developed idea of what a "seasonal firn aquifer" really is, since it is not clear that such a thing has been observed in the field. Throughout the paper, the authors seem to use this as a catch-all term for a damp firn layer that refreezes over the winter, which I am not sure necessarily qualifies as an aquifer. It might be better just to refer to these seasonal signals as "transient liquid water storage" or something similar.

Line 57 – how is the data reprojected onto the EASE grid? What type of interpolation or binning is used to assign values to a grid cell? How are the native resolution data aggregated within a grid cell – taking the mean, for example? If data are interpolated or average, is this done in linear of dB space? Can the statistics of the native pixels assigned to each grid cell provide an estimate of the radiometric uncertainty within each EASE grid cell?

Line 59 – roughly how much do geometric parameters like look angle vary from RON to RON? At high look angles, is there any concern that the backscatter will hit the noise floor and bias the average values used for geometric bias correction?

Line 61 – does "the bias is determined pixel-wise" mean that a unique bias is calculated for each pixel? If so, how is the average calculated for the RON? Or does this mean that for any given image, the average is found, normalized by the average across all images used in the study, and then all pixels in the image are offset by that difference?

Line 67 – what set-up is used for the SMRT runs? IBA vs DMRT vs Mie/Rayleigh scattering? Autocorrelation function and correlation lengths?

Line 70 – how are these parameters chosen? How much does the reliable penetration depth vary with the density or grain size of the overlying snow or the liquid water content of the wet layer? This setup makes sense for understanding the impact of accumulation events over wet firn on detection, but may not be particularly representative of system sensitivity to wet firn layers deeper within an established firn pack.

Line 92 – how sensitive are the results to the smoothing threshold?

Line 130 – what electromagnetic model is used (IBA, etc) and how are the microstructural parameters chosen?

Line 133 – section 3.1 might be better placed with section 3.4, since the main purpose of 3.1 seems to be to convince the reader that changes in IMAU-FDM LWC should be comparable to the Sentinel-1 signal and to highlight some of the uncertainties in the results.

Line 149 – have you considered masking out regions of bare ice or ice slabs as simulated by IMAU-FDM when looking for firn aquifers? This might help avoid the potential for mixed signals from buried lakes.

Line 152/Figure 5 – is there a reason to show the spatial distribution of DOY80 since a strict cutoff threshold is used for aquifer detection? I actually do think it's useful since the cutoff is uncertain, but it is worth discussing spatial patterns in that uncertainty in the text if you choose to show it.

Line 173 – I am not sure that melt underestimation in the model due to surface ponds should apply. Presumably firn aquifer areas should be largely mutually exclusive with areas of surface ponding?

Line 211 – where did 15m come from when your SMRT sensitivity study showed an effective penetration depth of only 7m?

Line 212 – why not try to incorporate this into the algorithm?

Line 200 - section 4.1 – this is a good and very important section that places the results of this work into the appropriate context of the uncertainty

Line 249 – again, I feel strongly that the uncertainty analysis and the system sensitivity are not sufficiently robust to make any real statements about seasonal variability.

Line 260 – any of the airborne OIB data available will be from November, so this requirement has been met by most if not all previous ice-penetrating radar data collection

Line 266 – much too strong of a statement given your level of evidence! Should you expect to see large floating ice shelves on the western side of the peninsula given the local ice dynamics and calving rates? Is there evidence of past ice shelf collapse along the western side of the peninsula in regions where you detect aquifers? You can speculate on these questions, but a statement that perennial firn aquifer development at the grounding line is a precursor for ice shelf collapse is not supported by the data in this manuscript.