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Discussions

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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 the thoughtful comments and for useful references that have improved the quality of the manuscript. We have addressed the specific comments below (reviewer's comments in bold). We will upload the new version of the manuscript and supplement, with and without changes tracked, separately.

This paper describes observations of mass budget for the Crosson and Dotson ice shelves in the Amundsen Sea sector of the Antarctic ice Sheet and attempt to provide a narrative for the timing of imbalance and for the succession of events and interaction that led to the current state of imbalance and mass loss of both

C1

floating and grounded ice in the region. The paper integrates new and existing observations with modelling in a balanced way in the context of existing literature. The manuscript is well structured, detailed and illustrated. I support the publishing of the manuscript with only minor changes.

We thank the reviewer for the support of publication and have incorporated the specific comments as described below.

General comments: I find the title vague and confusing; how varied, what response, and how varied are the elevated melt in the first place. Do you imply here that the same, elevated, melt under both ice shelf induce the various response? From the findings of this paper it does not look to me that this is the case.

We have changed the title to clarify that the response is in the flow of the ice shelves, and removed "varied," which was unclear: Changes in flow of Crosson and Dotson Ice Shelves, West Antarctica in response to elevated melt

Thinning rates used to calculated ice shelf elevation back in time are resolved over spatial resolution of 10's of kilometres; recent work have shown that, at least for Dotson, thinning is highly localised to the grounding lines and on the western margin of the shelf. You should at least address the impact that this might have, or not, on your analysis. You allude to it in section 4.3 but can you be more specific as to where this might be the case?

We think that this most appropriately belongs in the portion of the manuscript that compares our results to previous estimates of the pattern of melt (Gourmelen et al. 2017 and Khazendar et al. 2016). In short, we expect this error affect areas of high melt. We have added the following to section 5.3: "The peak regionally-averaged melt rates found in this study (23 m/yr) occur in the same regions (K1, SW1 and SE1) as found in other studies but the values are lower than the locally computed rates (50 m/yr in Gourmelen et. al (2017) and 129 m/yr in Khazendar et al. (2016)). This difference

C2

is not surprising, since the polygon-based method we use has limited spatial resolution and thus misses variations in melt rate in small-scale features such as within the channel on Dotson. Over broad scales, flux-gate and altimetry-based methods should agree, and indeed the overall melt rate we find beneath Dotson, 7.7±1.3 m/yr, agrees with the 6.1±0.7 m/yr found by Gourmelen et al. (2017). The lower peak melt rates found here compared to prior studies may also result from our assuming a spatially constant thinning rate over shelves where thinning rates vary substantially (Gourmelen et al., 2017). The shelf-wide rates used here likely cause us to underestimate melt in small polygons where thinning is most rapid (e.g. K1, SW1, and SE1 in Figure 3). Similarly, they may cause us to slightly overestimate melt over broad, slower-changing regions (e.g., D1 and D2), but this effect should be smaller due to the shelf-wide thinning rate being more representative of rates in these regions. On scales smaller than the polygons we use to calculate melt, thinning may reach 50 m/yr (Gourmelen et al., 2017), comparable to flux divergence in these areas, causing the polygon-averaged rates to locally underestimate melt by a factor of two and introducing an error in the polygon average that we estimate may be as high as 50% in small polygons near the grounding line. "

Specific comments

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L12: Ambiguous, suggests rephrasing. Is what is meant here that the melt under Dotson is dominating the mass loss? Or that the melt under Dotson is driving the mass loss elsewhere? From fig.3 it looks as though the mass loss of Dotson and Crosson are pretty much equal, that the increase in melt rate through the study period is similar, and that one is dominated by loss through basal melt (Dotson) and the other through calving (Crosson).

The increase in melt is greater on Dotson, but indeed the overall mass loss between the shelves is similar and the change is similar. We have changed the sentence to

"These ice shelves have lost mass continuously since the 1990s, and we find that this loss results from increasing melt beneath both shelves and the increasing speed of Crosson."

L17: The preceding line gives an explanation for the observation, but this one does not - what is causing the lack of response of Dotson's velocity? Added: "likely a result of the sustained competency of the shelf."

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L36: The terminology is indeed varied - Smith West is sharing a drainage basin and an ice shelf with Kohler, shouldn't this then be Kohler East instead as in Mouginot et al., 2014?

While Smith West and Kohler share an ice shelf, Smith East and West are flowing together as they reach the grounding line. Though there are justifications for either choice, we chose this terminology because the two trunks of Smith merge while grounded, and thus naming as in Mouginot et al., 2014 necessitates drawing a division between Smith and Kohler East in the midst of fast flowing, grounded ice. Following the naming conventions here, divisions between grounded glaciers match natural boundaries in velocity.

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L15: Ambiguous, suggest: "No similar changes in extent were observed over Dotson"

Agreed, thanks.

L23: "over" -> "under"

Done.

L24: "much higher than the surrounding ice" with the exception of the grounding line regions of Smith and Kohler Added the parenthetical "(though lower than at the

grounding lines of Smith and Kohler)"

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L15: Medley has no sup. Material.

This is correct, our phrasing was poor. Changed to "details can be found in Medley et al. (2014) and the supplementary materials to Joughin et al. (2014)."

L27: What air thickness is found here and how does this compares with modelled values? There are evidence that firn-air content found using this approach differs from model output, so well worth addressing this here. A map in Supp. Mat. For example.

We have added a sentence in the main text to give the range of values we find using this approach and to compare this to the Ligtenberg et al. 2011/RACMO2.3 model output. We have also added a figure showing the spatial pattern of firn-air content in the supplement (Figure S2). The relevant portion of the main text reads: "We find the firn-air content to range between 12-18 m across most of the ice shelves (Supplementary Figure S2); a firn model forced with output from RACMO2.3 shows greater firn-air content than we observe, generally between 20-25 m over these shelves (Ligtenberg et al., 2011)."

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Line 6: The longer dataset also agrees with recent, high-resolution, estimates.

We added "Additionally, this longer dataset matches recent, high-resolution radar-based estimates (Gourmelen et al., 2017)."

Line 10: Reference Shepherd 2010 is missing

We are unsure of what the reviewer is referring to here: the inline citation is present, and it is listed in the references as "Shepherd, A., Wingham, D., Wallis, D., Giles, K., Laxon, S. and Sundal, A. V.: Recent loss of floating ice and the consequent sea level

contribution, Geophys. Res. Lett., 37(13), n/a-n/a, doi:10.1029/2010GL042496, 2010."

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L2: You might want to compare and contrast the weakening described here with the shelf weakening and impact on speedup of Smith described in goldberg et al (2016, fig 3) - I think it is relevant here as there are similitudes but also new patterns, especially along the Eastern margins.

We were unaware that these ice shelves were studied in that publication, and we thank the reviewer for the reference. We chose to address this comment in the discussion of Crosson's speedup, and added: "Prior work has addressed the effect that thinning (equivalently, weakening) over different portions of Crosson and Dotson would have on ice loss upstream (Goldberg et al., 2016), and the areas in which we find weakening encompass several regions that are important for upstream dynamics. That work suggests that while the overall dynamics are insensitive to the bulk of weakening we find here, weakening in key areas, particularly near the Haynes tongue and at the western shear margin, is important for loss of grounded ice upstream, and thus may have influenced speeds near the grounding line as well."

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L14: There is also no indication of freeze-on from EO-based basal melt extraction

We thank the reviewer for the comment clarifying this statement. We have added the following: "Previous work (Depoorter et al., 2013; Gourmelen et al., 2017; Rignot et al., 2013) found no evidence of large-scale freeze-on beneath Dotson, and we eliminated the possibility of localized freeze-on being the primary cause of the channel by comparing radar observations to floatation levels."

L35: There seems to be melting occurring at the southern end of this portion of the channel. In fact it looks to me that the effect of the advection is visible

along most of the length of the channel (ice flow is transverse to the channel up to about half-way down the shelf) as the locus of melt occupies only a fraction of the entire topographic depression. Then, can other sections of the channel be used to derive timing?

We tried to use as much of the channel as possible for this analysis. There are two factors limiting where this analysis is viable: whether flow was transverse to the channel and whether there was an obvious point upstream at which to assume the channel originated. We think our analysis extends as far westward (downward in Figure 6a) as it can given these two conditions. Because of the along-flow channel coming from near the grounding line of Kohler immediately westward of where our analysis cuts off (Figure 6a), we lack an upstream edge of the channel in this region. Moreover, the westernmost (bottom) line/string-of-pearls in Figure 6a is no longer fully perpendicular to the channel, and the channel is more aligned with flow farther westward. West of the outflow channel from Kohler, the channel we analyze is nearly parallel to flow, and so cannot be used for timing.

How would ice divergence influence the time-evolution of this channel's width and depth (e.g. Drews et al., 2016)?

The difference in orientation of the portion of the channel that we analyze compared to Drews et al. makes comparison to that work difficult. Drews finds almost no expression of the channel in the along-flow velocity or strain, though he does find a gradient across flow (his figure 6). We do not expect an across-flow gradient here because the channel itself is oriented across flow, and thus the thickness gradient driving the effect on across-flow strain is absent. We also cannot find such a gradient in the across-flow surface strain (from the InSAR velocities). In the along-flow direction, there is no expression in the along-flow surface strain as calculated from the InSAR velocities. Moreover, we expect the way in which we calculate the timing to be relatively insensitive to ice divergence. Since we integrate the travel time along flow to get the timing, we are accounting for the along-flow change in ice speed/the along-flow strain. However,

we acknowledge that there may be some effect of along-flow strain not detectable in the velocity, though such an effect must be small. We have added: "While ice divergence following the initiation of channel incision may have had an effect on the channel width (Drews, 2015), there is no change in the divergence of the measured surface velocities around the transverse portion of the channel (conversely, along the flow-parallel portion there is significant convergence). Thus, any change in width due to divergence is at or below the level of uncertainty in the velocity measurements, and therefore small compared to the advection of the channel."

How does this timing relate to timings deduced from grounded ice thinning and ice shelf channel formation (e.g. Konrad et al., 2017; Gourmelen et al., 2017)?

While Konrad et al. get tight bounds on the timing of imbalance on Thwaites and PIG, for these glaciers the uncertainties are extremely high, and so unsurprisingly there is significant overlap with the timing we infer. We have added a sentence simply saying that our results are consistent with the timing found by those authors and by Gourmelen et al.: "This timing is consistent with prior work on the upstream propagation of thinning over these glaciers, which found that thinning initiated around 1970ïĆś30 at the grounding lines of Smith and Kohler (Konrad et al., 2017), as well as with an altimetry-based study of Dotson, which found that the shelf had been thinning for at least two decades but not more than a century (Gourmelen et al., 2017)."

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Line 21: Ok, how does these melt estimates fit in your comparison earlier in this paragraph between melt obtain here and melt rates obtained by Khazendar et al?

This section was changed substantially in response to comments by the other reviewer. We have made this paragraph a three-way comparison of these studies: "The peak regionally-averaged melt rates found in this study (23 m/yr) occur in the same regions (K1, SW1 and SE1) as found in other studies but the values are lower than the locally

computed rates (50 m/yr in Gourmelen et. al (2017) and 129 m/yr in Khazendar et al. (2016)). This difference is not surprising, since the polygon-based method we use has limited spatial resolution and thus misses variations in melt rate in small-scale features such as within the channel on Dotson. Over broad scales, flux-gate and altimetry-based methods should agree, and indeed the overall melt rate we find beneath Dotson, 7.7 ± 1.3 m/yr, agrees with the 6.1 ± 0.7 m/yr found by Gourmelen et al. (2017). The lower peak melt rates found here compared to prior studies may also result from our assuming a spatially constant thinning rate over shelves where thinning rates vary substantially (Gourmelen et al., 2017). The shelf-wide rates used here likely cause us to underestimate melt in small polygons where thinning is most rapid (e.g. K1, SW1, and SE1 in Figure 3). Similarly, they may cause us to slightly overestimate melt over broad, slower-changing regions (e.g., D1 and D2), but this effect should be smaller due to the shelf-wide thinning rate being more representative of rates in these regions. On scales smaller than the polygons we use to calculate melt, thinning may reach 50 m/yr (Gourmelen et al., 2017), comparable to flux divergence in these areas. This thinning may cause the polygon-averaged rates to locally underestimate melt by a factor of two and introduce an error in the polygon average that we estimate may be as high as 50% in small polygons near the grounding line."

Fig. 3: b and c should have the same y-axis range (e.g. [0 50])

We have matched the axes.

caption: steady state (SS) The cross-hatched light grey bar is labelled floating ice in the figure and grounded ice in the caption - should it be floating ice everywhere?

Indeed, the cross-hatched bars should be labeled floating everywhere, thanks. We have added parentheticals to define the acronyms "SS" and "GL".