Review 1 - Wei Ji Leong

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1 General Comments

This manuscript presents an observational study of six outlet glaciers draining into Vincennes Bay in East Antarctica using products derived from optical satellite imagery, Synthetic Aperture Radar, and laser/radar altimetry sensors. It is an impressive piece of work that uses over half a century of remote sensing data from 1963 to 2022, relying on methods like grounding line delineation using manual digitisation and DInSAR, automated feature tracking for measuring ice velocity, correcting ice surface elevation trends over different time periods, and integrating all of that to investigate decadal scale trends. Overall, the manuscript is well written, with clear references to datasets and other relevant studies, and an interesting discussion about potential forcing mechanisms for the grounding line retreat while highlighting the continued need for DInSAR based grounding line mapping into the future.

That said, I do want to offer some suggestions on ways to improve the manuscript for publication. One of the major things that stood out was the emphasis on how basal melt from warm modified Circumpolar Deep Water (mCDW) is linked to the grounding line retreat at Vanderford Glacier, with a lower emphasis on other factors like changing atmospheric forcings (surface air temperature) and/or sea ice mélange conditions. The authors have been fairly careful in their wording in the Discussion section on this, and offer a convincing line of reasoning on how increased basal melt could result in the observed rapid rate of grounding line retreat, but it does feel like there is a sudden jump between the methods used and that part of the discussion, which could be resolved with a few more sentences on how basal melt rate could be estimated to confirm this line of reasoning. Other than that, there are only some minor points of clarification that could help to reduce some ambiguity, which I will highlight in the specific and technical comments section below.

We thank Wei Ji Leong for their positive feedback on the manuscript and constructive suggestions. Please find our responses to the specific and technical comments outlined below.

2 Specific Comments

2.1 Data Availability

For reproducibility purposes, please upload a copy of your digitised central flowlines, sampling boxes and terminus positions (shown in Figure 1a and described in Section 2.2 and 2.3) in a standard OGR format to a suitable data repository, so that others can benefit from this work too. Ideally, the surface elevation anomaly data (shown in Figure 6 and Supplementary Figure 2) should also be uploaded. If the data are not publicly accessible, a detailed explanation of why this is the case is required. I acknowledge the hard work and time spent manually digitising the lines, and would really appreciate this data to be shared with the Cryosphere community to save time on doing duplicate work.

The digitised central flowlines, sampling boxes and terminus positions will be uploaded to a suitable data repository in order to make the data publicly accessible. We will cite the exact data repository in the final version, should the manuscript be accepted. The surface elevation anomaly data will not be uploaded to a data repository, as each of the surface elevation change datasets used are already publicly available. Direct links to these open-access datasets are provided in 'Section 6. Data

Availability'. Details of how the surface elevation anomalies shown in Figure 6 and Supplementary Figure 2 were calculated are provided in the methods 'Section 2.4. Ice Surface Elevation'.

2.2 Methods

pg9, L229-230: Just to clarify, it states here that grounding line position changes was measured along the central flowline. However, on pg10, L257-258 Figure 2 caption, it says width-averaged terminus position changes, and at pg12, L273 Table 3, it just says terminus position change without mentioning if the change is measured just along the central flowline or width-averaged. For each of these instances, could you be explicit and mention what method was used to avoid any ambiguity?

Terminus position change was assessed using the box method (Moon and Joughin, 2008) and therefore represents a width-averaged value, whilst grounding line position change was simply measured along the central flowline. Table 3 and the associated caption will therefore be updated to explicitly state 'rate of width-averaged terminus position change'.

pg15, L318: Figure 6. This is a nice time-series plot. Two minor comments though. 1) This figure appears to be duplicated in Supplementary Figure 2, albeit with a different colour scheme? 2) Is there a reason for leaving out the Smith et al. (2020) ice surface elevation trends from the plot? At pg 7, L180-183, you mentioned doing some work to allow cross-comparison of the Schröder et al. (2019), Smith et al. (2020) and Nilsson et al. (2022) datasets, but aside from a brief comment on pg 15, L322 that the trends are similar, the Smith et al. (2020) ice surface elevation anomaly trend is not included in the plot? Would the plot look too confusing with a third dataset added in?

1) Figure 6 shows the monthly surface elevation change anomalies calculated within the GL box of each glacier, whilst Supplementary Figure 2 shows the monthly surface elevation change anomalies calculated within the IN box of each glacier. As stated on L316, the surface elevation change anomalies were observed to be very similar across the GL and IN boxes, hence the decision to only include one within the manuscript and show the other within the supplementary material.

2) The Smith et al. (2020) dataset simply provides a value representing the average rate of surface elevation change observed between 2003 and 2019. In contrast, each of the Schröder et al. (2019) and Nilsson et al. (2022) datasets provides monthly surface elevation change values. Whilst the Smith et al. (2020) dataset could be added to the plot in the form of a trendline with a constant gradient, an arbitrary elevation would have to be chosen in 2003 which may potentially be misleading. Inclusion of such a linear trendline could also distract from the interannual variability captured by the 24-month rolling means shown for the Schröder et al. (2019) and Nilsson et al. (2022) datasets. The decision was hence made to not include the Smith et al. (2020) dataset within Figure 6. Instead, the Smith et al. (2020) dataset was primarily used to validate and provide further certainty to the long-term trends extracted from the Schröder et al. (2019) and Nilsson et al. (2022) datasets. We will add some additional text to explain this point in Section 3.3 of the manuscript.

2.3 Results

pg12, L265: Just need some clarification on how the median rate of terminus position change over 1973-1991 for the 6 glaciers is calculated. Are you 1) taking the median rate from 1973-1991 for each glacier, and then taking the median of those values over 6 glaciers; or 2) taking the median rate per year over 6 glaciers, and then taking the median over the 1973-1991. In other words, are you taking the median time-wise then glacier-wise, or glacier-wise then time-wise?

Each glacier had a digitised terminus position in 1973 and 1991. The difference between these two positions was used to calculate the average rate of retreat in m/yr for each glacier between 1973 and 1991. The mean, median and standard deviation of these 6 average retreat rate values was then presented in Table 3. L264 - L269 can be edited, as required.

pg12, L273: Table 3. Could you please provide the raw time-series data for each of the 6 glaciers' terminus position for every year, either as a CSV table or in the supplementary file? This would help with the ambiguity mentioned above, and also it would be good too for future scientists to compare rates of change for individual glaciers over different time periods.

The raw terminus position time-series data will be provided in a supplementary figure.

2.4 Discussion

pg 19-22: One concern on the disconnect between the Methods section (which has not explicitly measured or modeled basal melt directly) and this Discussion section 4.1 (which details how mCDW enhances ice shelf thinning and leads to grounding line retreat). At the very least, there would be some mention of how such basal melt rates could be measured directly using radio-echo sounding, or estimated using changes in ice volume (using changes in ice surface elevation over an area and making some assumptions like hydrostatic equilibrium). This could be mentioned as 'Future Work'.

On lines 84-85, we provide a previous estimate made by Rignot et al. (2013) regarding the area-averaged basal melt rates across the Vincennes Bay ice shelves. We therefore don't believe this necessitates an additional 'future work' section, however we do agree that the manuscript would be improved by addressing the disconnection between the methods section and the discussion section, as suggested. We will therefore add a few more sentences to the Vanderford Glacier discussion (Section 4.1), emphasising that the accurate quantification of basal melt rates across the Vanderford Ice Shelf could further our understanding of the rapid grounding line retreat observed. We will then suggest how this could be quantified, with discussion of direct radio-echo sounding methods and use of estimated changes in ice volume, outlined.

3 Technical Corrections

3.1 Abstract

pg1, L13: "satellite aperture radar" - "synthetic aperture radar"

This will be changed to state 'synthetic aperture radar'.

3.2 Introduction

pg2, L33: "Recent mass loss". Please state a general time period. E.g. 2010s to 2020s, to be clearer about when this mass loss is happening.

This will be changed to state 'Recent mass loss from the WAIS (2000s to 2010s) has largely been concentrated within...'. The Feldmann & Levermann (2015) reference will therefore be removed.

pg2, L36-40: "The ice flow acceleration ... has been attributed to ice-shelf thinning and reduced buttressing, a process forced by the wind-driven intrusion of warm mCDW ...". This is a nice information-rich sentence, it might be good to mention 'basal ice-shelf melting' somewhere to be explicit that the forcing is from the bottom-up and not top-down.

This sentence will be changed to explicitly state that the ice-shelf thinning is driven by basal ice-shelf melting.

pg2, L52: "-57.0 ± 2 m" - "-57.0 ± 2 Gt/y

The units will be changed from m to Gt/y as suggested.

pg2, L56-58: "..., Miles et al. (2016) have observed ... 74% of Wilkes Land outlet glaciers measured to retreat between 2000 and 2012". Table 1 from Miles et al 2016 actually mentioned that the results from Wilkes Land (DB13) were obtained from a previous study, see Supplementary Table 6 by Miles et al 2013 (https://doi.org/10.1038/nature12382). In the 2013 paper, the date range is 2000-2010, while the 2016 uses 2000-2012, my interpretation is that the former (2000-2010) is the correct date range. It is recommended to cite the earlier 2013 paper instead of the 2016 paper as the canonical data source for this statistic. Also, 74% of XXX glaciers can be somewhat ambiguous (though it is used like so by Miles et al. 2016), I'd recommend stating the absolute number of glaciers that have retreated (n=39) in addition to the relative percentage. Overall, this sentence could be modified into something like "..., Miles et al. (2013) have observed widespread terminus retreat across the region, with 74% (n=39) outlet glaciers measured to retreat at a median rate of -63.6m/a-1 between 2000 and 2010".

This is a valid point and the suggested modified sentence will be included.

pg3, L71: Figure 1. Missing Longitude/Latitude gridlines or Polar Stereographic coordinate tick marks. Need to have some spatial coordinate reference system to set the geographical context of this area.

Tick marks and longitude/latitude gridlines will be added to Figure 1.

3.3 Methods

pg9, L242: Could you be a bit more specific about the REMA product used and version? Assuming that you are using REMA v1, and the mosaic instead of the strip DEMs? If using the mosaic, what spatial resolution, 10m, 100m, etc?

Surface topography profiles were extracted using the REMA v1 mosaic. Figure 9 in Howat et al. (2019) shows the spatial resolution of this dataset was between 2 and 8 m within the Vincennes Bay study area. These specific details will be clarified in Section 2.6 of the manuscript. It is important to note, however, that surface elevation was sampled at the same 500 m interval spacing as the bed elevation, as stated on L243.

3.4 Results

pg11, L258: Figure 3. If possible, provide longitude/latitude or polar stereographic coordinate tick marks.

Tick marks and longitude/latitude gridlines will be added to Figure 3.

pg11, L263: Figure 3 caption. Please clarify source of background optical satellite imagery, is it Sentinel-2, Landsat, or other?

The figure caption will be updated to state all background optical satellite imagery shown in Figure 3 was sourced from Sentinel-2B.

pg13, L282: Figure 4. Maybe better to replace 'Distance from VA (km)' with something like 'Distance from top of flowline (km)' or something like that?

Figure 4 x-axis caption will be changed to state 'Distance along VA flowline (km)'. The figure caption will also be edited to explicitly state that distance is measured in the downstream direction, with 0 km therefore representing the inland start point of each flowline.

pg16, L329: Figure 7 colorbar. The colour bins have only one label placed at the middle of the bins, e.g. the white box for +0.00. So does white represent -0.025 to +0.025, or +0.00 to +0.05? Could the numbers be shifted to make it less ambiguous?

The symbology used for Figure 7 was a linear interpolation between a maximum value of +0.10 m/yr and a minimum value of -0.20 m/yr. The labels shown on the colorbar therefore correspond to the exact value for the specific colour shown. For example, the white coloured bin represents an exact value of 0.00 m/yr, as labelled. However, although the colorbar is displayed in discrete bins, it is important to note that a continuous colorscheme is used within the figure. We think that this colorscheme is intuitive to the reader, with white colours indicative of little change, more intense blues indicative of increased rates of thinning, and more intense reds indicative of increased rates of thickening. Whilst this symbology could have been represented using a continuous colorbar with the minimum and maximum values labelled, we believe the binned examples provide more detail of the graduations to the reader. As the labels correspond to the specific colour bins, shifting the numbers would therefore be innapropriate.

We would also like to emphasise that the intention of Figure 7 is not for the reader to necessarily extract specific rates of surface elevation change, as this is primarily conducted using the monthly SEC datasets provided by Schröder et al. (2019) and Nilsson et al. (2022). Instead, the purpose of Figure 7 is to allow the reader to simply assess the spatial variation in ice surface elevation change observed within Vincennes Bay, which we believe is achieved.

pg18, L347: Figure 9. Maybe better to replace 'Distance from VA (km)' with something like 'Distance from top of flowline (km)' or something like that?

Figure 9 x-axis caption will be changed to state 'Distance along VA flowline (km)'. As explained previously, the figure caption will also be edited to explicitly state that distance is measured in the downstream direction, with 0 km therefore representing the inland start point of each flowline.

3.5 Discussion

pg22, L428: Figure 11. If possible, provide longitude/latitude or polar stereographic coordinate tick marks.

Tick marks and longitude/latitude gridlines will be added to Figure 11.

3.6 Supplementary

pg4, L17: Supplementary Figure 1. Missing Longitude/Latitude gridlines or Polar Stereographic coordinate tick marks. Need to have some spatial coordinate reference system to set the geographical context of this area.

Tick marks and longitude/latitude gridlines will be added to Supplementary Figure 1.