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.

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

Line 23: replace "or" with ","

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.

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

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

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.

L84: replace “high” with the actual values

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.

L129: Any seasonal variability in the bias?

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.

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

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.

L196: “through”?

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?

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?

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.

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.

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.

Fig.8c: x-axis legend is missing

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?

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