The authors derive maps of elevation change and melt rate from high resolution stereo imagery over a rapidly changing ice shelf in west Antarctica. The paper has two strong axes, one on the methodology addressing challenges pertaining to such datasets and providing a community tool, and the second on the analysis of the findings with regards to ice-ocean interaction making use of an additional modelling dataset.
The paper is well written, clear, and well illustrated. It contains a number of novel elements both methodological and on process that will be of interest to the Cryosphere community. The tool developed by the authors will enable reproduction and should allow further exploitation of the high resolution DEM dataset which should shed light on new processes affecting ice shelves.

I made several comments that I hope can help improve the paper further. My only "major" request to the authors is to strengthen the notion of "resolution", in particular when discussing Lagrangian elevation change and basal melt rate. In several instances a resolution of 50m is mentioned. While this represents the resolution of the original DEMs and the posting of the final product, it might not represent truly the "resolution" of the final product for several reasons. The "resolution" of datasets needed for the mass conservation approaches ranges from 120m for the velocity to several kilometres for the surface dataset. The Lagrangian approach means that effectively over a 7 years period the Lagrangian elevation change and basal melt rate would represent an average over a distance of 2 to 4 km given the speed at which the Dotson ice shelf flows. Finally over distance of 50m or so, hydrostatic equilibrium is unlikely to be realised. While the authors never claim to resolve basal melt features at such a length scale, given the paper's focuses on high resolution inputs and on the production of a 50m "resolution" melt map, they would need to inform the reader and potential users of the product on the maximum effective resolution of the basal melt rate obtained using such an EO based mass conservation approach.

Thank you for the review. We have responded to all points clearly marked in red after each comment.
In the updated manuscript we will make sure to distinguish between posting and resolution and thereby write that we offer our product at a 50 m posting, and of course be consistent with this throughout the paper. We will make the difference between resolution and posting clear and clearly state that this also implies that melt signals in our product at very small length scales are not necessarily true signal.

Other comments:
L17-22: You may want to add something about the importance of mapping spatially detailed elevation change and melt rate when considering ice shelf and ice sheet stability, e.g. Morlighem et al., 2021; Goldberg et al., 2019

We will add the following line in L21: "...be initiated (Schoof, 2007; Ritz et al., 2015). Furthermore, Morlighem et al., 2021 show that the location of temporal changes in basal melt of ice shelves in the Amundsen sea sector of Antarctica matters for glacier-wide mass balances, making spatially detailed elevation changes and basal melt rates important. Therefore, …"

Line 23: replace "or" with ","

Will do

Line 24: "remotely through satellite observations of changes in ice shelf surface elevation". Consider rephrasing, melt rate can be calculated that way for ice shelves that display no change in surface elevation. In this mass conservation approach, elevation change (commonly used for Eulerian elevation change) is often a minor term when compared with advection or divergence.

Will be rephrased to: "remotely through satellite observations of changes in ice shelf surface elevation in combination with information about ice flow and surface processes."

Line 26: "with a temporal resolution defined by field work constrains" note that Apres provides continuous measurements with less ties to "field work constrains"

Will be rephrased to: “with a temporal resolution defined by field work constrains, though it should be noted that autonomous phase sensitive radars provide continuous point measurements with less ties to field work constrains.”

Lines 38 to 46: Also work by Dutrieux et al, 2013 - https://tc.copernicus.org/articles/7/1543/2013/tc-7-1543-2013.pdf

We will add the following in L46: “Earlier, also Dutrieux et al 2013 have assessed the basal melt rate of the Pine Island Glacier Ice Shelf using a similar approach, but using the slightly coarser resolution SPIRIT DEMs.”

L46: The term “swath” is not necessarily common knowledge, I would suggest at least adding a reference e.g. Gourmelen et al., 2018.

We will add Gourmelen et al., 2018 as a reference.

Lines 48 to 67 on limitations. I would suggest spending a bit more time rephrasing this section. It would be more informative to the readers to have a proper pros and cons of the different approaches with then a focus on what dem-differencing brings to the table. The section on temporal evolution especially needs to be reframed. Altimetry provides ~monthly” systematic observations and has been used to derive time-dependant melt rates e.g. see work by Adusumilli et al., 2020 or Paolo et al., 2022. High res. Stereo-imagery on the other hands are acquired opportunistically with, in general but not always e.g. TDX, a much lower temporal resolution. For mass conservation, the elevation dataset is not the only constrain i.e. ice velocity and information on surface processes are also needed, that will also impact spatial and temporal resolution and accuracy of the final product.

In the updated manuscript we will rephrase lines 48-67. Line 51-54 will be moved to the paragraph starting at L84. We will further do as proposed and have a proper pros and cons of the different approaches.

L84: replace “high” with the actual values

Will be rephrased to: “... to obtain high spatial (50 m) and temporal (3-yearly) resolution thinning and basal…”

L125: Could you specify which geophysical corrections are applied to CryoSat data? It would be useful given the discussion further down about tidal and inverse barometric corrections. Possibly also differentiate those applied to grounded, floating, transition, and importantly what ice shelf mask was used.

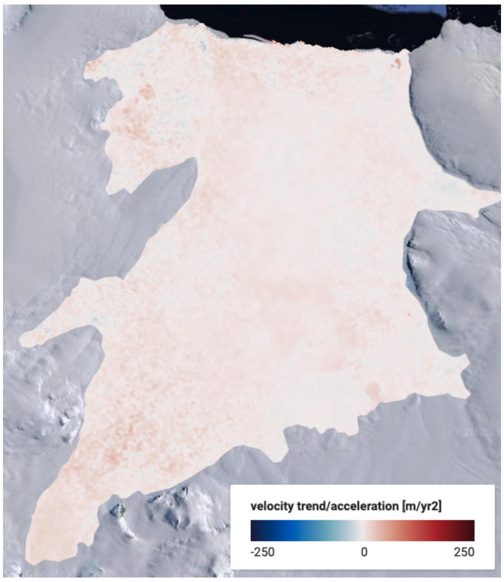
In L127 we will add the following: “Using the provided data from CryoSat-2, elevations are corrected for the following geophysical parameters regardless of the surface: ocean loading tide, solid earth tide, geocentric polar tide, dry and wet tropospheric correction, and ionospheric correction. Ice shelf specific corrections are outlined in Sect. 4.1.”
In L191 we will add the following: “For this purpose we use the ASAID grounding line (Fig. 1, Bentley et al., 2014).”

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.

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”

L140: Even in the case of a non-thinning ice shelf SMB would be needed to calculate basal melt from mass conservation, consider rephrasing.

Rephrased to: Since part of the ice shelf mass balance may be related to surface processes (Ms in eq. (4)) monthly surface mass balance values are obtained from the regional climate model RACMO 2.3p3 (van Wessem et al., 2018).

L180: It would be of interest if you were able to comment here on the differences between your inverse barometric correction, and that provided with the CryoSat-2 data L1b product.

The inverse barometer correction is only provided to the CryoSat-2 SAR data (open ocean) and not the SARin mode which is used around the coastline of Antarctica.

L196: “through”?

Yes! - Changed

L245: Indeed this is a critical step in such computation and can result in increased noise in the final product. The authors could add a figure illustrating the improvement of the divergence methods used here.
What is the effective resolution of the final divergence?

The improvement resulting from this method has already been nicely illustrated by Berger et al., 2017 Fig. 3. Due to the nature of the regularized divergence, namely the regularization parameter \alpha in Chartrand 2011, it is not possible to determine the effective resolution of the final divergence.

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.

L287: I wonder whether it would not be better to distinguish posting and resolution? Especially when discussing Lagrangian elevation change, and when discussing melt rate.

In the updated manuscript we will make sure to distinguish between posting and resolution, and therefore say that we provide our results at 50 m posting instead of resolution. We will make sure to be consistent with this throughout the paper.

L307: I can just about see an area of high melt in that sector in Gourmelen et al., 2017, just at the limit of their map. I wonder wether there is a masking issue here rather than a issue with the dataset itself, as the boundary in their map appears pixelated.

In Figure 5 in Lambert et al. 2022 the light gray area shows the "missing pixels" of Gourmelen et al., 2017. Here it can be seen that it is a narrow band of pixels which is missing compared to LADDIE, and that most of the area where the channel is present is indeed an area captured by Gourmelen et al., 2017. Therefore, we think that it may be a consequence followed by CryoSat-2 limitations in this area of the ice shelf.

Fig. 7: Could you comment on the nature of this melt signal near the calving front, seen in the BURGEE melt map (Figure 5a) but not in the alternative melt dataset shown in figure 5b and 5c.

In L306 we will add the following: "A slight exception to this is the melt signal near the calving front seen in BURGEE. Here, there are large crevasses and fractures in the ice shelf, which may not be well represented in the divergence signal when assessing the basal melt rate at a 50 m posting. "

Fig.8c: x-axis legend is missing

Thanks! Has been fixed.

L340 and Fig. 9: Very interesting section. Would it not make more sense, or at least be interesting, to compare and discuss the correlation between the measured melt rate, rather or in addition to the modelled one, with the simulated friction velocity and thermal forcing? Do you seen a similar strong correlation? Where does that correlation breaks down? What does it say about the melt process or about measured melt rate accuracy?

That would indeed be interesting, but since LADDIE is forced with a different ice shelf geometry (BedMachine) a 1-1 comparison between the modeled friction velocity and thermal forcing and the BURGEE basal melt rates cannot be done. In the updated manuscript we will make sure to mention this.

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