The Cryosphere

Discussions

EGU

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Interactive comment on "Antarctic Ice Shelf Thickness Change from Multi-Mission Lidar Mapping" *by* Tyler C. Sutterley et al.

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Received and published: 19 February 2019

This manuscript reports on estimates of thickness change and basal-melt rates along airborne survey lines over West Antarctic and Antarctic Peninsula ice shelves. These estimates were derived from lidar measurements (of surface height change) obtained from NASA's airborne campaigns between 2002 and 2015, combined with available surface velocity data from MEaSUREs/NSIDC, and surface-mass-balance and firn state information from models (RACMO2.3, and a firn-densification model). The manuscript focuses on the methodology to invert height-change measurements from airborne lidar to basal-melt estimates in a Lagrangian framework. Finally, a brief discussion on the Lagrangian vs Eulerian approaches is presented, as well as putting in context some of the ice-shelf melt-rate values obtained. I believe the results of

this manuscript are of great value for comparing and calibrating satellite-derived estimates of ice-shelf thickness change and melt rates. The authors put considerable effort to integrate all available/usable NASA airborne lidar data over the West Antarctic ice shelves. While these data set is quite sparse (only available along flight lines and with a few repeats), there are still very little data available to compare against the vast amount of satellite measurements, which makes this work of particular interest to the remote-sensing community. I have, however, several questions and suggestions that I would like to see addressed prior considering publication (see comments below). Overall, the manuscript is well written and the figures are of good quality.

Thank you. We appreciate the thoroughly beneficial review of our manuscript. We address your comments point-by-point and update the manuscript accordingly.

1   General comments:

I feel a thorough error assessment on derived melt-rate estimates is lacking. Given that, as mentioned in the manuscript itself, this set of estimates is expected to serve as a reference for published and future (e.g. from ICESat-2) satellite-derived estimates, I would expect a more comprehensive error assessment: How close to (available) in-situ measurements are these values?

We expanded upon our error calculation in the manuscript. There are tide gauges around Antarctica that are used for validating the CATS2008 model. The 5.5km SMB models are validated against Operation IceBridge snow radar observations, satellite melt observations, and and in-situ observations (Kuipers Munneke et al., 2017; Lenaerts et al., 2018).

What are realistic confidence intervals given that some of the information comes from models?

This is an excellent question. We assume a 15% uncertainty in surface mass balance and firn height following estimates from Kuipers Munneke et al. (2017). Tidal uncertainties are estimated using the constituent RMS values from King et al. (2011). Uncertainties in flux divergence were estimated using annually resolved velocity maps (Mouginot et al., 2017a) and uncertainties in Bedmap2 ice thickness (Fretwell et al., 2013).

How sensitive are the estimated melt-rate values to unaccounted processes (due to lack of data or knowledge)?

Great question. Because the ice shelves are largely in hydrostatic equilibrium, any uncertainty in terms of elevation will be magnified by approximately 10× in the final estimates of thickness change and basal melt rate.

Some of the short-time-scale (2 to 5 years) estimates are likely subject to the large interannual-to-decadal variability characteristic in the AS-BS sector (e.g. Paolo et al. (2015)). For example, it has been shown that even ICESat-derived estimates (5-year period) can disagree substantially from longer-timescale averages (as those derived from radar altimetry). In many cases, the ICESat short time span (Pritchard et al. (2012); Rignot et al. (2013)) overestimate the underlying decadal trend, simply because their estimates are focusing on the more variable short-term scales.

Absolutely. At present, records from laser altimetry are far less compete than records from radar altimetry in terms of temporal resolution and duration (Paolo et al., 2015; Adusumilli et al., 2018). However, laser altimetry datasets have more accurate surface determination and can more accurately track over regions of abrupt topographical change. ICESat-2 should provide a valuable extension to the laser altimetry record and help separate short-term oscillations with long-term change. We expand upon these points in the discussion.

Substantial (and important) information on the methodology is being introduced in the

Discussion section of the manuscript. I understood some aspects/limitations of the methodology only after reaching the discussion page (which is the final portion of the Main text).

Good point. Text and figures have been reordered for improved continuity and presentation.

Can direct comparisons with previously published estimates be made for some locations (using, for example, Pritchard et al. (2012); Rignot et al. (2013); Paolo et al. (2015) and Adusumilli et al. (2018)? This would be very valuable and could motivate good discussion regarding discrepancies and/or similarities.

Great point. We have added a direct comparison with the results from Adusumilli et al. (2018) and have added points emphasizing this purpose of our dataset. We did not compare with data from Paolo et al. (2015) as the publicly available data is for a different time period. We did not compare with Pritchard et al. (2012) as the data is not provided in a compiled form. Rignot et al. (2013) do not provide publicly available data.

2 Specific comments:

p2, l3-4: "accelerated 2 to 8 times their previous flow rates"...Please define "previous", i.e., when those measurements were taken (right before 2002, or five/ten years before)?

Great point. Added that the before and after measurements were taken in 1996 and 2003. "In 2003, a year after the collapse of the Larsen B ice shelf, some tributary glaciers draining into the Weddell Sea from the Antarctic Peninsula were flowing 2–8 times their 1996 flow rates (Rignot et al., 2004). These glaciers continued flowing at accelerated rates years after the collapse (Rignot et al., 2008; Berthier et al., 2012)."

p2, l5: "surface thinning"... Are you referring to thinning of the firn layer (i.e. densification), which I don't think any of the provided references support this? Or perhaps you mean "surface lowering"?

Clarified to mean "dynamic thinning" as noted in Pritchard et al. (2009) and Flament and Rémy (2012).

p2, l7: What is an "internal change in ice dynamics" (as opposed to "an external change")?

Changed to "The dynamical change of these glaciers..."

p2, l8: ocean melt → ocean-driven melt

Done. Thank you.

p2, l25: "over Pre-IceBridge and NASA Operation IceBridge campaigns is shown"... Do you mean "prior to and during NASA Operation IceBridge campaigns is shown..."?

Changed to "The spatial coverage of each instrument in Antarctica for the campaigns prior to and during NASA Operation IceBridge are shown in Figure 1." Thank you.

p2, l27-28: What exactly do the 'converted' heights represent? Height w.r.t. an ellipsoid model or w.r.t. a geoid model... it seems you are tracking deviations from the

geoid, and why you need this conversion? Perhaps to invert for thickness/basal melt, but it is not clear at this point in the text.

Good point. Changed to "In order to track changes in ice shelf freeboard, the ellipsoid heights for each instrument were then converted to be in reference to the GGM05 geoid using gravity model coefficients provided by the Center for Space Research (Ries et al., 2016)."

p3, l7: What is "the scale of the individual triangular facet"?

Added that an individual triangle is ∼10–100m.

p3: On "Tidal and Non-Tidal Ocean Variation": Armitage et al. (2018) showed substantial sea-level anomalies (changes w.r.t. mean sea level) around Antarctica: about 3 cm at seasonal scales and 5 cm associated with the ENSO cycle. How will these translate to/impact the derived ice-shelf height changes? At the very least, these should be accounted for in the error budget. Note that these SLAs around Antarctica could not be precisely measured until only recently (e.g. Armitage et al. (2018)). What precisely are the "Non-tidal sea surface anomalies over ice-free ocean points", i.e., what process are you removing with the CMEMS product? Is this accounting for spatially variable sea-level rise? For example, Paolo et al. (2015) corrected for rates of sea-level change around Antarctica varying from 2 to 4 mm/yr (compared to the global mean of ∼3 mm/yr)

Good point. Paolo et al. (2015) used the same dataset from AVISO in their study (described in their supplementary materials). We clarify that the sea surface anomalies removed are local sea level change, which will include long-term sea level rise and inter-annual fluctuations. We add the sentence "Regional sea levels fluctuate due to changes in ocean dynamics, ocean mass, and ocean heat content (Church et al., 2011; Armitage et al., 2018)." We also include that the sea surface anomalies are added to estimates of mean dynamic topography, which are the mean deviations of the sea surface from the geoid. "The non-tidal sea surface anomalies are added to estimates of mean dynamic topography, which is the mean deviation of the sea surface from the Earth's geoid due to ocean circulation."

p4, l8-10: What's the relevance of "highly complex topography of mountains and glacial valleys" if you are working over (relatively flat) ice shelves? I'm saying this because I haven't seen a comparison between the 27km and 5.5km SMB models against in-situ measurements specifically *over* the ice shelves, to be convinced that the higher-res product does provide a more accurate representation of SMB state over flat surfaces.

Fair point. While there is little difference for the ice shelves in the Amundsen Sea, there are some substantial differences for the ice shelves in the Weddell Sea. The major difference is how well the topography of the peninsula is resolved at 5.5km versus 27km. Resolving some downstream effects within the climate model requires the highest-possible spatial resolution topography (Datta et al., 2018). Added "The higher spatial resolution topography improves the modeling of wind-driven downstream effects over ice shelves (Datta et al., 2018)."

p4, l13-14: I'm confused here: "The absolute precision of the RACMO2.3p2 model outputs has been estimated...", are you referring to the latest high-res model (the 5.5 km)? If so, why is the reference from 2006 (I assume they did not have

the high-res model back then)?

The van de Berg et al. (2006) citation was for the method used for evaluation of the RACMO2 models. We updated the sentence to say that it is "following Kuipers Munneke et al. (2017) and Lenaerts et al. (2018)".

p5, l3-4: What is "basal thickness change rate"? Changes in ice-shelf thickness due to mass loss/gain at the bottom? Or...

Correct. It referred to changes in ice-shelf thickness due to losses at the base. We changed the maps to use two separate colorbars and show basal melt rates in meters of ice per year.

Fig 10: "The elevation change rates shown here are not corrected for oceanic or surface processes and are not RDE filtered"...Why not?

Fair question. The original intent was to only show the differences due to the processing method. We updated the figure to correct for ocean and surface processes and we noted that we have corrected for strain in the Eulerian-derived values. The data isn't RDE filtered in order to show the worst case of each technique (such as near the rifting that developed before the calving of the A-68 iceberg).

General comment: I don't know what 'basal thickness change means'...Thickness change solely due to basal mass change? Please be more specific/accurate.

Yes, this is what it referred to in the previous manuscript. All plots have been changed to show basal melt rates in terms of meters of ice per year.

p5, l33-35: Could the difference in melt rate near the grounding zone be explained simply by the (large) interannual-to-decadal variability in the AS sector (as shown, for example, by Dutrieux et al. (2014); Paolo et al. (2015); Jenkins et al. (2018))?

Yes. This point has been added to the text.

p6, l15-16: However, Lagrangian estimates miss the grounding lines due to the direction of ice flow from grounded to floating. That is, sampled sites near the grounding lines were previously over grounded ice, lacking the corresponding measurement pair for comparison. This limitation affects measurements downstream of the GL depending on time separation between data points and flow speed. Another limitation of the Lagrangian approach is the sparseness of the estimates (compared to Eulerian solutions) since not all measurements will have a matching upstream pair (as also demonstrated by Moholdt et al. (2014)). Further, in the case of airborne surveys where the flight segments do not cross the entire ice shelf, measurements on the downstream end of the transect will also lack corresponding matching pairs.

These are great points that we have been added to the methods and discussions sections.

" In order to minimize the possibility of co-registering measurements over ice shelves with measurements over grounded ice near the grounding zone or measurements over the ocean, sea ice floes and icebergs, we only include points that are on the ice shelf for both time periods using grounded ice delineations from Rignot et al. (2016) and Mouginot et al. (2017b) and ice shelf extents manually digitized from Landsat (LPDAAC) and MODIS imagery (Scambos et al., 2001)."

p6, l18-20: Substantial smoothing was required because the effect of ice advection and divergence was not corrected for. With high-quality velocity products available today (e.g. Rignot et al. (2017); Gardner et al. (2018)) the flux-divergence signal can and should be removed (or at least reduced substantially) from the basal mass balance estimates (see for example, Berger et al. (2017); Lilien et al. (2018); Adusumilli et al. (2018)).

Excellent point. We have noted this in the text.

p6, l19-20: "spatial smoothing [. . .] to filter out the effects of advection". . . This misleading. The smoothing is not targeting specifically the advection-related features, instead, is removing everything that falls within the cutoff frequency of the smoother.

Fair point. While one of the main noise sources for ice shelves are these advected features, it is absolutely correct that the filters were not specifically used to remove these artifacts. This portion has been removed.

p6, l32-33: I think a more comprehensive "update" (to Pritchard et al. (2012)) has already been presented (see Paolo et al. (2015)). . . or not?

Fair point. While they are based on different datasets (radar vs. laser), Paolo et al. could be considered an update to Pritchard et al.. This sentence has been removed.

p7, first para: The discussion on the limited velocity coverage back in time for Lagrangian estimation is important (modern Eulerian estimates also depend on the removal of the advection signal). I feel the authors should go beyond just discussing and try and quantify the effect (i.e. the contribution to the error budget) of potential changes in ice flow. In other words, how sensitive are the melt rate estimates to changing velocity magnitudes? Typical magnitudes of velocity change can be taken from the literature for the few locations they are available (e.g. Mouginot et al. (2014)).

This is an excellent point. We include estimates of annual changes in flux divergence in our error budgets. Including time-variable velocity maps to advect the locations of the elevation measurements in our Lagrangian methodologies is the subject of future work.

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