Review for “REMA reveals spatial variability within the Dotson Melt Channel”

The authors use a novel cloud-based method to prepare DEMs and perform basal melt rate calculations called BURGEE (Basal melt rates Using REMA and Google Earth Engine). They show that this method produces high-resolution (50 m) gridded melt rate results that match closely with two other lower-resolution (500 m) studies for large ice shelf features, but that the higher-resolution product captures smaller melt features as well. They also show that errors in basal melt rates due to shifting DEMs using surface velocity maps are reduced at lower resolutions (250 m), without significant loss of spatial variability. Finally, they put forward explanations for some of the spatial variability in surface elevation and melt rates in the vicinity of two basal channels.

This paper fits the topic of the journal and presents a novel, scalable, open-source method for producing basal melt rate maps from REMA DEMs. Overall, I find this study to be well-thought-out, and the figures are *excellent* in both quality and content. However, I have several concerns about the organization and clarity of the paper. I have some recommendations to address these concerns:

1. Several of the thresholds within the steps used to register and correct the REMA strips seem arbitrary to me (Section 4.2) – I would like to see brief justifications for them:
    - L. 200 (criterion ii) Why is the 15 m CS-2 vs REMA difference used to filter CS-2 outliers? I see how this filter would remove control points where either CS-2 or REMA is erroneous, but if the strip is consistently 15+ m too high/low but otherwise fine, it will be excluded. If the authors wish to remove erroneous CS-2 points before using them for registration, I recommend filtering CS-2 using published quality flags and/or by comparing CS-2 to the published REMA mosaic and/or ICESat-2 and removing outliers that fall outside some range around a spatially-averaged elevation or elevation difference (for example, interpolate REMA mosaic to CS-2 points, take moving mean of both sets of points or their differences over some along-track distance, and remove CS-2 elevations that are above/below 2 standard deviations from the moving mean) – this would likely be reported in Section 3.1.
    - Line 202 (criterion iii) Why is the minimum threshold for the number of match-ups 80? It seems that this minimum should depend on the relative size and resolution of the strip and footprint of CS-2.
    - L 203-205 (criterion iv) Similar to my comment above, it is not clear why the north-south and east-west distances between distal CS-2 points should be >40 and 10 km, respectively. This is effectively setting a minimum size for the strips – and the strips could easily be filtered earlier by area or length and width. I think this step is fine, but please include a specific justification for the minimum distances. It would also be helpful to include a figure showing the CS-2 reference ground tracks, or actual ground tracks used in this study, and how they are oriented compared to the strips (for example, add the reference ground tracks to Fig. 3h), and a discussion of how these orientations informed the thresholds that have been set.

2. The process by which the annual DEM mosaics shown in Fig. 3 are produced is not outlined clearly. I recommend including a separate section in the methods addressing this, separately from the (co-)registration of each strip. This omission also leads to confusion in interpreting the basal melt rate results in Fig. 5, which show maps with nearly complete coverage for combinations of years in which the mosaics are not complete. How are the complete coverage maps in Fig. 5 obtained? Are supplementary strips used that were not included in the mosaics?
3. Related to point 2, the description in Section 4.3 of the Lagrangian displacement method is unclear. I made connections (perhaps mistakenly) between both a. displacing strips acquired within a given austral summer as part of the co-registration process to produce an annual mosaic, and b. displacing annual mosaics to t_end in order to calculate rates of elevation change. However, the distinction between these processes is not clear as written, and I think that devoting a section to describing the mosaicking process will greatly help readability. It may also help to add a panel to Fig. 2 showing the workflow for the mosaicking.
4. I recommend reorganizing section 4.5 for clarity, particularly to distinguish the different time scales used in basal mass balance estimates. Please see major comments 2 and 3 above about how DEM mosaics are created and how DEM strips/mosaics are displaced to estimate Lagrangian changes. I find a number of possible inconsistencies that could be clarified by separating bits from Sections 4.2 and 4.3 to form a new section devoted to describing how the annual mosaics are made:

If we consider an example in which we calculate a tri-yearly basal melt rate between the years 2011/12 – 2014/15:

- "M_s is the average yearly surface mass balance from the time of the first DEM mosaic to the time of the last DEM mosaic" indicates that the surface mass balance is the average value of all annual values between 2011/2012 and 2014/15.
    - However, section 3.3 indicates that monthly SMB values are used – so is the 2011/2012 annual DEM associated with the average of all monthly values between 1 July 2011 – 30 June 2012 OR is each strip associated with M_s most close to its collection time, and the M_s is "mosaicked" similarly as the strips to create an annual M_s map?? Please clarify here and/or in Section 3.3/new mosaicking section.
- "The divergence field and the firn air content are added as bands to the DEMs before applying the velocity displacement" indicates that The 2011/12 annual mosaic and its firn air content and divergence field are shifted to the ice's position in 2014/15.
    - However, Section 3.4 indicates that firn air content is obtained on a 10-daily basis – so is the 2011/2012 annual DEM associated with the average of all 10 day values between 1 July 2011 – 30 June 2012? OR is each strip associated with the firn air content most close to its collection time, and the firn air content is "mosaicked" similarly as the strips to create an annual firn air content map? Please clarify here and/or in Section 3.4/new mosaicking section.

- L. 261 indicates that $(h - hf)(\nabla \cdot u)$ is calculated on a yearly basis – so is each annual DEM associated with its own $(h - hf)(\nabla \cdot u)$ map? Please clarify here and/or in Section 4.4/new mosaicking section.
    - Related, "If $\Delta t > 1$ year, the average of the years considered is taken" – does this mean that for 2011/12 – 2014/15 that the $(h - hf)(\nabla \cdot u)$ used in Eq. 1 is the mean of the 2011/12 $(h - hf)(\nabla \cdot u)$ map and the 2014/15 $(h - hf)(\nabla \cdot u)$ map? Please clarify.
- "The firn air content and divergence fields are therefore also displaced, and their values correspond to those at the original position and time (in the case of firn air content) of the DEM strips" – this indicates that divergence fields and firn air contents are calculated/interpolated to each strip before annual DEM mosaic are created. I recommend this be moved to a new section about how the annual DEM mosaics are created.

- L. 266-269 about the entire study period relates to later questions about how the maps in Fig. 4 are produced. I read that all annual mosaics are shifted to their position in 2017/18, moving ice parcels forward one year at a time, and recalculating $(h - hf)(\nabla \cdot u)$ after each 1-year shift – is this right? Please clarify if not. Please also clarify how Mb is calculated over the entire time period – are elevation changes, firn air column averages, and M_s averages computed between every combination of flow-shifted DEMs (i.e. 2010/11 – 2011/12 and 2010/11 – 2012/13… and …2010/11 – 2017/18, and so on), or between consecutive DEMs (i.e. 2010/11 – 2011/2012 and 2011/12 – 2012/13 and so on), OR between each DEM and the 2017/18 DEM (i.e. 2010/11 – 2017/18 and 2011/12 – 2017/18 and so on)? Is the map reported in Fig. 4 the average or median across all combinations, or something else?

5. Currently, some contextual discussion is present in the Results section, (e.g. how main channel's bend may be caused by buoyancy forcing of a meltwater plume; how variable melting pinning points may cause surface undulations; postulation about how the hydrostatic assumption might obscure the western margin melt channel in other studies), which makes parts of the Discussion seem like a recap of the results. I recommend reorganizing the Results and Discussion sections a bit so that the Results presents only the authors' novel findings, while the discussion places them in context.
6. Please check for consistency when describing the locations of ice shelf features. I recommend explicitly naming the two basal channels discussed to remove ambiguity (for example, abbreviate the large, more central basal channel DMC (Dotson Main Channel) and the smaller channel at the western shear margin DWC (Dotson Western Channel) or similar). I recommend including a brief section on the morphology of the entire ice shelf, either in the Introduction or beginning of the Results (since a major result of this study is high-resolution elevation maps) in which the features can be identified. They could also be marked on this paper's Fig. 1 with superimposed lines of different colors/weights/styles as in Fig. 1 of Dow et al. (2018, Science Advances – Nansen ice shelf channels). This will make it easier for the reader to orient themselves throughout the paper.

Minor scientific comments:

- L. 22 I recommend being more specific about what the potential consequences of basal melting and ice shelf thinning may be (e.g. ice shelf/ice sheet mass loss and Sea level rise)
- L. 49-50 Most literature suggests that this loss-of-lock near steep gradients is unique to radar altimetry – I recommend including a citation here
- L. 50 At this point it is not clear why Fig. 1 is included, since there is no indication so far in the introduction that CS-2 data were used in this study, or that this study focuses on Dotson. I recommend moving L. 50 from "From Fig. 1…" to 54 to the paragraph starting in L. 84, including a brief reminder of the limitations of radar altimetry on steep slopes
- L. 55-57 The impact of the resolution is relative to what questions are being answered – I recommend being more specific about what is lost due to varying resolutions across studies/methods. Furthermore, in L. 56 "the chosen temporal and spatial resolution" is a little misleading, because the spatiotemporal resolution and range is constrained by data availability, not just choice – please revise.
- L. 78/L. 354 Chartrand & Howat used OIB for registration where available and CS-2 otherwise (ICESat and REMA do not overlap temporally) – please revise
- L. 83 Please specify what metric (elevation, elevation change, basal melt rate) the signal-to-noise ratio is concerning for
- L. 103 Please specify the signs for melt/accumulation in Eq. 1
- L. 122-131 I recommend including some or all of the detail about how both CS-2 and REMA strips are filtered prior to registration (including moving L. 173-174 to here).
- L. 154/Section 3.5 Consider renaming this "basal melt rate comparison products" or similar since "evaluation" connotes methodology.
- L. 173-174 I recommend moving the sentence beginning "However, before all that is done…" this to Sect. 3.1 and revising surrounding sentences as appropriate
- L. 179-182 Applying the tide and ibe corrections requires knowledge of the time the elevation data were collected – please specify how the collection times of the strips were determined and discuss the implications of using 6-hourly corrections
- L. 186 Please specify which grounding line product was used to calculate the correction transition
- L. 190/Section 4.2 Please see major comments 1 & 2 above
- L. 214 Please provide brief justification for using tri-yearly basis for elevation changes – why not annual?
- L. 217-218, L. 244 Similar to major comment 1 above, please provide brief justification for removing points with elevation change rate of > 15 m/yr as opposed to deviation from a spatial mean. Particularly in Lagrangian, this could obscure fast, small scale processes like rift opening (which may be fine for this study since it is focused on melt rates and not fracture)
- L. 224-241 Please see major comment 3 above
- L. 256-270/Section 4.5 Please see major comment 4 above
- L. 275 Related to major comments 2-4 above, please define what a "perfectly aligned annual DEM mosaic" is. I would recommend avoiding using "perfectly" unless it is specifically defined earlier.
- L. 283 Please include more detail about the various resolutions used and why

- L. 286/Fig. 4 Related to major comment 4 above, please describe how the 2010/11 – 2017/18 trends were calculated
- L. 293 Related to comment on L. 186 above – please describe how the Dotson/Crosson border was defined and/or why was the ASAID grounding line is used to define the Dotson ice shelf
- L. 294-295 isn't the "smaller area" the same as "at the border towards the Crosson" in L. 293? Remove or revise.
- L. 299 Specify which panels of Fig. 5 the striped pattern is visible
- L. 300 varying coverage – see major comment 2
- L. 303 spatial description – see major comment 6
- L. 313 "bending toward the margin" – which margin? See major comment 6
- L. 315 I recommend including a velocity and/or strain rate map in one of the figures so the reader can see the convergence zone clearly
- L. 321-322 see major comment 6
- L. 324 Please specify what the cross section is and what quantity (basal melt, thinning?) has an error of +- 2 m/yr
- L. 330 please specify what quantity (elevation, melt rate?) has a smooth/wavy pattern
- L. 338-339 Please clarify what is meant by "implying greater thicknesses", or consider reframing this as evidence for deeper basal drafts experiencing more melt, which is seen elsewhere in literature

L. 362-363 Alley et al. (2019, Sci. Adv.) also shows marginal channels observationally, and Alley et al., (2016, Nat. Geosc.) identify this western marginal channel (not plotted, but in the shapefiles)

L. 381 It seems that the authors should be able to verify the presence of a surface depression associated with the basal channel from the DEMs – please clarify.

Editorial Comments:

- L. 1 Consider "The intrusion of circumpolar deep water in the Amundsen and Bellingshausen Sea Embayments of Antarctica causes ice shelves in the region to melt from below, potentially..." for concision
- L. 7-8 Revise for clarity and concision
- L. 9-11 I recommend reworking these sentences for clarity: "…based on CryoSat-2. Both products show a wide melt channel extending from the grounding line to the ice front, but our high-resolution product indicates..."
- L. 11 "main channel" >> "this channel" since others haven't been introduced yet. Consider "Additionally, it reveals" >> "Additionally, our basal melt rate product reveals…" for specificity
- L. 13 "This emphasizes" >> "These results emphasize" or "Our results emphasize" for specificity
- L. 41 "This approach allowed to assess" >> "This approach allowed the assessment of"
- L. 42 "and thereby also revealing" >> "and thereby revealed" or "…revealing…" for concision
- L. 43 Consider "…satellites to generate high-resolution digital surface models of the PIG ice shelf, which were converted to DEMs by co-registering to…" for concision/readability
- L. 61 "Both" >> "For example,"
- L. 75 "and CryoSat-2 are" >> "and CryoSat-2 surface elevation data are" for specificity
- L. 80-81 "i.e.," >> "e.g.,"

- L. 123“reference frame” >> “reference surface” to avoid confusion with Lagrangian vs Eulerian or geodetic reference frames
- L. 167-170 Consider "…changes (Sect. 4.3). The latter, along with ice flow divergence (Sect. 4.4), are used in the basal mass balance calculation (Sect. 4.5)" for concision
- L. 246 “calculating the gradients have” >> “calculating velocity gradients have” or as appropriate for specificity
- L. 247 I recommend removing the gradient calculation method types or describing them briefly for clarity
- L. 271 “increasing the spatial resolution” is ambiguous, consider “coarsening” or “refining” as appropriate
- Fig. 7 I recommend including an arrow labeled “direction of flow” on the cross section plots to orient the reader further
- Fig. 8 bottom plots needs x-axis label/units
- 358 “after” >> “downstream of”
- L. 369 Please revise for clarity; see major comment 6
- L. 384 please revise for clarity and concision
- L. 404-405 please revise for clarity