Review of tc-2022-217 "Extensive and anomalous grounding line retreat at Vanderford Glacier, Vincennes Bay, Wilkes Land, East Antarctica" by Hannah Picton et al., 2023

In this manuscript, Picton et al. use a range of remotely sensed datasets to examine recent glaciological changes within Vincennes Bay, East Antarctica. Amongst other interesting indicators of glacier change, they report, most notably, upon the rapid retreat of Vanderford Glacier's grounding line at a rate of 0.8 km/yr between 1996 and 2020.

Overall, the paper is generally well-presented, well-structured and is scientifically robust, and even considering the observations of Vanderford Glacier's behavior alone – which places it as the 4th fastest retreating glacier in Antarctica over the satellite era – I believe this manuscript will be of broad interest to the readership of *The Cryosphere*. For this reason, I recommend publication. Prior to publication, however, I believe the manuscript has several limitations in its current form which should be addressed. These limitations are detailed in my comments below.

General Comments

Vanderford grounding-line retreat. While the retreat rate reported here is undoubtedly significant, the problem is that it is for the most part not a new finding. This is because, as the authors themselves allude to on Line 84, Rignot et al. (2019) have previously reported upon this behavior as observed between 1996 and 2017 (over which time they also find a retreat rate of 0.8 km/yr). In this regard there are two key issues with the manuscript in its current form:

1) The grounding-line retreat-related findings – as presently reported at least – are perhaps more incremental than the narrative of the manuscript would suggest.

2) Apart from the introduction (Line 84), no further acknowledgement of Rignot et al.'s earlier observations is included, which could be misconstrued by some as slightly disingenuous.

To remedy these issues, I would suggest the authors:

1) Rework the text to contextualize their findings more clearly alongside this earlier research. (In e.g. the abstract and conclusion, phrasing like "Our results confirm extensive grounding-line retreat…" is used, but this doesn't make it explicit that this is a confirmation of a previously documented observation). More explicit follow-up discussion of the fact that the author's observations show *continued* retreat since 2017 would also be beneficial, and serve to demonstrate that their research goes beyond that discussed by Rignot et al.

2) What's also new (and arguably much more interesting) relative to the simple trend of 0.8 km/yr reported here and in Rignot et al. (2019) is the seemingly step-wise, temporally variable patterns of retreat observed between 1996 and 2021 (Fig. 8). I think a more explicit/nuanced discussion of this phenomenon and its links with e.g. changes in bed topography/MISI (or otherwise) as seen in Fig. 9 would make for a much more interesting read, while again going beyond that described in Rignot et al. (2019). (See also my ***Minor Comment*** on Fig.9 below).

Structure/presentation of manuscript. While generally well-presented/written overall, I believe the manuscript could also be overhauled in places (abstract, discussion and conclusion especially) to offer a more succinct / 'to the point' discussion of the key points and novel findings only. Perhaps most importantly, I think the structure of the discussion requires some careful refocusing, as at present it contains a lot of unnecessary details which should either be moved to the Methods, Results or Supplementary Information. Elements of the Discussion and Conclusion also have the tendency to jump back and forth between ideas and/or from one sub-section to the next, which I think should be corrected for improved readability. (See my ***Minor Comments*** below for some examples of these sorts of issues).

Minor comments

Line 13 – The ice surface velocity, thinning and GL position datasets are also derived from remotely sensed techniques, so I suggest rephrasing the sentence to better convey this point.

L13 – synthetic aperture radar

L34 – driven > dominated (I agree that these are the two main glaciers, but other parts of the coast are just as sensitive to oceanic influence and are now contributing to these trends too)

L57- 'measured to' > 'having undergone'?

L60 – Weaker easterly winds relative to what and where? This was unclear to me. This line should be revised to clarify this.

Fig.1 – Nice figure. Could 1b be underlain by hillshade and/or contours to make the topography 'pop out' more? In 1a, I also strongly recommend displaying the most recent GL (or ideally all of those included in your timeseries if the figure doesn't look too cluttered) to give the reader an instant sense of how each glacier has retreated through time. To help the reader find the locations referred to in the text (e.g., L81), please also add lat/lon graticules to both panels (and all other maps for that matter).

L101+ – Here, I suggest removing the methodological detail behind the analyses performed (terminus position, velocity, elevation, GL change) as this information is better placed in the following section.

L109 – 'USGS Earth Explorer data repository, with …'

L112 – Why was co-registration of the earlier scenes performed? I presume this pertains to the old ephemerides and DEMs used for geocoding Landsat 1-4 images, and thus the need to ensure spatially consistent imaging through time? Worth stating that here if so. Also, why was co-registration not performed for the ARGON mosaic? Considerable positional errors can exist in those images, and likely exist in the Kim et al. mosaic too.

L119 – … are hereafter referred to as …

L132 – … the AutoRIFT feature tracking algorithm.

L135 – Typo? I believe ENVEO records date back to 2014?

L136 – Suggest rephrasing to say "… using a combination of coherent and incoherent feature tracking techniques". (following Nagler et al. (2021; doi:10.1109/igarss47720.2021.9553514) which should also be cited here).

L184-197 – As the authors are aware, the grounding line and hinge line are two different components of the grounding zone, the latter being a proxy for the former which cannot be detected from satellite imaging. This fact should be stated somewhere in this paragraph, if anything to make it clear that the two components haven't been conflated.

L206/7 – '… was generated using … as applied to ERS-1 and ERS-2 imagery…'.

L216 – Should read '… most seaward, spatially continuous break-in-slope' (since multiple discontinuous breaks-in-slope can exist downstream over, for example, pinning points or ice rumples).

L227 – Following my comments on L184-197, I think it's important to state here that it's difficult to compare break-in-slope and hinge line positions in a direct/like-for-like manner, because they're ultimately measuring two very different components of the grounding zone. Therefore, even given the standalone instrument/technique errors shown in Table 2, any changes in GL position identified from the two techniques should be interpreted with caution, with any further discussion restricted to those exhibiting pronounced retreat where we can be confident change represents a true signal.

L230-234 – I'm not sure I follow this, and specifically why the two GLs would be so far apart. Geocoding issues? Tidal effects (if different imaging dates)? Both? Additional information explaining this would be good here.

Fig.2 – Suggest noting the non-linear x-axis scale in the caption or, even better, editing the figures to show a linear scale.

Fig.3 – Nice figure. Caption should read '…from the USGS Earth Explorer data repository' or similar.

Fig.4 – Nice figure, but what does, for example, 'Distance from VA' mean on the x-axis? Does this literally mean from the 'VA' label on Fig 1? If so, suggest changing the notation to read 'Distance downstream' (or similar), and annotating VA to VA' on both Fig.1 and the cross section profiles of Fig.4.

L290 – Associated errors. This is great, but please also show these errors on Fig 5 too.

Figs. 6 and 7 – These are clear, detail-rich figures. My one thought, however, is whether they could be merged somehow to save the reader flicking back and forth between figs?

L340 – Following my comment on L227, I think it's important to mention here that the retreat observed falls greatly outside sensor error limits, and likely also any between-sensor uncertainties associated with combined DInSAR/break-in-slope comparisons.

Fig 8 – This is a nice figure, although I question whether it's integral to the interpretation presented. I think a revised version of Fig. 9 with each GL position shown would be far more impactful, with Fig. 8 moved to the supplementary information instead. Either way, remove the word 'manually' from the caption since the methods states that both break-in-slope and DInSAR mapping was carried out in this way.

Fig.9 – Please add each GL location to portray the temporal evolution of retreat more clearly, and if needed revise the results/discussion to reflect any temporal patterns of retreat this reveals (see also my ***General Comment*** on this point).

L368-375 – I have two comments about this paragraph. First, I believe this is a significant finding which could/should be better articulated both here and in the abstract and conclusion. By my understanding this places Vanderford Glacier as the 4th fastest retreating glacier in Antarctica (let alone East Antarctica) over the satellite era, only after Thwaites (0.8 kmyr = 3rd, as mentioned by the authors), Pine Island (0.9-1 kmyr = 2nd; Park et al., 2013; doi:10.1002/grl.50379) and Pope (3.3 kmyr = 1st; Millilo et al., 2022; doi:10.1038/s41561-021-00877-z).

That said, I was a little surprised to see no discussion of Rignot et al. (2019) here, who following Line 84 report the same rate of retreat between 1996 and 2017 (i.e. overlapping 90% of the present study's observational period). As such, I believe this paragraph (and elsewhere) should be reworked to better contextualize the author's findings against this study.

[NB: I don't think the above suggestion will necessarily detract from the novelty of the present study, as long as it's made clear your observations show the *continued* rapid retreat of Vanderford Glacier to 2020].

L416-419 - So what caused this significant GL retreat then? Discussion of passive ice according to Furst et al. is good, but strikes me as a convenient diversion away from what should be the main focus of this paragraph. A revised discussion of the expected key drivers of this phenomenon will make for a much more compelling read, even if the interpretation is 'the causes remain unknown and an important region for future research' or the like.

Fig.11 – If GLs are added to Fig.1 (see my comments on that figure above), then I'd consider moving Fig.11 to the supplementary information to avoid figure repetition in the main text.

Line 450-476. How has landfast sea ice changed between 1996 and present? An increasing amount of research has shown the importance of landfast sea ice/mélange for controlling calving rates in Antarctica (by congealing together / buttressing ice fronts), which I think should also be noted here.

(See papers by e.g. Greene et al. (doi:10.5194/tc-12-2869-2018), Francis et al. (doi:10.5194/tc-15-2147-2021), Fraser et al. (doi:10.5194/essd-12-2987-202), Arthur et al. (doi:10.1017/jog.2021.45), Christie et al. (doi:10.1038/s41561-022-00938-x) and Massom et al. (doi:10.1038/s41586-018-0212-1) for some recent examples of this in Antarctica).

L498-510 – OK, the majority of this paragraph goes on to discuss the importance of landfast sea ice for controlling terminus stability, but it seems largely out of place to me in this section. All things considered, to improve readability/clarity I would suggest merging Sections 4.2 and 4.3 into a new, single section which first overviews the observed glaciological changes (frontal retreat and thinning) and then discusses their possible links to sea-ice loss.

On a related note, recent work has also shown that decreased sea ice cover may also leave coastal regions vulnerable to the influence of storms or katabatic wind events which can disturb ocean surface slopes leading to enhanced strain-induced calving (see e.g. Francis et al. (doi:10.5194/tc-15-2147-2021; 10.1029/2021JD036424) and Christie et al. (doi:10.1038/s41561-022-00938-x) for recently documented examples of this phenomenon).

To my mind, these sorts of atmosphere-sea ice interactions (sea-ice debuttressing, wind-induced strain) are more likely to explain the observed patterns of calving than the mechanism proposed by Miles et al. 2016 (at least over the relatively short timescales considered here), because in Antarctica the majority of basal melting is confined to the GL (e.g. Rignot et al., 2013; doi:10.1126/science.1235798). This phenomenon doesn't occur by coincidence, and is because mCDW resides at the same depth as the GL. Elsewhere (including at the relatively much more shallow ice-shelf fronts), mCDW does not physically interact with the ice in such a way that can drive rapid frontal fracture/calving, and thus the more moderate thinning signals observed at those locations instead reflect lagged responses resulting from historical perturbations at the GL.

Section 4.4 – While interesting, most of this section reads like background information and less pertinent results which I'm not sure is best placed in (or integral to) the discussion. I suggest making this a supplementary discussion and alluding to it somewhere in Section 3.2.

Section 4.5 – Similar to Section 4.4, much of this section contains information which should belong in the Methods (especially given my comments on Lines 184-197 and 227). Following my suggestion for Line 340, I also think the discussion contained on Lines 539-545 could be removed as the signal-to-noise of break-in-slope-derived change relative to the 1996 DInSAR GL pick seems compelling. I further suspect that the pattern of retreat revealed by plotting each GL onto Fig.9 will support this conclusion.

Section 5 – While acceptable as written, the conclusion is very long and could/should be overhauled to provide a much more succinct, punchy summary of the key findings and implications only. I expect this could be done in half a page or less. Structure-wise, I also find the discussion going from *GL retreat* to *terminus change* and then back to *GL retreat* to be confusing, so suggest that the ideas contained in the paragraphs beginning Lines 547 and 573 could be merged into one coherent narrative.

L550 – Same comment as Lines 368-375.

L585 – In these days of reproducibility I would strongly encourage the authors to archive and openly share their terminus positions via a repository such as Cryoportal.

Table S1 – Table Caption – For completeness, insert ‘optical’ after ‘Details of’.

Technical Comments

L33 – WAIS in this instance is a pronoun and so should not be preceded by ‘the’. Similarly, ‘embayment’ should be capitalized. Multiple such blunders exist throughout the manuscript, so it will be worth carefully going through the text and weeding these out.

L37 – ‘grounding line retreat’ should read ‘grounding-line retreat’. This should be changed throughout the manuscript, including in the title. Same for phrases such as ‘ice-shelf thinning’, ‘ice-surface velocity’, ‘sea-ice production’ etc.

L208 – Tense issue. Suggest rewording to: ‘…has an overall, associated error of +/- 100m’.

L221 – Grammar. Why does the use of MODIS imagery mean that the error is 250 m? Suggest rephrasing for improved clarity.

L291 – Typo? Think it should read ‘… at box IN accelerating from …’.

L323 – ‘These trends’. What trends? Grammar. Perhaps the opening sentence of this paragraph could read something like: “Mirroring the elevation patterns observed by Schroder et al. (2019) and Nilsson et al. (2021), similar trends are found in the ICESat/ICESat-2-derived data of Smith et al. (2020) (Figure 7)”.

L406 – Insert comma after ‘(Figure 6a)’.

L560 – Missing citation?

Section 7 – Text missing?

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