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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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1 SUMMARY

The authors use airborne laser altimetry (from airborne topographic mappers (ATM)) over Antarctic Peninsula (AP) and Amundsen Sea (AS) ice shelves, plus models of surface mass balance and firn compaction, to measure ice shelf thinning rates and assign these rates to individual terms in the mass balance. The study is complementary to several previous studies that used satellite altimeters. The coverage of ATM is poor prior to Operation Icebridge (OIB). However, it has some advantages in terms of dedicated tracks, in particular allowing measurements to get close to grounding lines. It is therefore a valuable study, and dataset, to provide to the community.

Note that I have read the comments by Anonymous Referee #1 and agree with most of those, which I won't generally repeat. It seems unlikely that regional sea level trends could matter much, although it could amount to $\sim$5 cm of elevation in a decade. It's possible that general ocean variability that isn't corrected for (currently only tides and inverse barometer) is a bigger source of error especially given that the ATM missions are essentially instantaneous, and sparse in time.

We are really appreciative for the helpful review of our manuscript. In response, we have revised the manuscript and clarified some essential points. We completely agree that other sources of oceanic variability can influence the measurements over ice shelves. In the current and previous versions of the manuscript, sea level variations are accounted for using AVISO products distributed by Copernicus. The use of these sea level anomaly products follows the ice shelf work of Paolo et al. (2015).

2 GENERAL

1. I spent a lot of the paper being confused by the term "ice thickness change rates". This relates to the use of Lagrangian calculations. The authors explain why they use Lagrangian methods, which makes sense, although it often seems to lead to massive data loss: compare figure 1 flight lines with locations of ice thickness change on figures 5, 7, 8 and 9. However, Lagrangian methods are really just a tool to get the mass balance terms. The most important thing is whether the ice shelf is losing mass, and the spatial distribution of that loss, so that Eulerian variability is really what you want to report in terms of SMB, BMB and divergence.

Figures and text have been changed to use basal melt rates (in terms of meters of ice equivalent per year). The data is spatially sparse over ice shelves regardless of reference frame (especially in the pre-IceBridge era). After 2009, it is possible to have nearly annually resolved estimates of ice thickness change along the flight lines for some ice shelves. Idealistically, reporting Eulerian variability would be preferable over Lagrangian variability. However, substantial smoothing or averaging is required with Eulerian-derived estimates to reduce the impact of noise, and thus Lagrangian-derived estimates can provide more accurate solutions if the spatial coverage isn't comprehensive.

If you agree with that, then the important "ice thickness change rate" is Eulerian, which you get back from Lagrangian by adding back in the strain thinning and advection terms. (If they appear to be changing, that's relevant too.) The simplest approach to clarify what you're reporting would be to introduce Eulerian and Lagrangian rate symbols early (d/dt and D/Dt), then use the symbol rather than the words. Every time I see capital 'D', I'll know it is Lagrangian.

Nomenclature has been updated.

2. It is strange that Results are presented first, then back to Methods, as far as figures go. Given how much the data distribution thins out from Fig. 1 to the Lagrangian maps, the first thing to do would be to determine if Lagrangian is a good method. Potentially, you are better off with averaging of a lot of noisy Eulerian measurements rather than far fewer cleaner Lagrangian values. I'd move Fig. 6 to Fig. 5, demonstrating the value of along-flowline repeat ATM, then next I'd have something like Figure 10 to make points about the value of Lagrangian vs Eulerian. You need to also check that you are comparing the same things here: results from Eulerian TINs should be the same average as Lagrangian TINS provided the Lagrangian values have been re-corrected for advection and

strain.

Thank you. The figures and text have been reordered for improved continuity. We include that the Eulerian-derived values are corrected for the effects of advection following Moholdt et al. (2014). As mentioned in the manuscript, we have results for all available Operation IceBridge data in an Eulerian reference frame using a similar TINs-based methodology. You are absolutely correct that there is more available data when computed in an Eulerian frame of reference; however, the data is still spatially sparse over ice shelves (particularly in the pre-IceBridge era). Mission priorities have limited measurements over ice shelves until fairly recently (when Mag/Grav measurements have enabled improved estimations of sub-shelf bathymetry). The strength of the airborne laser altimetry data lies in its accurate measurements at relatively small spatial scales compared to radar altimetry data, repeatable processing methods, and ability to follow glacier flowlines.

3. The authors should look at another Cryosphere Discussions paper by Shean et al. (2018) https://www.the-cryosphere-discuss.net/tc-2018-209/, where Pine Island melt rates are assessed using high-res image-based Lagrangian processing.

   We are looking forward to the publication of the Shean et al. paper as it is a very complementary work that uses an independent dataset. As the paper is not presently through peer-review, we have only included citations to the Discussions paper in anticipation of a future acceptance.

4. The authors should probably compare their results for Larsen C with the ATM measurements presented in Adusumilli et al. (2018).

   Done. Figure and text have been added.

5. Overall, I think this paper fails to exploit the key features of ATM vs satellite-based

products. Satellite altimeters and stereo imagery (Shean et al. (2018), and an earlier Dutrieux et al. (2014) paper), make the process easier, but all satellite altimeters lack spatial resolution and radar altimeters struggle near grounding lines and other steep regions. Think about the new science that is available from a carefully compiled ATM data set where all the biases have been corrected for. If there is no new science, the data set is still valuable as it provides independent estimates to compare with the satellite-derived values. In this case, the most obvious value of the data set is as intended for OIB: a continuation of the ICESat laser altimeter record. Why not look at ICESat data as a third, earlier period in the various plots that compare pre-2011 and post-2011 data?

We expand upon the purpose of this paper and the benefits of having an airborne lidar derived dataset for the validation of satellite datasets and model outputs. We are investigating processes that can be derived with the airborne dataset but are leaving that for a future work. A comparison of Operation IceBridge with ICESat laser altimetry is certainly possible if a compiled dataset was publicly available. We have added a comparison with the published radar altimetry estimates from Adusumilli et al..

3 MAJOR: SPECIFIC

p.1/l.2: See general comments. The reader needs to know whether you mean Lagrangian or Eulerian ice thickness change, and if the Lagrangian estimates have been re-corrected back to Eulerian.

Emphasis has been added to clarify that these are Lagrangian estimates.

p.1/l.8-9: Comments on Larsen C depend on the quality of the SMB and firn models. This sentence suggests that the ice thickness change really is DH/dt, not dh/dt.

Yes, most of the estimates in this work are Lagrangian-derived ice thickness change. Also, in any reference frame, the conversion from volume to mass will be an important aspect. We added an additional sentence noting how SMB uncertainties will directly impact the basal melt rate estimate.

p.1/l.9-11: I don't think *you* show that Wilkins depends on "short time-scale and upper-ocean processes": the only evidence I see for this is citations to previous work.

Fair point. Attempting to quantify the effects of individual processes is the subject of some future work.

p.1/l.11-12: Again, this is where you'd be better off reporting dH/dt, even if you're deriving it via re-corrected DH/Dt. I was surprised that PIG was "thinning" by 40 m/yr, even close to the grounding line. The more important numbers are in comparisons: you want to show actual Eulerian thinning (dH/dt), BMB, and maybe the ice divergence term.

To clarify, we are reporting Lagrangian thickness change rates and melt rates of the Pine Island Ice Shelf and not Pine Island Glacier. Shean et al. (2018) present very similar numbers for the Pine Island Ice Shelf over similar time periods.

p.2/l.31: The Shean et al. (2018) TCD paper is another example of Lagrangian processing.

The Shean et al. paper is an excellent example of a similar methodology and a very complementary to our own work. Citation has been added.

p.3/l.27-29: I don't understand how you remove non-tidal ocean height change for ice-free ocean points from ice-shelf data. Extrapolate under the ice front? Do you get AVISO sea surface height all the way to the ice fronts at all times of ATM surveys, or does sea ice get in the way? What processes do you think the AVISO products are correcting for, or is this a coarse approximation for regional sea level rise?

Yes, it is simply an extrapolation and is a coarse approximation for regional sea level rise. The extent varies based on sea ice. We chose to use a measured estimate in order to include processes that can deviate strongly from the global ocean averages, such as steric effects and self-attraction and loading effects. The use of this correction follows Paolo et al. (2015) that used the same dataset.

p.4/l.20 ff: You need to explain all the terms in this equation immediately.

Great point. We've expanded upon each of the terms.

p.5-6 (Results): This would be clearer if you used sub-headers for each ice shelf that you are considering: Larsen C, Wilkins, Pine Island, and Dotson/Crosson. Also, this is a critical place to use symbols regarding ice thickness change: is it Eulerian dH/dt, Lagrangian DH/Dt, or Lagrangian-derived Eulerian dh/dt?

Great suggestion. The results section has been now partitioned into subsections. We added emphasis to note that all results are in a Lagrangian reference frame and the use of the Eulerian frame was for comparison purposes only.

p.5/l.27-28: Sentence starting "These periods" suggest that RACMO2.3p2 ASE055 is only available for specific periods, which then determine the breakdown of ATM into different epochs. Is this true? Regardless, the reader needs to know the period for which this high-res surface processes model is available.

Great point. We added the range of each climate simulation. "We use 5.5km horizontal resolution outputs from a 1979–2016 climate simulation of the Antarctic Peninsula (XPEN055, van Wessem et al., 2016) and a 1979–2015 climate simulation of West Antarctica (ASE055, Lenaerts et al., 2018)."

p.5/l.32-33: Rignot and Jacobs (2002) is not the right cite for "highest impact on glacial flow dynamics". They just assume that and use it to justify looking at melt rates near the grounding line. There are many more recent papers that might be relevant, e.g., Walker et al. (2007), Gagliardini et al. (2010), probably others.

Fair point. We include citations to updated and more relevant work.

p.6/l.3: Rignot (2002) seems like a strange single choice for citation here.

We expand upon this sentence and add reference to more updated work.

p.6/l.4-13: The Dotson/Crosson data are incredibly sparse, which I assume is a consequence of using Lagrangian processing given data density on Fig. 1. So (a) is this a place where higher noise in Eulerian would have been better? (b) Maybe you haven't enough data to learn whether conditions are different from the ICESat-era results of Khazendar et al. (2016)? This points again to using the ICESat-era results as a natural comparison for the more recent ATM.

Spatial sparseness at Dotson/Crosson is largely due to the availability of coincident data and not as much of a function of Eulerian versus Lagrangian reference frames. The included plot shows the same time period as the figure in the main text, but uses an Eulerian approach. Khazendar et al. (2016) used the ICESat/OIB data from Sutterley et al. (2014) as an estimate over the grounded ice and estimates from radar depth sounding and ATM over ice shelves. Creating estimates from ICESat would certainly be possible but we believe outside the purview of this paper.

p.6/l.17: The statement "Our Eulerian approach" seems to contradict everything you've said about using a Lagrangian approach. This comparison should be much earlier in "Methods", then you could mention "We began by calculating. . . using three approaches, . . . , . . . , and . . . , applied to Larsen C. Results

(Fig. X) demonstrate that. . . " Just make sure the figure really does compare Eulerian with re-corrected Lagrangian, or advection-and-strain-corrected Eulerian with Lagrangian.

Text and figures have been reordered for improved continuity and for the clarification of points. We also clarify that the Eulerian values are corrected for advection and strain effects.

p.6/l.26-27: Dotson/Crosson data are extremely sparse, and it isn't at all clear that Lagrangian methods are the best approach here.

The lack of data at Dotson/Crosson is more due to the lack of coincident flight lines over the ice shelf. This point has been emphasized.

p.6/l.29-30: the statement "would likely not be representative" is probably true, especially for Dotson/Crosson, but needs to be justified, e.g., on the basis of data sparseness.

Point added.

p.6/l.30-32: It isn't really obvious why you need a DEM, specifically from photogrammetry, to use ICESat-2 for dH/dt. It helps with the advection terms and Lagrangian TINs, but maybe you need to set up the idea better, along the lines of "The Lagrangian method is strongly dependent on a detailed understanding of surface topographic features being advected by the ice flow. . . "

Absolutely correct that digital elevation models are not necessary for deriving elevation change with ICESat-2, particularly in an Eulerian reference frame. Lagrangian reference frames are more difficult, particularly if the ice shelf flow is roughly perpendicular to the satellite tracks, such as in the Antwitharctic peninsula. DEMs would improve the tracking of ice parcels, and decrease data "loss" between different satellite tracks. These points have been clarified in the text. Thank you for the comment.

p.7/l.13-22: (a) This section does not flow well, but it does raise two issues that you haven't really explained well up to now:

1. Pros and Cons of radar vs laser. The goal, probably, is change in vertically integrated *mass* (or ice-equivalent thickness). With laser, you get true surface height really well, but conversion to mass depends on the firn model. If the snow layer is lighter than you thought, you infer too much mass. With radar, it is complicated by penetration (and footprint size), but on the other hand maybe that's good because the inferred reflecting surface is below the lightest snow. However, you still need the model of firn compaction below the reflector.

   Good point. We expand upon the strengths and weaknesses of each dataset. We include a more detailed description of the complications for detecting ice shelf surface height from each dataset.

2. The study hasn't really been set up as well as it could have. This gets back to: Is there really new scientific insight here, or is the goal mainly to provide an independent data set that is of specific value in comparing with satellite-derived ice-shelf changes, specifically laser-based? Either way is good for a paper, with the latter being the justification for OIB anyway. A clearer goal, stated early, might help organize the paper so that results are written around that goal. At the moment the paper reads like you're identifying new science, but the Results section mainly relies on, or repeats, previous studies, just with a new data set. e.g., Adusumilli et al. (2018) reach the same conclusions regarding Larsen C, except they don't spend much time of the advection and-strain terms, but they do use ATM as validation. Wilkins is interesting, but why not compare ATM tracks with ICESat to get a better sense of pre- and post-ICESat behavior?

Fair point. We expand upon the justification of the research and the manuscript in the introduction. We clarify the purpose of the work and justification of using Operation IceBridge data. Creating a comparable dataset using data from the original ICESat mission is worthwhile and certainly possible; however, we believe outside the purview of this manuscript. We include a comparison with the Adusumilli et al. dataset over the Larsen-C ice shelf. We are working towards the overarching goals of ice-ocean interaction and the downstream effects on the grounded ice. We will also have more in depth interpretation of the results for particular ice shelves in future work.

4 TECHNICAL: SPECIFIC

p.1/l.19-20: (a) I think Rignot et al. (2013) just assumes that ice shelves buttress grounded ice, don't show it. You can't cite every paper that makes that claim. (b) Sentence starting "The thinning. . . " just repeats the idea of buttressing.

The buttressing effect of ice shelves is fairly well documented and we include citations to modeling efforts to quantify the effect.

p.2/l.1-2: Again, you're repeating the buttressing argument.

The purpose of this second sentence is to emphasize how changes in ice shelves affect the grounded ice.

p.2/l.19: Abbreviation "WFF" isn't used again, so not needed.

Done. Thank you.

p.2/l.32: Here you cite Rignot et al. (2017) for MEASURES, but on p.4/l.24 it is Mouginot et al. (2017).

This has been fixed. Thank you. The Mouginot et al. (2017) data was used to calculate deviations from mean ice flow.

p.4/l.28: This reads like the range of validity for hydrostatic is only the narrow band of 1-8 km from the grounding line. You mean that this region is *not* hydrostatic, but that the flexural boundary width is in this range.

Correct. The intent was to describe regions downstream of the 1–8 km wide grounding zones. Changed to "The ice thickness estimates are calculated assuming hydrostatic equilibrium, which should be valid for most areas downstream of the 1–8 km wide grounding zones (Brunt et al., 2010, 2011)."

General style, especially in Results: You make a habit of starting paragraphs with "Figure X shows. . . ". This sounds like you have a collection of figures to describe, rather than making figures to fit your narrative.

General style has been updated to first describe each ice shelf and introduce results.

General Style: "{Name} ice shelf" or "{Name} Ice Shelf"?

Updated. Thank you for the suggestion.

p.6/l.2: Why refer to Fig. 8*b*, specifically?

Fair point. This has been fixed. Thank you.

p.6/l.5: I think this means "two periods – 2002-2010 and 2010-2015 – are shown in"

Precisely. Thank you.

p.7/l.8: more precisely "maps of time-varying velocity"

Changed to "time-variable velocity maps"

p.7/l.9-12: Would be good to have cites to each of these products.

Citations added.

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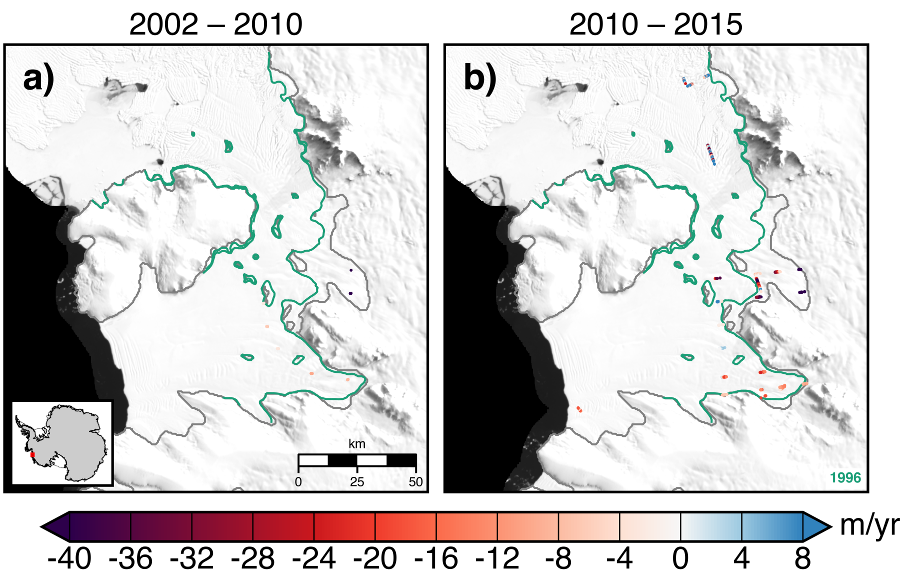

Fig. 1. Ice thickness change of the Dotson and Crosson Ice Shelves from a) 2002–2010 and b) 2010–2015