The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-186-RC2, 2019

The Cryosphere
Discussions

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

Anonymous Referee #2

Received and published: 3 January 2019

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 ~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.

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.

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.

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.

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

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

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., and an earlier Dutrieux et al (I think) 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?

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.

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.

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.

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.

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

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?

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

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?

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.

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. (2008), Gagliardini et al. (2010), probably others.

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

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.

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.

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.

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.

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 ..."

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:

(a) 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.

(b) 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?

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.

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

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

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

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.

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

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

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

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

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

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-186, 2018.