The Cryosphere Discuss., 8, C326–C332, 2014
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The Cryosphere Discussions

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Interactive Comment

Interactive comment on "Constraining the recent mass balance of Pine Island and Thwaites glaciers, West Antarctica with airborne observations of snow accumulation" *by* B. Medley et al.

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Received and published: 27 March 2014

Review of: Constraining the recent mass balance of Pine Island and Thwaites glaciers, West Antarctica with airborne observations of snow accumulation

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General comments: In this manuscript the authors estimate the mass balance of Pine

Island and Thwaites glaciers by comparing surface accumulation estimates with estimates of ice discharge. Accumulation estimates for the glaciers is derived from airborne radar surveys tied to a dated firn core from Pine Island Glacier (PIG). The method of estimating surface accumulation employed by the authors is widely accepted as an accurate proxy for large scale accumulation variation. Ice discharge estimates provided in the manuscript are based on radar estimates of ice depth, satellite estimates of surface elevation, and surface velocity estimates from InSAR and speckle tracking. Regional average accumulation rate maps are derived from interpolation of inline data. Elevation-dependent accumulation rate derived from this study is compared to relevant climatologies and reanalysis and climate models. A comparison of the gridded accumulation product and the widely-used RACMO2 accumulation map for the region is also included in the manuscript. The interpolated average regional accumulation maps are used in conjunction with the catchment scale ice discharge estimates to constrain time varying changes in the regional mass balance for five catchments in the Thwaites/PIG area. Errors for each measurement are assessed as are errors for the raw and interpolated data products. Also, the methods used in this study are explained thoroughly and precisely, I compliment the authors on their work.

Overall, this is a well written manuscript with scientifically interesting results that show significant deviation from the model based estimates of the large scale accumulation pattern in the region. The method of determining spatial variations in accumulation rate from airborne radar employed in this study is appropriate and fairly thorough. The upscaling of the accumulation estimates from the radar profiles to a gridded accumulation product for the region is carefully and thoughtfully achieved. The limitations of the methods as well as the product are discussed which adds validity to the final estimates of the catchment mass balance.

The only major concern that I have relates to what I find to be a relatively small error assigned to the accumulation estimates derived from the airborne radar data. The source of my concern is the constant density profile used to determine the traveltime/depth relationship of picked internal horizons as well as the total integrated mass above the horizon. I think that the justification of the single density profile is valid, as is the justification of the error assigned to the accumulation estimates, however, I do not think that the 1 standard deviation error estimate attributed to the density profile and propagated through the twt/depth and cumulative mass/depth calculations (figure 3) is conservative, as the authors claim. As I argue in my comment on Section 3.4 below, just by taking into account the variation in mean annual temperature over the region the calculated error in the density profile is greater than the 1 standard deviation estimate given by the authors, potential errors from initial surface density and annual accumulation rate are greater still. I would like to see a more conservative error estimate given to the radar derived accumulation estimates, especially in light of the large region surveyed in this study.

That being said, I think that this is a quality paper and I would recommend publication after addressing the following comments:

Section 1: Page 956 Line 19-21: Here the authors correctly state that "in-situ measurements are inadequate for mass balance studies because recovery over inaccessible regions, such as highly crevassed areas, is not possible". This implies that airborne radar surveys are appropriate for highly crevassed areas. However, these regions will also have large disruptions in internal reflection horizons (i.e. vertical discontinuities in layer TWT, highly dipping layers over snow bridges, reflection hyperbolas from crevasse walls, etc.; in general these regions are a total mess in ground based radar measurements). Please explain how a single horizon can confidently be tracked through these regions, especially as it pertains to an along track spatial sampling rate of 500 m.

Section 3.1: Page 960 line 7: Please be specific as to which cores were used to determine the ages of the mapped internal horizons.

Section 3.2: Core sites were selected based on the radar data (Page 960 lines 11-12). Please specify what criteria were used in site selection for the cores. Presumably, the criteria for the site selection affects the range of variability observed in the core density profiles. Also, the cores were obtained the year after the radar survey (Page 960 line 12-13) and the vertical resolution of the radar is too coarse to image annual stratigraphy (Page 959, line 4-5). How were the horizon depths corrected to adjust for the accumulation during that year?

Section 3.3: The sentence of this section states: “Spatial variation in the depth to a given horizon is a consequence of variations in the accumulation rate”. The structure of this sentence indicates that the only factor in variations in layer depth is accumulation, which is not true. There are other factors that influence the depth to a given horizon which include lateral variations in vertical strain rate and lateral variations in firn density. Please change this statement to better reflect the complexity of the problem.

Section 3.4: The Herron and Langway model is fit to the retrieved density profiles (Page 959 Line 20-24). This steady-state model has three fitting parameters: 10 m temperature, accumulation rate, and initial (surface layer) density. The fit to this model is then used to calculate the depth to the internal reflection horizon and the mass of the overlying firn. The error analysis described in this section is appropriate and very reasonable for the data presented in this paper however, I take exception to the statement that the error estimate is conservative. Calculated depths for layer H1 range from 4.3 m to 36.9 m (Page 960 line 1), calculated accumulation measurements range from 0.13 to 1.37 m w.e. yr-1, and average surface temperature (equivalent to the 10 m temperature in the H&L model) likely ranges by at least 15 degrees C (as indicated by the ALBMAP gridded data for the region) over the ~1700 m elevation range sampled by the airborne survey; any one of these large variations will cause accumulation estimates based on the H&L curve used in the study to vary by more than the estimated 1 std. used as error bounds in the paper. I have uploaded a figure which shows the variation in density profile calculated with the H&L model for the ranges of the accumulation quoted in the paper (red dashed curves), measured surface density variations (350-450 kg m-3) taken from figure 3 in the paper (blue dashed curves), and a 10 degree mean annual

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surface temperature variation (black dashed curves); the solid black curve is calculated with the values for accumulation and surface density given in the paper and a mean annual temperature of -23 degrees C. The point here is that the range of values given in the paper do not support that the error estimate given in the paper is conservative, it is more likely a very non-conservative estimate of the errors inherent in the method.

Section 3.6: The first sentence of this section indicates that the along track measurement interval is small even though the quoted interval is 500 m. This interval is small in relation to the size of the basin but it is very large compared to the Nyquist spatial sampling intervals for the radar frequencies used in the survey, which are much less than 1 m. I recognize the need for such large sampling intervals in airborne data acquisition, but 500 m sampling interval for data with a vertical resolution of 50-62 cm is not considered small in a geophysical sense. Please revise the statement.

Section 3.7: Equation 1: Is the assumption that all of the ice flow is due to sliding at the bed valid for the entire catchment? Would the final mass balance estimate change drastically if large portions of the basin were frozen to the bed? Please justify the pure sliding assumption and estimate the error added to the full mass balance of the catchment due to frozen-on portions of the bed.

Section 5: Page 975 line 18-20: This sentence needs a date range for the accumulation range, I assume that it is 1985-2009 but this should be stated explicitly here.

Table 4: The quoted value for the total mass balance for 2010 does not work out mathematically (257.4-158.5 does not equal 96.1). This erroneous value is also quoted in the text (page 976 line 24). Please double check the rest of the table values for similar errors and change any erroneous values in the text.

Figure 1: Many of the flight lines in this figure indicate that none of the three internal horizons are traced for portions of the flight line, yet they are tracked further along the profile. How can the H1 horizon, which varies with depth and absolute reflectivity, be positively identified over large data gaps and thus be considered isochronal on each

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side of a ∼50 km gap in horizon continuity? Please expand on how H1 was identified over gaps where neither H2 or H3 were tracked, specifically at the core site THW2010 which is not continuously connected to the PIG2010 core, yet the H1 horizon is used to compare ages in the two cores.

Figure 4: The solid red line that indicates H1 completely obscures the horizon in the data. Please change this line to a dashed line so readers can assess the continuity of the layer better. Also, please indicate where the H2 and H3 horizons are in these figures.

Figure 6: Two of the ITASE core locations are absent from this figure (01-6 and 01-3), please add them.

Again, I compliment the authors on a thorough and well written paper.

Interactive comment on The Cryosphere Discuss., 8, 953, 2014.

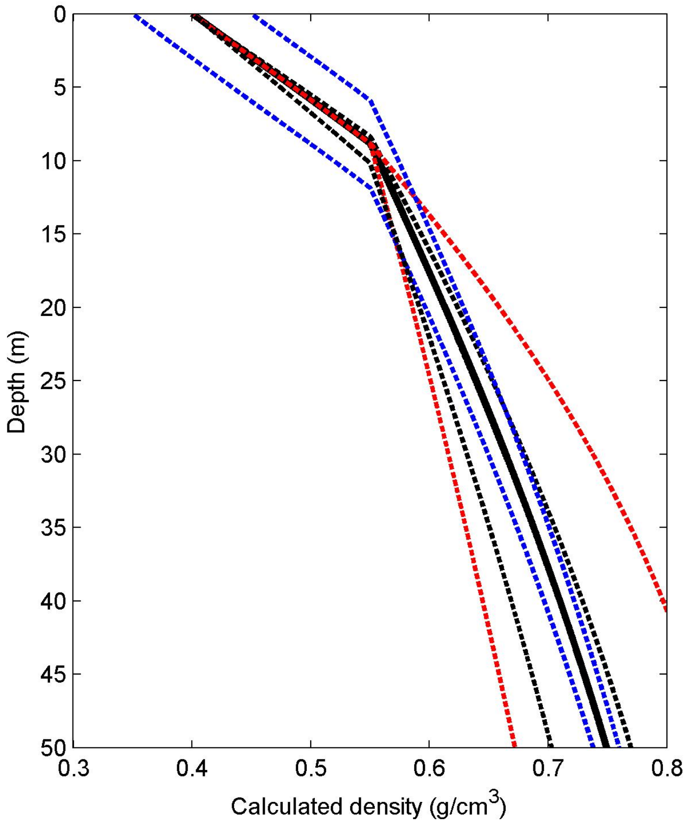

Fig. 1. This figure shows ranges of depth/density profiles calculated using the numbers given in the paper and the H&L model

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