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The Cryosphere
Discussions

Interactive comment on "Elevated melt causes varied response of Crosson and Dotson Ice Shelves in West Antarctica" *by* David A. Lilien et al.

Anonymous Referee #2

Received and published: 9 February 2018

This manuscript presents a detailed analysis of the changes in speed and grounding-line position of two rapidly changing ice shelves, and their respective catchment basins, in the Amundsen Sea sector: Crosson and Dotson. The work combines a diverse set of measurements: satellite remote sensing data of horizontal velocities and elevation changes, airborne radar sounding for ice thickness, satellite imagery to determine ice-shelf front position, and published estimates of ice-shelf thinning rates. The authors then estimate the ice viscosity using a numerical model and perform an analysis of changes in ice-shelf flux, basal melt, and calving rates. Their results show that Dotson and Crosson ice shelves exhibit different dynamic responses to similar perturbations, and that is likely that both ice shelves were out of balance well before the beginning of

the satellite record (the 1990s).

General comments:

The manuscript presents a comprehensive study on the mass balance and velocity state of Dotson and Crosson ice shelves. The work provides some important insights into the origin of the current observed mass imbalance on these ice shelves and their respective catchment basins.

I couldn't find major issues with the work. Overall, the manuscript is well written, and the science is solid. I have, however, a few comments/suggestions that I think need further clarification and will improve the presentation of the work. These edits should be straightforward to incorporate.

In particular, the Summary section needs a bit more work. I think there should be a preference for the active voice in the 'Summary' (e.g. we analyzed, we found, we concluded, we showed), briefly stating the implications of your findings, as well as summarizing the path that leads to your conclusions. I feel this is missing here.

On the melt rate calculation; Fig. S1 shows melt rates estimates for 1-5 year time intervals. Since these are anomalies in basal melting, it is expected (in fact it has been demonstrated, e.g., Jacobs et al., 2013; Dutrieux et al., 2014; Christianson et al., 2016; Paolo et al., 2018) that melt rates are (highly?) sensitive to changes in ocean forcing in the Amundsen Sea sector, which fluctuates substantially at interannual timescales. So, I am unsure how much "weight" one can put on these (highly variable) short-term estimates of basal melt rates in the context of past (longer-term) dynamics of the ice shelf.

I am a bit confused about the fact that you used an average thinning rate for the entire ice shelf, which it might be fine for your polygon estimates, but then compare point estimates of basal melt against Khazendar et al. (2016). How meaningful is this comparison? If I understand correctly, the spatial variation in your melt-rate estimate is determined mostly by the flux divergence since you do not account for the spatial variability in thinning rates, is that right? If so, how significant do you expect the changes in dH/dt across the ice shelf to be compared with the spatial changes in horizontal velocity?

The title needs to be a bit more informative (Observed elevated melt? Over what period? Elevated relative to what? What kind of response?).

To facilitate the reviewing process, please use continuous line numbering throughout the entire manuscript, and number every single line.

Specific comments:

Page 1

Lines btw 5-10: "remotely sensed datasets". Which datasets?

Lines btw 10-15: "melt" => "elevated melt" or "melt in excess" (there is basal melt in the steady state)

Lines btw 20-25: "instability" => "internal instability"?

Lines btw 20-25: "are the dominant source of sea level rise" => "are currently the dominant source of sea level rise" or "are the dominant source of current sea level rise"

Page 2

Lines btw 20-25: "ice flux on ice thickness" => "ice flux across the grounding line on ice thickness there"

Lines btw 25-30: "deeper ocean waters are generally warmer in these systems". Careful here, this cannot be a general statement. In locations where CDW is present (not everywhere), there is more of this warm water reaching the deep grounding lines because the CDW is denser than surface waters. This is different from saying "deeper

waters are warmer".

Lines btw 30-35: "recent speed and thickness change" => "recently observed speed and thickness changes"

Page 3

Lines btw 10-15: "significant changes in the extent of Crosson". How significant? Can you provide a percentage of area change?

Lines btw 30-35: "not straightforward" => "challenging"

Page 4

Lines btw 30-: How is the uncertainty propagated for the values predicted by the quadratic function where there is no data at all? This uncertainty should increase (substantially) as the predictions get further away from the period constrained by data.

Lines btw 30-: Not sure I understand the following statement: "The results from this method more closely match the available ICESat-1 data than using the elevation and thinning rate from 2004 alone, as was done in Mouginot et al. (2014)". In what sense it matches better ICESat (2003-2009), which is outside of the extrapolated range (1996-2002)?

Page 5

Lines btw 5-10: "The thinning rates on the 10 grounded ice are more accurate since they lack tidal effects". This is not the main reason why altimetry thinning-rate estimates may be more accurate over grounded ice. Over (rapidly-changing) grounded ice the signal-to-noise ratio is much higher than over floating ice where the altimeter only "see" about 10% of the thickness-change signal due to hydrostatic balance. Also, tide models are reasonably good (outside of the grounding zone) at the current stage.

Lines btw 15-20: "This method reduces many of the artifacts that can occur when interpolating sparse ice thickness measurements while avoiding making any assumptions about the present state of balance (e.g. assumptions for mass conservation methods (Morlighem et al., 2011))". It is still interpolation, right? Meaning that regardless of any "smart" weighting, you don't have information in between the sparse thickness samples. The mass conservation approach uses additional information (velocities) to fill in those gaps. Bottom line, one interpolation method, another interpolation method... it is still interpolation.

Page 7

Line 1: "m_s is the basal melt rate". m_s => m_b

Line 5: I think a more appropriate reference for RACMO2.3 SMB is "Van Wessem et al. (2014)".

Page 8

Line 16-17: "but this choice should not have affected the overall spatial pattern". How sure can you be about this (i.e. what is the effect of many small artifacts on the overall result)?

Page 9

Line 9: "and any additional amount entering the shelves in each year". Suggest adding "by precipitation over the ice shelves" or "through ice-shelf surface accumulation".

Page 10

Lines 12-13: "would not substantially alter our qualitative conclusions". You can remove "qualitative" here.

Lines 8-9: "implying that we likely underestimate the error in our melt and flux calculations". Not necessarily. The discrepancy in the thinning rates on those studies is mostly due to some using a 5-year (laser) altimetry record while the other using an 18-year (radar) altimetry record. So, the different thinning rates are likely representing different timescales. For example, as shown more recently by Paolo et al. (2018), there are

large fluctuations in ice-shelf thickness at ENSO timescales ($\sim$4-5 years), a time span comparable to the ICESat period.

Page 11

Line 5: "ice that is thinner and cooler than expected" => ???

Page 13

Lines 13-14: "the downstream increase in thickness is large compared to the SMB". Can you provide the typical magnitude of SMB over this region (e.g. from RACMO) to put things in context?

Page 16

Lines 16-24: When estimating basal melt rates using ice-shelf surface height changes (e.g. estimates derived from altimetry), there is a substantial uncertainty associated with fluctuations in surface mass balance, which can have large amplitudes at seasonal-to-interannual timescales. Moreover, it has been shown that, often, increase basal melting and surface height change are out of phase (i.e. they can go in opposite directions) (e.g. Paolo et al., 2018).

Line 28: "similar" => comparable

Line 28: "changes in melt" => changes in basal melt

Line 30: "were associated" => by whom?

Line 32: "total basal melt was further increased" => by whom?

Please avoid the passive voice. It is not clear whether you are stating known facts (from the literature) or stating your conclusions.

Line 34: "A change in ocean forcing years or decades before 1974 likely led to in Dotson's imbalance in 1996". This is highly speculative, particularly regarding "a change in ocean forcing". It is OK to speculate but clarify (in the Summary) what led you to this conclusion.

Page 17:

Lines 4-6: Are you suggesting this as future work? If so, please clarify you are pointing the future direction of work needed; and justify why we would need such direction (i.e. what's the relevance in the context of understanding and predicting future ice-shelf/ice-sheet loss).

Supp. Page 2:

Line 18: "and we are unsure of the exact method used by Khazendar et al". Why don't you ask them?

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