Dear Christian Haas,

Thank you very much for the feedback.
Comments from the reviewers that have not been included explicitly are outlined below. In black is the comment from the reviewer, in red our original answer and in blue our updated answer/comments.
Furthermore, we have in the meantime found a bug in the code which has now been fixed. The ice flow divergences had not been adjusted accordingly for their original resolution. The consequence of this was that areas with high divergence or convergence were suffering from either too high or too low basal melt rates. This means that the coastal channel discussed in the original manuscript (Fig. 6 in original manuscript) is less pronounced in the updated results. We have therefore decided to exclude this part from the updated manuscript.

Kind regards,
Ann-Sofie P. Zinck and co

Reviewer 1 comments:

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.

We will name the channels Dotson Main Channel and Dotson Coastal Channel and be consistent with that throughout the paper. We do not agree that high resolution elevation maps are a major result of this study, but rather what we use them for, namely the high resolution basal melt rates. Further, surface depressions in an ice shelf does not necessarily mean high basal melt rates which we also see on Dotson. In the Figure below we have marked an anchor shaped surface feature on the ice shelf which is not associated with high melt rates. These kinds of features are very interesting, especially because we do not know what has formed this feature, but it is out of the scope of this paper. Therefore, we also believe that adding a section on the morphology of the ice shelf will reduce the readability of the paper, since our focus is not on surface features, but on melt features from which some happen to be explained by surface features.
Since we have decided to exclude the part about the Dotson Coastal Channel from the manuscript we only discuss one channel which we call Dotson Main Channel.

- 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

We use the Dotson Ice Shelf as a test site to show what BURGEE is capable of, for which the ASAID grounding line is used to define the Dotson/Crosson border. We acknowledge that newer and better grounding line products exist for the Dotson/Crosson ice shelf/shelves, but they do not change the findings of this paper.
No adjustments made in the manuscript based on this comment.

- L. 324 Please specify what the cross section is and what quantity (basal melt, thinning?) has an error of +- 2 m/yr

"resulting error" will be replaced with "basal melt rate difference".
This has been specified throughout the section.

- L. 247 I recommend removing the gradient calculation method types or describing them briefly for clarity

Please refer to the new structure of the methods section, which also includes a move of this section.
We have removed the gradient calculation method types as recommended.

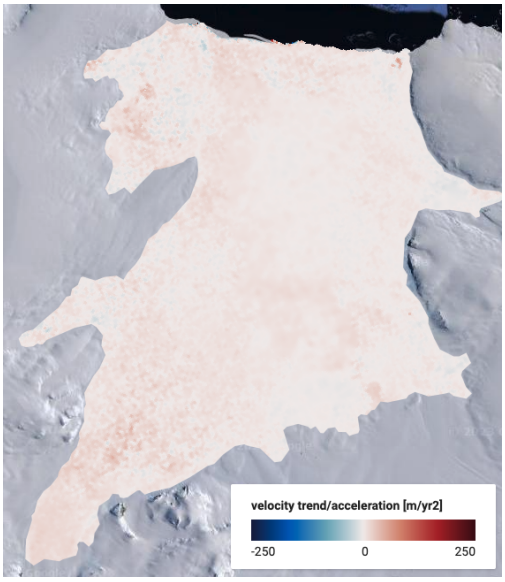
Reviewer 2 comments:

L129: Any seasonal variability in the bias?

We did not look into this since the REMA strips availability more or less is constrained to the austral summer period.
No adjustments made in the manuscript based on this comment.

L136: You would probably need a reference to support the statement of lack of velocity change between 2010 and 2017. Figure 3b of Wild et al., 2023 suggest that areas of slowdown and acceleration exists through Dotson during this period, interestingly matching some of the melt patterns observed including the new marginal channel.

It is not entirely clear from the Wild et al., 2023 manuscript how the velocities are obtained. The ITS_LIVE velocities over Dotson are incomplete up until 2013. However, when we calculate the trend of all yearly ITS_LIVE velocities from 2013-2018 and applying roughly the same colour scheme as Wild et al Fig. 3 we see the following pattern:



Here we see an almost uniform pattern of a very slight acceleration. Further, it seems that the velocity changes mentioned in Wild et al happened before our study period began. In the updated manuscript we will add a reference to the Lilien et al., 2018 paper who argues that Dotson was stable: “Dotson, which has maintained its speed despite increasingly high melt rates near its grounding line”
In the updated manuscript we refer to Lilien et al. 2018. We have not made any further adjustments based on this comment.

L254: Same comment as in line 136. How would the velocity change described in Wild et al. translate into divergence? I am curious also whether the coregistration refinement is robust enough to be used to refine the divergence between DEM dates?

As seen in the figure above we do not see the same trend/velocity change as Wild et al. when using all yearly ITS_LIVE velocities from 2013-2018. The trend pattern which we can see in the above pattern has a speckled pattern due to noise in the yearly velocities, noise which is not present to the same extent in the averaged ITS_LIVE product. There are no sharp edges between areas of deceleration and acceleration, which is why the resulting signal in the divergence field due to changing velocities will be limited.

Whether or not the coregistration refinement is robust enough to refine the velocities and thereby also the divergences as well is a good question. However, in our study, the final correction / feature tracking is never larger than 300 m, and often also much less than that. So given the 120 m surface velocity resolution, the extra correction which the feature tracking may provide will most likely be too small to properly affect the divergence. No adjustments made in the manuscript based on this comment.