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The Cryosphere
Discussions

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

Anonymous Referee #1

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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.

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? What are realistic confidence intervals given that some of the information comes from models? How sensitive are the estimated melt-rate values to unaccounted processes (due to lack of data or knowledge)?

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 (Prichard et all 2012; Rignot et al. 2013) overestimate the underlying decadal trend, simply because their estimates are focusing on the more variable short-term scales.

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).

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; Adusumilli et al. 2018)? This would be very valuable and could motivate good discussion regarding discrepancies and/or similarities.

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)?

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"?

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

p2, l8: ocean melt -> ocean-driven melt

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..."?

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.

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

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).

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 $\sim$3 mm/yr)

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.

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)?

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

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

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

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)?

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.

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, NSIDC; 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).

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.

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?

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).

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