We would like to thank the editor and both reviewers for their comments which have helped further strengthen this manuscript.

Editor

Thank you for thoroughly addressing the reviewers' comments. The revised manuscript has a significant improvement, including higher-quality figures and expanded discussion and supplementary information. Both reviewers are satisfied with the changes. Nonetheless, I would like to raise a few minor but important concerns that should be addressed prior to final publication:

1. As noted by Reviewer #1, the definition of "1-pixel uncertainty" remains somewhat arbitrary. I recommend the authors clarify this term explicitly in the text to reflect its empirical basis, and consider the uncertainty estimation approach proposed by the reviewer.

We appreciate the argument made by Reviewer 1 that there may be subtle changes in the shape feature through time. However, we do not believe their proposed solution addresses this issue. If the feature changes shape through time, then having multiple authors track the feature still doesn't work. Secondly, there is strong argument that author skill levels and experience in working with the older imagery and tracking features are important here. An author with no or limited experience is not directly comparable to an author with extensive experience.

Out of caution we have increased the uncertainty associated with feature tracking over our decadal image pairs to two pixels, giving a total uncertainty of ±3 pixels. We note this is a very conservative estimate of uncertainty compared to other published studies.

2. In Figure 5c, the red curve from 2008 to 2020 appears inconsistent with the light blue curve in Fig. 5b. As a sanity check, the authors should calculate the velocity anomaly from MEaSUREs for the same location of the undulation and include it in Fig. 5c for comparison.

Yes, there is some discrepancy between the MEaSUREs data in the feature tracking zone (light blue line) and manual tracking of the feature over biennial timescales (red line) in Figure 5. This is to be expected because they are not over the same timescales. Our manually derived estimates are over ~2 years depending on image availability. The annual MEaSUREs data are from some point in the 12-month period between 1st July and 30th June in the given time period e.g. 1st July 2013- 30th June 2014. There is no way of knowing whether the data in these mosaics truly represent average flow speeds across the entire time period, or alternately come from only certain months/weeks within the 12 month time period. Given that ice speed anomalies at Totten can rapidly change, we do not believe there is a way to meaningfully compare ice speeds over different timescales. We note the general pattern of a peak in ~2007-2010 (+4%) and trough in ~2013-2015 (-4%) is consistent with Greene et al., 2017 who used a dataset independent to MEaSUREs.

See Figure 2 in Greene et al. ,Wind causes Totten Ice Shelf melt and acceleration. Sci. Adv.3, e1701681 (2017). DOI:10.1126/sciadv.1701681

3. The manuscript conflates the concepts of ice velocity, ice discharge, and mass loss in several sections (abstract, Section 4.1, lines 326–327, and conclusion). While the authors convincingly show no acceleration in velocity over the past five decades, however, ice discharge also depends on grounding-line ice thickness, for which no direct evidence of constancy is presented. Given the sensitivity of mass balance to small changes in surface mass balance and discharge, more

quantitative evidence need to be shown before definitive conclusions are drawn. The authors should revise these discussions for greater conceptual clarity and precision.

We have improved section 4.1 by confirming that there have been no major changes in the thickness of ice over the grounding line. The slope along which the grounding line is migrating is shallow at -0.3 degrees, so the published estimate of 3 km grounding-line retreat between 1996 and 2013 accounts for a 15 m deepening in bed elevation. Grounded ice thinning is approximately 0.5 m / year, so25 m over 50 years. Given that Ice flowing over the grounding line is 2300 m thick, so any changes in ice thickness are proportionally negligible in the context of Totten's current ~10% imbalance.

4. Regarding the concern from Reviewer #1 on basal melt rates: there is insufficient evidence to assert that basal melt must equal surface thinning. The surface elevation change of ~9 m implies a total thickness change of ~81 m under flotation assumptions. Ice dynamics must be considered in this context, and the discussion should clearly reflect this complexity.

We have amended the text to highlight the role of ice dynamics in surface undulation formation.

Minor:

- Line 160: Please ensure Tables 1 and 2 are reformatted such that dates in the first two columns occupy only one row each. Additionally, avoid splitting column headers like "Days", "Distance", and "Uncertainty" into separate rows.

Amended

Once these issues are addressed, the manuscript should be suitable for publication in The Cryosphere.

Best regards,

Cheng Gong

Reviewer 1

Dear Editor,

Miles and others have done a wonderful job strengthening the paper and addressing reviewer questions after the latest round of reviews. I only have minor comments.

Minor comments

1. Grounding line evolution without yearly labels in Figures 1b, 5, and 7abc. Please add colors and text to describe which line corresponds to which year.

For simplicity we now only show one grounding line from October 2017 which was the most spatially complete of the grounding lines between 2017 and 2019 that we originally showed.

2. Lines 202-207 in the difference PDF (called diff henceforth), 175-178 in the new main PDF (called main henceforth), please split into two sentences.

We have split the sentence into two.

3. Line 254 in diff, 226 in main: remove the word subtle, as the differences are fairly pronounced.

Removed.

4. Line 277 in diff: Do you mean to say ice speed anomalies rather than ice speed? Figure 5 does not compare speed magnitude quantitatively (we get a qualitative sense in a) but rather measure speed anomalies in b. Similarly, consider changing appropriate to reasonable for proxies, with my point expanded below.

a. More on this point: In Figure 5b, before 2014, the two locations overlap in error, but the sign isn't the same. In 2014, the velocities are beyond uncertainty in percent speed change. Afterward, yes, there is more similarity in phase, with large uncertainties. I think that today this is a reasonable proxy for the regions of interest, but still would not exactly expect this to always hold. I am glad that the previous lines 138-140 in the previous main document, 148-153 in the main, have been removed.

Thanks for pointing this out, we did mean to say 'ice speed anomalies' and we have amended this. We have also changed 'appropriate' to 'reasonable'.

5. Line 308 in diff: Add a space before the first parenthesis at (location …).

Space added.

6. Section 3.3.1: Consider concluding this paragraph by claiming that the dominant balance in the mass conservation equation is between thinning and basal melting.

We do not introduce the equation at any other point of the manuscript, so we do not think it would be well placed here.

7. Great additions to Figure 7, I found it quite captivating.

Thanks

8. Lines 414-415 in diff: Please consider putting something, such as a 1 and a 2 with arrows perhaps, for the main and secondary rumples this sentence references. Additionally, I'd reference the animation S1 in this paragraph, it is quite informative to see.

We have added the arrows and referenced animation S1.

9. Lines 432-433 in diff: I do not find this to be a strong claim in the current phrasing, although I agree with the larger point of undersampling. I would consider rephrasing to follow the idea that the full range of variability of basal melt rates may not have been captured in the short two-year window, which is further supported by oceanographic studies of CDW variability on longer timescales, etc.

We have amended the text to address this point.

10. Figure S2: Why is the y-axis of this plot in m yr^-2? Presumably a typo?

Amended

11. Paragraph 1 of section 4.3, lines 463-480 in diff: You should break this up into two paragraphs. First, end the first paragraph before "A key implication …". Second, write about your considerations of three non-exclusive hypotheses that may permit smaller undulations in phase 3 than in phase 1.

We have decided to keep the paragraph structure as is because the paragraph is all on the same subject.

12. Lines 519-21 in diff: Please refer to your Figure 9b advection hypothesis as Eulerian advection.

a. Broader point: I think the argument (explaining thickness change in Figure 9b with Eulerian advection) would be more convincing if you explain what one would expect with high melt rates in phase 2 at the GL, followed by a hiatus (or intermittent high and low melt rates in Phase 3). Then, by drawing your readers on board with expectations, showing both the distance and phase boundary are in line with the observations may enhance the delivery of the claim.

We have referred to Eulerian advection and have re-ordered the text as suggested

13. Figure 9a legend title: Basal (typo).

Amended

Reviewer 2

I think the paper has substantially improved based on these latest revisions. I am satisfied with the responses to all my minor comments. In my original review, I had three major comments on the manuscript:

[1] I was uncertain whether a 1-pixel uncertainty for manual feature tracking is sufficient/appropriate given the long time period between images.

I really appreciate the addition of Figure S1 showing the feature tracking vectors and the stability of the surface feature used for the velocity estimates. I totally agree with the authors reasoning that using decade long offsets between images reduces the overall uncertainty in velocity due to the large feature displacements relative to the picking uncertainty. However, I still do not see how it is possible to pick the exact same spot on this feature to within only 1 pixel error when the precise shape of the feature is shifting over time, as shown in Figure S1. For example, looking at the 2007 vs. 2017 images, the feature in question seems to have evolved from a single ridge or peak to a double ridge with a trough in the middle. My argument is that uncertainty could be better quantified by having 2-3 people independently pick these features in the imagery and then comparing their displacement estimates, rather than assuming an error of 1 pixel based on past literature. Given that the study only uses ~17 images, I do think this is a manageable amount of effort for an accurate quantification of uncertainty.

We have addressed this comment in the response to the editor.

[2] I felt more evidence was needed that the velocities within the feature tracking box are a good proxy for velocities across the grounding line.

The authors addressed this question very comprehensively in their new Figure 5b and I am satisfied that the feature tracking regions is a reasonable proxy for overall trend in velocity across the grounding line.

Thanks.

[3] I felt a more thorough discussion of how the surface undulations form was warranted.

The new Section 4.3 and Figure 7 largely resolve my concerns. I have just one comment on the discussion of the magnitude of the basal melt rates:

At lines 377 to 383, the authors argue that 25 m of differential surface-elevation anomalies over the course of three years requires basal melt rates of a similar size. I interpret this to mean that the authors assume all the changes in surface elevation (e.g. ice thickness) across these undulations come from time-varying basal mass balance. But my understanding is also that the authors are arguing that the undulations are forming because of grounding/ungrounding from a basal pinning point. If that is the case, I would expect the surface elevation change to also come from changes in the local divergence of the ice flux. When the ice is grounded, local compression due to the sudden spatial shift in basal traction could lead to a local pile-up of ice, and when it is ungrounded this doesn't occur since the shelf is locally in extension. In that case, the relevant magnitude of basal melting might just be whatever is required to ground/unground the shelf, which could be quite a bit more or less than the observed elevation change due to the ice dynamics and imprint of the basal topography. This argument would be stronger if the authors could clarify their conception of the role of basal melting vs. local changes in the velocity gradients at the pinning point in creating these undulations and explain why they conclude they think that ice shelf thinning from basal melting dominates the actual elevation signature (not just the existence of the undulations or their spacing).

(I realize there is inherently some circularity here, since the change in basal melt rates causes the change the velocity gradients through grounding/ungrounding. I agree that basal melt rates have to play an important role here. I'm just struggling to understand why, quantitatively, the surface elevation change rate you measure has to be in directly equal to the basal melt rate and would not also be majorly impacted by the change in ice dynamics due to the basal melting.)

We have addressed this comment in the response to the editor.