Initial author response to reviewer comments

We would like to thank both reviewers for taking the time to provide valuable comments that will help improve this manuscript. We are pleased that both reviewers found our work interesting. Below, we provide initial responses to the reviewers' main points in the hope that we will be invited to submit a revision, allowing us to provide full responses and revise the manuscript accordingly. We note, we have also carefully considered all line and minor-level comments and are confident that we can address them all. However, to avoid unnecessary duplication, we do not respond to them individually in this document. We will, of course, include a full, traditional response to all comments upon submission of a revised manuscript.

Response to main comments

Both reviewers raised similar concerns surrounding the processes leading to the formation of the surface undulations and the reliability of our ice speed estimates as a proxy for grounding line flow. We address both concerns in a joint response below and have included the reviewers' original reports beneath our response.

1. How do surface undulations form?

In the revised version, we will include an additional figure to clarify that the surface undulations form due to time-varying re-grounding on a bathymetric high, driven by basal melt variability. This follows the helpful suggestions of both reviewers. ***Specifically, we will add a panel showing the updated gravimetry-derived bathymetry of Totten from Vaňková et al. (2023).*** *The bathymetry near the ice rumple is complex.* ***The undulations form approximately 2–3 km away from the main rumple and immediately downstream of a secondary or connected bathymetric high.*** *As both reviewers acknowledged, bathymetry data beneath ice shelves is notoriously uncertain, and these features could easily be a few kilometers larger or smaller in extent and tens to hundreds of meters shallower or deeper. Nevertheless, we can demonstrate that the undulations are forming on or very close to a bathymetric high. A secondary argument is that no other plausible mechanism explains their formation. Reviewer 1 notes that these features cannot be basal crevasses—an argument we will incorporate into the revised text. Thus, the very presence of these undulations in satellite imagery provides strong evidence of a bathymetric high in the vicinity of their formation.*

Reviewer 1 also asks whether we definitively conclude that the surface undulations are formed by basal melt variability, given the challenges in measuring melt rates. Fortunately, novel in situ observations of basal melt rates (2017–2019) from autonomous phase-sensitive radar exist at Totten (see Vaňková et al., 2023). The spatial location of this time series is very close to the surface undulation formation zone. ***These observations show basal melt variability of 7–9 m a^{-1}, which is of a similar magnitude to the ~20 meters of surface elevation change associated with the surface undulations over two years. We will incorporate this into the revised manuscript and argue that this provides strong evidence that the undulations are formed by time-varying basal melt.*** *For reference, grounded ice thins at a long-term average rate (1992–2022) of ~1 m/a, though this rate varies over time within a range of approximately 0.25–1.75 m/a. Over multiple decades, long-term grounded ice thinning would result in progressively thinner ice flowing into the ice shelf, which likely contributes to the observed trend of progressively fewer and smaller surface undulations, as shown in Fig. 7c.*

2. Regional ice speed as a proxy for flow speed at the grounding line

Reviewer 2 asks: "Can you show, using the MEASURES velocity record in the modern era, that velocity changes within this box are correlated with velocity changes across the grounding line in both relative *magnitude and phasing?" Similar concerns were raised by Reviewer 1, who asks: "Is velocity downstream of a pinning point actually representative of grounding line velocity?"*

*We have investigated this, and in the revised version, **we will include a new figure demonstrating that ice speed in the region where we track surface features is correlated with ice speed at the grounding line over the MEASURES era, both in terms of magnitude and phase**. This provides strong support for our initial hypothesis that ice speed 30 km downstream of the grounding line is a reasonable proxy for grounding line speed. Furthermore, if ice speed at the grounding line were not in phase with ice speed 30 km downstream over the long term, we would expect to see significant damage and crevassing. However, there is no evidence of any notable damage or crevassing in satellite imagery from the past 50 years.*

Reviewer 2 also requested an expanded Figure 3 showing the feature we track over more epochs – we are very happy to provide this in the revised version, along with more detail on the uncertainties in tracking the feature.

References

Vaňková, I., Winberry, J. P., Cook, S., Nicholls, K. W., Greene, C. A., & Galton-Fenzi, B. K. (2023). High spatial melt rate variability near the Totten Glacier grounding zone explained by new bathymetry inversion. Geophysical Research Letters, 50, e2023GL102960. https://doi.org/10.1029/2023GL102960

Reviewer 1

Miles et al. put forth a compelling study of ice rumple history on the Totten ice shelf over the past century. By analyzing the formation of repeated surface undulations that are likely due to interaction with seafloor topography, these authors suggest that a period in the absence of undulations is likely due to variation in basal melt rate. Additionally, they compute interannual and decadal velocities, both of which do not show a significant trend and have large anomalies. I think that after answering the following questions, the authors can greatly strengthen their case through analytical rigor and clarity. However, without these added changes, their arguments remain speculative and claims are overreaching.

Major Points

1 . What actually are these undulations from?

For the first half of the paper, I wasn't convinced that the undulations you show were not large basal crevasses/channels generated from flow past the pinning point. The main reason I thought of this is that there is the large (roughly stationary) pinning point that is several kilometers away from the location you analyze. I know you are aware of the phenomena of fractures propagating laterally beyond pinning points, as you have shown in Miles et al. 2024. However, the icesat2 time series that show surface growth flipped my opinion, as you would not expect surface raising of O(10m) from a basal crevasse. To not have other readers have this confusion, please put a map of bathymetry that explains how local topography may promote the observed undulations, rather than having them originate from the larger ice rumple. Additionally, mention that fractures can be generated downstream of pinning points, and suggest that the observed surface undulations are inconsistent with the surface signature of basal crevasses (e.g., Luckman et al., 2012; McGrath et al., 2012). I know bathymetry products like BedMachine are uncertain (and have associated uncertainty provided), and you cite some folks saying that, but I had to dig into bathymetry data to find that there is a tail or foot to the seamount inferred in that region that highly suggests that what you are looking at is formed locally due to regrounding. This will significantly strengthen your argument.

2. Is velocity downstream of a pinning point actually representative of grounding line velocity?

The observed velocity data is quite noisy - on long time scales there are fluctuations, and on short time scales there are similar or larger amplitude fluctuations (your Figure 5). I would expect this signature of noise in a stick-slip type system, which I would expect from the ungrounding, regrounding, and potentially fracturing that is generated by these seamounts. In a stick-slip system, I would not expect the velocity downstream of the stick-slip location to be similar to those upstream, aka the grounding line. Please provide a convincing explanation of why we would expect the velocities you measure to be representative of the grounding line, as I find lines 138-140 to be unconvincing and appear as too much extrapolation.

To help illustrate the point, the most extreme example of flow past a pinning point may be the Brunt ice shelf. If one tried to compute velocity downstream of the pinning point, then these would be dominated by the flowing/fracturing that occurs as ice moves past an obstacle, and the results may not be a sensible measure of the upstream or grounding line velocity. This is a more extreme case, where the buttressing provided by the pinning point significantly impacts the ice velocity both downstream and upstream of the obstacle. All in all, I am not convinced that the velocity measured is representative for the ice shelf as a whole, nor the grounding line, but mostly for the local region in which it is measured. Please update your phrasing where appropriate - statements about whether a glacier is “in balance” is a measure of grounding line flux versus snowfall integrated from the ice divide, so I would remove or strongly minimize these large-scale claims.

3. Can you definitively conclude that basal melt is the dominant signal by concluding that the changes in snowfall (thickening) and grounded ice thickness (H) and velocity (u, v) cannot correspond to the observed thickness variation? Currently, it is argued (e.g. line 18) but not sufficiently proven.

You show in Figure 1a that you get about 2 meters a year of thinning of grounded ice over a half century window. I imagine this is an average rate. You show in Figure 8 that there is +-3 meters a year of melting on the ice shelf. Finally, you show 20 meters surface elevation changes in Figure 7a within 2 years. The mechanism you propose, largely basal melt variation, needs to get cyclic variations of melt pulses causing 20 meters of variation, as a baseline. I’m with you in that I think this is from the ocean, but mostly because I don’t expect this to be from grounded ice thickness and velocity changes or from ice shelf snowfall. Given that basal melt rates may be challenging to measure, can you construct your argument in terms of ruling out the other two options?

The thin film mass balance equation is partial_t H + div (u H) = a - b, as you are likely familiar with the terms: thickness tendency, divergence of ice flux with (depth-averaged) horizontal velocity vector (u,v), and snowfall rate and melt rate on the opposite side of the equality. This is a more mathematical way of framing the above argument. Please incorporate this logic if possible to see if you can definitively conclude that the changes in partial_t H would be driven by -b and not by div (u H).

Reviewer 2

Miles et al. presents an interesting history of ice flow on the Totten Ice Shelf from the early 1970s to the present day, reconstructed from feature tracking in historical Landsat imagery. They also produce a history of ice surface undulations that they ascribe to varying degrees of interaction between the ice shelf and a hypothetical pinning point. They interpret this as a qualitative history of decadal variations in ice thickness that also suggests decadal variations in basal melt rates. Overall, they conclude that Totten Glacier was likely losing mass at a similar rate in 1970s, but that a period of high melting in the 1940s to 1960s may have initiated that mass loss.

Overall, the paper investigates an important question - the long-term history of mass loss in one of East Antarctica’s most dynamic areas. However, the study cannot directly measure the variables of interest: flux across the grounding line and basal melt rates. What can be measured is the velocity of a single feature in the middle of the ice shelf and the presence or absence of surface undulations at certain locations. I think the paper’s argument could be much stronger with more attention to giving quantitative, data-supported, or physics-based explanations that strongly link these proxy records to the variables of interest.

Major Comments:

1. More detail is needed on the manual feature tracking for velocity estimates. To what degree of precision can the exact same point on a feature be detected 10 years later? Is 1 pixel of error for the feature tracking a sufficient uncertainty bound given changes in features shape, illumination conditions, etc over time? How reproducible is this tracking? (e.g., if someone else were to do the tracking with the same imagery, would they get the same numbers.)

A supplementary or appendix figure showing all the features that were tracked in each image pair and the specific points that were tracked would go a long way towards increasing confidence in these results. I'm imagining something like an expanded Figure 3.

2. I am not entirely convinced by the argument that velocity variations within the feature tracking box should reflect velocity variations across the grounding line. Since the feature tracking box is directly downstream of the ice rumple, it seems like it would be more indicative of flow as modulated by interactions with that pinning point, including fracture processes as the ice flows around or over a barrier. Can you show, using the MEASURES velocity record in the modern era, that velocity changes within this box are correlated with velocities changes across the grounding line in both relative magnitude and phasing?

3. Line 189 – this argument is not convincing to me. I generally understand how undulations might form as ice passes over a pinning point, but from Figure 1, it seems like these undulations are forming to the left side of the ice rumple, not directly down flow from it. Is there some submarine bedrock high or pinning point directly upstream of these undulations that is not marked on the maps or visible in the imagery? If not, what is the theory for undulations forming next to the pinning point? The 2D schematic in Figure 4 does not seem to capture the real geometry of the Totten Ice Shelf and would be more useful and convincing in 3D. I know that sub-ice shelf bathymetry is always highly uncertain, but if you are using BedMachine or some other project to infer the presence of a potential pinning point, it would greatly strengthen your argument to show that bathymetry.

On further digging in the literature, it seems that perhaps there is a lot of reliance on the conclusions from Roberts et al. (2017) when ascribing the origin of these undulations? If that is the basis for these conclusions, the paper would be strengthened by some review of their arguments for the reader who is not as informed about the history of these research on Totten.