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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.

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We thank the reviewer for a thorough review that has helped to significantly improve the clarity of the manuscript. Comments from the reviewer (bold) and our responses follow. We will upload the new version of the manuscript and supplement, with and without changes tracked, separately.

This manuscript presents a detailed analysis of the changes in speed and grounding 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 iceshelf 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.

We have changed the wording of a number of sentences in the summary to be active and to clarify what work we did and what had been done in prior work. Some examples are: "We find that thinning and speedup…", "These conditions lead us to speculate that…", "Our results indicate that…", and "We used a diagnostic ice-flow model to show that…"

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 spatial variation in our melt-rate estimates is almost entirely determined by the flux divergence; the only spatial variability in thinning we account for is slightly different rates for Crosson and Dotson. dH/dt (if the change is assumed to be all ice and not snow or firn) and horizontal flux divergence both map linearly into our estimate of the melt rate. Locally, horizontal flux divergence can vary between ïĆś100 m/yr, while local thinning rates found in Gourmelen et al. (2017) reach as high as 50 m/yr. Our spatially averaged estimates do not capture these peaks, but the estimates of Khazendar et al. explicitly measure thinning and thus, when corrected for flux divergence, ought to represent the anomalous melt at that point. In short, these methods are sensitive to different sources of error, and can provide information on different spatial and temporal scales, so we think that they are complementary and the comparison is worthwhile.

In response to these comments, and suggestions from the other reviewer, we have entirely re-worked the text surrounding this comparison (section 5.3). The section now

includes three paragraphs. The first discusses the limitations of Khazendar et al. study, and how our result complement theirs. The second paragraph discusses the uncertainties and limitations of the Gourmelen study, and why our results are still useful in light of this more spatially resolved estimate. The third reconciles our lower value peak melt with the prior estimates by discussing the effects of spatial averaging and spatial variation in thinning.

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

We have changed the title to emphasize the type of response discussed in the paper. It is now: Changes in flow of Crosson and Dotson Ice Shelves, West Antarctica in response to elevated melt

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

We apologize that this numbering makes reviewing more difficult, but we have adhered to the guidelines provided by The Cryosphere.

Specific comments: Page 1

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

Changed to “remotely sensed measurements of velocity and ice geometry”

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

Changed to “elevated melt”

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

Changed to “susceptible to internal instability triggered by increased ocean melting of buttressing ice shelves.” The instability generally requires some triggering (despite the potential for internal instability, these ice streams may have maintained their position for 1000s of years with no external forcing).

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"

Changed to "currently the dominant source of sea level rise"

Page 2

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

Done

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".

We have revised these sentences to avoid over-generalizing. They now read: "In the case of a retrograde bed, the deepening of the grounding line caused by ungrounding also increases melt because the melting point decreases with depth. For glaciers along the Amundsen Sea, this effect can be intensified because warm, dense circumpolar deep water generally intrudes at depth and results in elevated melt at deeper grounding lines (Jenkins et al., 2016; Thoma et al., 2008)."

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

Done

Page 3

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

We rephrased this to be more specific. The percentage change in area is deceptive because of extension of the middle of the calving front the absence of any large calving event during this period, so we calculated the absolute area change of the margins. The sentence now reads: "There were also significant changes in the extent of ice at the margins of Crosson (250 km2 of ice extent lost) and thus in the amount of contact with its sidewalls and with the tongue of Haynes Glacier (Figure 1b) through this period."

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

Done

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.

Added "While we are unable to formally calculate the uncertainty of the surface elevations produced by this extrapolation, we estimate it as 50% of the change from the earliest measurement (in 2003)."

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)?

Expanded this sentence to: "To assess relative to previous methods, we calculated estimated surface elevations for 2003-2008 using this quadratic function to test its ability to match the available ICESat-1 data. Residuals are smaller than those resulting from using a quadratic fit to the elevation and thinning rate from 2004 alone, as was done in Mouginot et al. (2014)."

Page 5

Lines btw 5-10: "The thinning rates on the 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.

Indeed, this effect is large. We have update the text to: "The thinning rates on the grounded ice are more accurate because the entirety of any change to thickness is manifest in the surface elevation while over floating ice 90% of the thickness change is accommodated through raising the ice bottom due to hydrostatic balance. Thus, we smoothed the thinning over the shelves for 10 km downstream of the grounding line to preserve continuity and reasonable surface slopes."

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.

We were primarily concerned with pre-empting the criticism that we should have been using a mass-conserving bed. We have updated the text to clarify that this is just a different interpolation method. We added: "though like other methods used to interpolate

radar data it still has high uncertainty due to the sparseness of the underlying radar profiles."

Page 7

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

Thanks, fixed.

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

We have updated the text and figure to use the 20% error that Van Wessem et al. estimate rather than that used in Depoorter. This sentence now reads "For the SMB, we use the annual mean for 1979-2013, which has an uncertainty of $\pm$20% (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)?

We cannot be positive, but this would generally be more worrisome if we had found areas with strengthening and weakening intermingled, where regularization may have forced an entirely different solution. Rather, we find areas in which we infer weakening, and expect that regularization would just diffuse these areas. We have updated the text to read "The lack of regularization may have concentrated the weakening or strengthening into smaller areas than would have been found with regularization, but any solution, regularized or not, likely would have to introduce weakening into these same areas in order to reproduce velocity field. Thus, the lack of regularization likely did not affect the overall spatial pattern of weakening."

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".

We have clarified the three categories of incoming flux that we are partitioning. This sentence is now “The incoming flux consists of ice-shelf surface accumulation and the flux across the grounding line; we partition the grounding-line flux into a steady-state amount (i.e. the accumulation in the catchment upstream) and any additional amount (in excess of steady state) entering the shelves in each year.”

Page 10

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

Done

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 (âĹij4-5 years), a time span comparable to the ICESat period.

Indeed, there may be real variability incorporated into these differences, but in the case of Dotson it is almost certainly error. Paolo et al. 2015 show the trend in thickness in the extended data, and it is very nearly linear in the longer timeseries. Since this longer record encompasses the time covered by the laser record, the factor of 2 difference in estimated thinning rate is not just a sampling effect on real variations, though perhaps some of it is. We have updated the text to reflect the possibility that a component of the discrepancies results from variability. The relevant text now reads: “The thinning

rates measured via radar altimetry use a longer time series of thickness data (Paolo et al., 2015), and so we expect these values to be more representative of the average thinning over our study period than previous laser-altimeter based estimates, which range from 36-63 Gt a-1 (Depoorter et al., 2013; Pritchard et al., 2012; Rignot et al., 2013; Shepherd et al., 2010). Some of the range in measured thinning may reflect real multi-annual variability (Paolo et al., 2018) that is sampled differently during the 5-year laser-altimetry record compared to the 18-year radar-altimetry record. However, even if this discrepancy reflects real variability, we likely underestimate the error in our melt and flux calculations by using a temporally constant thinning rate and propagating only the stated error from Paolo et al. (2015). While using different thinning rates substantially alters the estimated melt, melt rates on Dotson calculated using any of these values are larger than the grounding-line flux. Thus, using a different thinning rate within the range of published values would not substantially alter our conclusions, though it would imply greater magnitude of melt.”

Page 11

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

Added “This effect is likely not real, but rather is introduced by the model as compensation for poor estimates of temperature and thickness at the calving front.”

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?

Done. Rates from RACMO range from 0.48 to 1.21 m/yr.

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 uncer-

tainty 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).

We have reworked this section in response to the general comments of this reviewer, though this point remains relevant. We have added the following: "While Gourmelen et. al (2017) are able to compute spatially resolved melt rates beneath all of Dotson, their altimetry-based method has greater sensitivity to certain errors than the methods we employ. The amount of snow on an ice shelf significantly influences surface elevations because the lower-density snow does not hydrostatically depress the shelf as much as an equivalent thickness of ice. Thus, uncertainty in SMB leads to significant uncertainty in thickness changes, particularly because SMB may be inversely correlated with basal melt on seasonal-to-interannual timescales (Paolo et al., 2018). Moreover, mismeasurement of the surface elevation is increased tenfold in estimating the melt rate using altimetry-based methods, leading to substantial uncertainty. Our method is primarily sensitive to horizontal flux divergence, so it is less sensitive to errors in surface elevation and SMB than the method of Gourmelen et al."

Line 28: "similar" => comparable

Done

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

Done

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

We changed this sentence to the active voice: "We find that thinning and speedup early in the study period are likely an ongoing response to earlier changes."

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.

We clarified what has been established previously and made this sentence active: "Similar to previous studies, we show that basal melt rates increased on areas that were floating throughout the study period, and we find that total basal melt was further increased as ungrounding exposed more area to melt. "

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. We amended this sentence to make explicit that this is our speculation. The sentence is now: "These conditions lead us to speculate that change in melt, likely resulting from a change in ocean forcing years or decades before 1974, may have led to in Dotson's imbalance in 1996. "

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/icesheet loss).

We have added an additional sentence to state the importance and clarified that we are suggesting this as future work. The last lines now read: "Determining the initial cause of change to this system is key to understanding whether the present retreat results from ongoing oceanic or climatic changes, natural variability, or internal instability, and thus important for placing these observations in the context of other changes to submarine basins around Antarctica. In the future, prognostic modeling of this system beginning in 1996 or before (i.e. "hindcasting"), could help test how different initial perturbations to the system would have affected its flow speed and mass balance, and thus provide context to these changes relative to those observed in other glaciers."

Supp. Page 2: Line 18: "and we are unsure of the exact method used by Khazendar et al". Why don't you ask them?

We emailed Ala Khazendar, and he indicated that he thought this point anomalous. Those authors had referred to an anomalous point in the text, though the connection between that portion of the text and this particular measurement had not been clear to us. We thus eliminated the speculation that interpolation caused this discrepancy and note only that they identify that point as anomalous. The relevant text now reads "...our values for the thinning rate agree to within error for the 27 points that they do not identify as anomalous (Table S2)." Additionally, we have added a note to the caption of table S2 stating: "Note that Khazendar et al. identify point 24 as anomalous, though the value we calculate is similar to others in the area."

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