Review comments for Bryony I. D. Freer et al.'s "Modes of Antarctic tidal grounding line migration revealed by ICESat-2 laser altimetry"

Tian Li

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General Comments

In this research, Bryony Freer and co-authors mapped the short-term variations in grounding line locations at the Bungenstockrücken Ice Plain using ICESat-2 satellite laser altimetry with a new approach of calculating elevation anomalies. They observed > 15 km tidal GL migration and discovered four different modes of tidal GL migration which are useful in validating numerical model simulations of GL migration and understanding the tidal processes of the ice shelf-ocean-subglacial system. Overall, I find this study interesting, robust and provide new insights into tidal GL migrations, which is important in determining long-term GL changes. The paper is well written and the topic fits well in this journal. I have several moderate concerns detailed below and I hope the authors could address them in the revised version.

Major Comments

Inaccurate statements on mean-profile method:

In **Section 3.2.2**, the authors claim that the mean elevation profile approach cannot be used to calculate the fine-scale short-term GL migrations with ocean tides compared to the newly proposed "Lowest-Sampled Tide" (LST) approach, this is not technically correct.

First, it does not make sense to map short-term GL migrations using a mean elevation profile of all 7 repeat cycles (in the case of RGT 559 GT3L). The nature of using an average elevation profile of all repeat cycles across the study period has already determined that we can only derive one Point F - which is the most landward ice flexure location experienced by the ice shelf among all the repeat cycles. This is the reason you see the clustered Point F features in Figure 2c. But this does not mean that the mean elevation profile method itself cannot be used to derive fine-scale short-term GZ features, which can be achieved by reducing the number of cycles used in calculating the elevation anomalies using a mean elevation profile.

Second, as the authors already mentioned "*only two repeat measurements are required to locate Point F using RTLA*" in **Line 131**, if you iterate different combinations of **any two** ICESat-2 repeat cycles, calculate the mean elevation of these two cycles and estimate Point F from elevation anomalies based on this mean elevation profile, you will get a very detailed spatial sampling of the short-term Point F locations along one ground track, similar to the results in Figure 2e. For example, using only cycles 9 and 11 along RGT559 GT3L with the mean elevation approach can produce a most seaward Point F location between -80.90 and – 80.85 latitudes.

In fact, the mean elevation approach has several benefits over the LST approach proposed in this study:

1) **Denser spatial sampling**: the number of different repeat cycle combinations available for Point F calculation will be higher than LST, because the latter only compares different repeat cycles to one fixed cycle with the lowest-sampled tide.

2) **Higher temporal resolution**: using the neighboring two repeat cycles can provide Point F with a time resolution of 91 day, this cannot be achieved by LST.
3) **Free from errors in tidal models**: the mean elevation approach doesn't need tidal model to determine a reference profile.

Third, in contrast to the statement in **Line 227-229** "*This also overcomes the issue of the mean reference profile being skewed by inland flexure at higher tides, but with the additional advantage that it can be applied consistently across RGTs, enabling automation*", the automation of the iteration of different repeat-cycle combinations with the mean elevation approach is straightforward to implement, actually the method should be similar to the LST method shown in Figure 3. In addition, using only two cycles can also overcome the issues of skewed ice flexure at high tide.

Therefore, I would like to hear the authors' opinion on this point, and would like to see a comparison between Point F locations calculated from any given two repeat cycles using the mean elevation approach and the results from their LST method.

Tidal model accuracy:

The Neutral Tide and the LST methods proposed in this study rely on tidal model predictions from CATS2008. Previous research show that the ice at deep grounding zone at narrow ice shelf embayment may not respond adequately in phase with ocean tides (Li et al., 2023; Reeh et al., 2000), which means that tidal model cannot always provide accurate tidal amplitude predictions. If this is the case, will the proposed approach still be valid for deriving short-term Point F locations and how the inaccurate tidal model predictions will impact your Point F result? Please clarify.

<u>Specific Comments</u>

Line 37: Please rephrase this sentence, GZ can be wider than 10 km as demonstrated by the results of >15 km migration in this study.

Line 45-50: This paragraph lacks mentioning previous research in studying the short-term GL migrations using satellite altimetry and DInSAR, such as:

- Brunt et al. (2011): https://www.cambridge.org/core/journals/journal-of-glaciology/article/analysis-of-ice-plains-of-the-filchnerronne-ice-shelf-antarctica-using-icesat-laser-altimetry/80F41E7FDC8124136AF50615861D5C71
- Milillo et al. (2017): http://doi.wiley.com/10.1002/2017GL074320
- Milillo et al. (2019) https://www.science.org/doi/10.1126/sciadv.aau3433
- Brancato et al. (2020): https://onlinelibrary.wiley.com/doi/10.1029/2019GL086291
- Milillo et al. (2022): https://www.nature.com/articles/s41561-021-00877-z

Line 52-53: What are the spatial and temporal resolutions of these historical archives? What is the requirement of spatial-temporal resolution for assessing the tidal GL migration?

Line 63-65: "*where short-term GL migration is likely to impact both ice dynamics through rapid variations in basal shear stress, and basal melt rate through changes in cavity geometry enhancing tidal mixing.*" Here needs a reference.

Line 109-110: Dawson and Bamber (2020) also mapped Point H in addition to Point F in their study.

Line 111: Need to mention why CryoSat-2 is not suitable to detect short-term GL changes – due to its long repeat cycle (369 day) (Dawson and Bamber, 2017).

Line 116-122: Mohajerani et al. (2021) have also improved the DInSAR sampling under different tidal phases using Sentinel-1a/b SAR images across the Antarctic Ice Sheet.

Line 128: Please mention that the "*unrecoverable topographic biases across GZs*" is caused by the large across-track slope across the GZ.

Line 128-131: "*In contrast, the Advanced Topographic Laser Altimeter System (ATLAS) that launched on board ICESat-2 in 2018 has a six-beam design with more accurate pointing, which reduces across-track deviation from the reference ground track (RGT), providing better spatial sampling of the GZ.*" Not accurate and need clarification:

1) The six-beam design reduces the across-track deviation from the **Reference Pair Track (RPT)** inside each beam pair, not the Reference Ground Track (See https://nsidc.org/sites/default/files/icesat2_atl06_atbd_r005.pdf).
2) Please give a number of the ICESat-2 point control accuracy here and how it compares to ICESat. Luthcke et al. (2021) show that the performance of ICESat-2 can achieve 4.4 ± 6.0 m, this represents a very small across-track separation across repeat cycles.

Line 133: Here I suggest the authors to add some discussions on the pros and cons of Li et al. (2022b)'s results, and talk about why it is necessary to extend the data record in this study.

Line 142-144: I think there is no need to mention the switch between weak and strong beams because this study did not use this feature in GZ calculation.

Line 149-151: see my previous comment, can also merge this sentence into Line 142-144.

Line 152: "*We obtained coincident tide amplitudes at the most seaward point of each ICESat-2 ground track per cycle*". Please provide an average distance between these ICESat-2 seaward points used in the tidal amplitude calculation and a reference (historic) grounding line. This is important because if the seaward point still locates inside the GZ, then the modelled tidal amplitude may not represent the actual ice shelf elevation change in hydrostatic equilibrium. In addition, the orientation of the ICESat-2 grounds tracks are not always perpendicular with the actual grounding line, this can also introduce biases (Li et al., 2020, 2022a).

Line 157: Please clarify why the annual tidal distribution is essential in GZ calculation.

Line 172: "*minimum segment difference exceeds 1*", I assume this is 1 meter, please add a unit.

Line 192-195: Here the authors merely mention the traditional mean elevation approach in calculating elevation anomalies, then directly propose two new methods without any discussions on the disadvantages of the mean profile method. It is not clear why it is essential to develop two new approaches, why mean elevation approach fails to map short-term GL locations, and how the two new methods can further improve these research gaps. I suggest incorporating some of the information in **Line 201-235** to the beginning of **Section 3.2.2**.

Line 201-213: As I have mentioned in the major comment, mean profile approach can provide high spatial-temporal resolution GZ calculation, please consider modify the relevant content accordingly.

Line 229-230: The mean elevation profile approach can allow the calculation of Point F from repeat cycles both sampled at low tides, please see my major comments, and rephrase this sentence.

Line 500-501: "*The 15 km observed tidal GL migration at Bungenstockrücken is much larger than standard model grid spacing at the GL*", what is the standard model grid size at the GL?

Line 500-503: "*Similarly, the time step of an ice sheet model is generally much longer than one day; therefore, prescribing a sub-daily change in GL position is not generally possible.*" Not sure why mentioning sub-daily GL change here, especially the results in this paper cannot achieve the sub-daily resolution either.

Figures 2 c,d,e:

I assume the reference elevation profile of each different approach is plotted in the upper panel of subfigures c/d/e, however it is very difficult to discern them from the ICESat-2 repeat profiles. Please change the line symbols of reference elevation profile under different scenarios. Here are some suggestions:

1) consider only include one subfigure to show all the ICESat-2 repeat cycles with a colormap.
2) then add a different subfigure, plot three different reference elevation profiles in distinct colors or symbols and add all ICESat-2 repeat cycles in light grey color with low transparency as a background - this is to make sure that the reference elevation profiles can stand out from ICESat-2 profiles and the readers can clearly distinguish these three different reference profiles.

Figure 6: Both panels f and l are RGT 1223 1R, why do the Point F locations along the same ground track behave so differently? Is this a labelling error in the figure?

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