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Interactive comment on "The regional scale surface mass balance of Pine Island Glacier, West Antarctica over the period 2005–2014, derived from airborne radar soundings and neutron probe measurements" *by* Stefan Kowalewski et al.

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Kowalewski et al., The regional scale surface mass balance of Pine Island Glacier, West Antarctica over the period 2005–2014, derived from airborne radar soundings and neutron probe measurements

The authors present a very thorough and well conducted analysis of airborne radar data collected over the Pine Island Glacier catchment to recover estimates of surface mass balance. By tracking single horizon (circa 2005), dated through unique neutron

probe depth-age and depth-density information, they successfully mapped SMB over a large portion of the Pine Island catchment area. The radar technique presented is robust, and the authors apply an improved kriging technique to spatially interpolate the tracks to a more complete grid. The results are largely in agreement with a prior work, suggesting little change in snow accumulation over the region and also highlighting the robustness of the approach.

The paper details a significant amount of work that is praiseworthy. A few clarifications are required to improve transparency in a few locations (see Major Comments). The paper is generally well-written and outlined but could use substantial editing for improved flow and clarity. It is great to read such interesting studies; I have made a few major and minor comments below for the authors to consider to help improve the presented work.

Major Comments

- On line 49 there is mention of "adjust"ing layers at crossovers. How was this accomplished? Will this impact the results? In the description later on line 132-134, there should be discussion regarding the magnitude of the adjustments. Some discussion of what this means for the total uncertainty would be welcome. Are only the layer traces impacted or are the layer ages adjusted to the adjusted layer depth?

- Regarding the analysis of the NP density profiles: please clarify in the Fig. 2 caption an in the paragraph beginning Line 88 that only n = 22 profiles are used in the calculation of the depth-density profile? If the entire 43 individual profiles are used, it would act to minimize the standard deviations of the profile. Are the "43 NP profiles" in line 89 all from traverse T2?

- The authors comment that the picked layer depths (Lines 142-143) do not necessarily associate with a peak in the density profile yet argue that density-driven dielectric contrasts form the radar reflection horizons. Do the authors have any insight into why that might be? Some further discussion of this would strengthen the paper.

- Uncertainty Analysis: The authors clearly put in a significant amount of time designing the uncertainty analysis, which is quite commendable and appreciated. Some additional details would help clarify the exact plan since the authors produce two estimates (Fig 4 a and c), and it become unclear which is being used in what way. It is not entirely clear why there is a “picking” uncertainty in the age uncertainty as well as an evaluation of the standard deviation of the ages in Table 2. Wouldn’t any uncertainty in picking layers manifest in the numbers in Table 2? There is also uncertainty in the NP dating; perhaps this is what the +/- 1 year uncertainty is meant for? What does the small-scale variability in snow accumulation look like? Can comparisons be made from over 2 km away? Perhaps a good alternative would be a simple weighted mean of the age by distance (as well as standard deviation). Please also clarify that in Eqn 5, the “spatial” component refers to the density profile. Finally, is it justifiable to assume that the spatial errors in density are uncorrelated with depth? While firn depth-density is noisy with depth, there is no reason to expect that under different accumulation/temperature regimes, there will be large biases between measurements (that will accumulate with depth, rather than cancel out).

Minor Comments

- Some description of the range of "N" in Section 2.4 would help clarify the robustness of the methodology. Maybe the authors could add the N to Table 2. Also, be consistent with significant digits. Why do some have 2 decimal places and others only one? Based on the remainder of the text, it looks like the standard is just 1 decimal place. Also, please explain the “comment”s in the Table 2 caption.

- Is the uncertainty in the depth of the tracked layer in determination of the age uncertainty accounted for? From Line 148, it sounds like only a standard deviation of points is used. Additional uncertainty is imposed from the translation of the uncertainty in depth to an uncertainty in age based on the slope of the depth-age profile.

- For Figure 4, please indicate that the darker shades of grey refer to a higher density

of layers. Also, the caption should have more information. It’s difficult to attribute what calculations are made in Figure 4 c,d. Perhaps clarify that the “Spatial” error is in reference to the variability in density.

- Several studies have shown that RACMO is biased (often too low accumulation in the interior), so is it sufficient to take the RACMO values at face value when adding them to the regions that you cannot resolve. Perhaps a correction could be made to the RACMO data first using overlap between the radar estimates and the model over the region of overlap.

- The uncertainties provided are found to be larger than in ME14, which is expected as the authors note. From the text, the temporal error from this work is 1.4 y / 10.1 y = 13.9%, which indicates that the smallest error for any radar-derived SMB estimate is effectively 14%. In ME14, the temporal uncertainty is 1 y / 25 yr = 4%, suggesting that the temporal uncertainties are much lower in ME14. That reduction is likely due to the robust dating techniques used on the firn cores used in that study. Based on this alone, it is not clear how the shorter time window and larger dating uncertainties in this analysis do not at least account for a substantial amount of the increased basin-wide uncertainty values found in this work. Locally, it does appear that uncertainties from the kriging technique are much larger in this work than in ME14; however, they appear smaller than some of the uncertainties (say, in the southernmost reaches of the catchment) than in ME14. The total ME14 basin-wide uncertainty is 9% (6.8/78.3), whereas this work is 24% (19.2/79.9). Adding an additional 10% temporal uncertainty to the ME14 estimates puts values at 19%. Therefore, the likely impact of the kriging technique is on the order of 5%. This is a very simplistic take but should be robust from an order of magnitude perspective.

Minor Edits

Line 4: consider replacing "allowing" since "allow" is used in the previous sentence Line 5: add a comma after "scheme" Line 6: replace "the same main" with "similar"

Line 17: remove "the" in front of "upwelling" Line 18: consider replacing "stimulating" with "initiating" Line 28: replace "in particularly" with "in particular" Line 29: clarify what is meant regarding the sentence starting with "Basin wide mass..." It is unclear what it means in this context and might require a citation. Line 30: replace "in the following" with "hereinafter" Lines 32-33: remove "the" before "logistical" Line 33: change to "dielectric properties of snow and firn" Line 37: replace "In the following," with "Hereinafter," Line 44: replace "trace" with "track" Line 48: replace "self-intersections" with "crossovers" Line 50: change "measurement" to "measurements"

Note, there are several minor fixes needed beyond section 1, which will require further refinement by the authors.

Figure 3: add a righthand y-axis with depth equivalent.

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