Response to R. Arthern's review

My recommendation is that this paper should be published in the Cryosphere after a few minor modifications listed below. The study is a valuable assessment of the mass imbalance of the Pine Island and Thwaites glaciers in West Antarctica. It is now well established that these two glaciers and several others in the region contribute each year to rising global sea level. By providing an accurate picture of this contribution, and how it has varied over recent decades, this study represents a very worthwhile contribution to the glaciological literature.

Other assessments of the state of balance of this region have been published, using satellite altimetry, satellite gravity, and mass input-output methods, summarised in the recent IMBIE assessment (Shepherd et al. 2012). This study uses the input-output method, but is much more comprehensive in its treatment of the mass input from snow accumulation than previous assessments.

In particular a dedicated series of flights with airborne radar have allowed remotely-sensed observation of the accumulation rate along multiple flight lines, providing reasonable coverage of the whole drainage sector. The value of this is twofold. First, it allows the accuracy of earlier estimates of accumulation rate, either from meteorological models, or interpolated from field observations, to be assessed: a comprehensive comparison with many earlier studies is performed here. Second, the gridded accumulation map interpolated from the airborne radar observations is combined with velocity estimates from satellite interferometric radar to assess the mass budget of this region of Antarctica.

The result of the budget calculation is a time-resolved assessment of mass changes. A significant finding from this investigation is that while flow rates and hence ice losses are still much faster than they were in the 1990s, they have recently levelled off at these high rates of loss. These observations will be extremely valuable for testing models of glaciological changes underway in West Antarctica.

The paper is very clearly written, and the level of detail for ice cores, radar, and other techniques seems appropriate. The figures are good and the presentation is clear throughout the manuscript. I do think a number of additional points need to be addressed in the manuscript, and have listed these below.

We kindly thank R. Arthern for his extremely positive comments on the overall quality and scientific contribution of our work. We address his comments below in bold.

Minor revisions

The interpolation techniques could probably be improved upon. For instance, the separate regression and Kriging steps could be combined using Universal Kriging. More consideration could also be given to whether the residuals from the regression surface follow a Gaussian distribution or a log-normal distribution. For instance, it is quite possible that a different map would arise by Kriging residuals after a logarithmic transformation, and this might actually be a more appropriate procedure, depending upon the distribution of fluctuations in accumulation rate over short spatial scales. The scatter in the layer-to-layer regression clearly increases with accumulation rate (figure 5). This suggests that a logarithmic transformation of the observations similar to that used by Arthern et al. 2006 might be more appropriate. Otherwise observations of high accumulation rate from the tails of a log-normal distribution could be given disproportionate weighting in the interpolation. This would produce a map that is biased.

The reviewer brings up a very important point here concerning the normality of the measurements that form the basis of interpolation. When kriging accumulation rates, you must carefully consider the distribution as often there is a peak of relatively low accumulation rates with a long tail in the direction of higher rates. This skewed distribution can be "transformed" to a normal distribution by applying a logarithmic transform as done by Arthern et al. 2006 and mentioned by the reviewer here. We find that the residuals that formed the basis of our kriged map form an approximately normal distribution with a skewness of -0.4 (skewed left), which indicates there is some degree of skew but the distribution is approximately symmetric. If we were, however, to perform the kriging on the actual accumulation measurements (not the OLS residuals), log-transformation would be absolutely necessary.

The reviewer also suggests using Universal Kriging rather than Ordinary Kriging as employed here. Universal kriging assumes a general polynomial trend to the observations, whereas Ordinary kriging assumes a singular mean or constant model. While universal kriging was considered, we found the areas along the edges of the measurements were largely susceptible to the order of polynomial used, which sometimes generated spurious values. Because the goal of this work was to provide catchment maps of accumulation, we wanted to include some areas of the catchments outside of the main group of measurements. Extrapolating different order polynomials to these areas along the edges would result in a wide range of model values. Thus, we attempted to eliminate the sensitivity to the polynomial order by building a simple regression model that we feel provides a more realistic model outside of the main cluster of measurements.

The possibility that the accumulation rate map is biased by neglecting to transform the data should be acknowledged in the discussion section. The logarithmic transformation can only be defined for accumulations above some positive threshold, so is not appropriate for ablation or blue-ice regions, but that should not be a serious limitation here. Ideally, the Kriging procedure would be performed with and without a logarithmic transformation to assess the difference that this makes.

While this issue is mainly addressed above we note that it is likely that only localized areas of scour would result in a negative surface mass balance on our study area and that log-transform is thus possible in this wet region of West Antarctica. See above for our reasoning behind not using a log-transform on the measurements that provide the basis of interpolation. We added a sentence on Lines 248-249 to mention the distribution is approximately normal.

The assumption that ice velocity is constant with depth for flux gates upstream of the grounding lines is questionable for both Pine Island and Thwaites glaciers. There are regions close to the grounding lines of both glaciers that show significant basal drag, and this would cause at least some shearing within the ice. The consequences of this for estimates of discharge should be discussed, and an attempt should be made to place a bound on the error in flux estimates from vertical shearing.

We computed the ratio of column averaged to surface speed near the Thwaites grounding line and obtained typical values of greater than 0.99 (often above 0.995), indicating a bias of less than 1%.

We have addressed this in the text by adding the statement: "Our velocity measurements are made at the ice sheet surface but we assume they are equivalent to column average velocity due to the high-degree of sliding at near the grounding line. Based on analysis of estimated deformation velocities that are internal variables of a temperature model (Joughin et al., 2009), any biases introduced by this assumption are less than 1% and are not included in our error analysis."

It would be good to provide more glaciological context for the observations. A similar rapidly accelerating loss followed by a plateau of sustained high loss has been seen in several model simulations of this sector of Antarctica (e.g. Joughin et al, 2010, Favier et al., Nature Climate Change, 4, 117–121, 2014, doi:10.1038/nclimate2094). It would be worth spending some time discussing this observation in the light of the model simulations. For instance, did the rapid acceleration of losses coincide with the retreat of the grounding line of Pine Island into much deeper water?

The main focus of this paper was on generating the accumulation map for the purpose of determining the mass balance. We do agree with the reviewer that we should at least mention the relationship between the measurements and models and include a sentence in the discussion: "Several model simulations from this region (e.g., Joughin et al., 2010; Favier et al., 2014; Joughin et al., in press) exhibit a similar pattern of accelerated loss followed by a period of sustained and stable high loss." The paper in press compares the measurements from this work with model simulations. Additional discussion of the models would be outside of the scope of this paper.

Other than these minor modifications, which I think should be addressed before publication, the paper represents a very valuable study of glacial change in this sector of Antarctica.

We would like to thank the reviewer once again for his insightful comments and overall positive review of our paper.

References

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