We thank the reviewers for providing thorough and detailed reviews of our manuscript. We have revised the manuscript according to each of the reviewers' comments, which we think has greatly improved it.

Both reviewers suggested that the analysis and discussion of Surging Climatic Envelope (SCE) changes over Antarctica should be shortened and limited to just focus on the Peninsula. Given that the SCE may not be applicable to ice sheets and ice streams, we agree with this suggestion and have revised the manuscript accordingly. This has involved substantial removal of text from Section 4.4.

We provide a detailed point-by-point response to each of the reviewers' minor comments below.

Review #1

The authors present a study on recent glacier surges on James Ross Island, northern Antarctic Peninsula region, based on satellite observations of ice flow velocity, surface elevation change and terminus position. The report on the glacier surges is complemented by an extended account on the potential occurrence of surge-type glaciers on the Antarctic Peninsula and in other regions of Antarctica in the 1940 to 2150 time frame, using a predictor based on meteorological reanalysis data, respectively climate model projections under two different warming scenarios.

The analysis of the satellite data revealed four surges on three glaciers during the period 2005 to 2025. For one of the potential surges (Gourdon Glacier 2005) the data base is rather thin, insufficient for definite proof of a surge. Another case of ice flow acceleration (Kotick Glacier 2015) has been reported by St[r]inger et al. (2025) as a potential surge and surveyed with an enhanced data base by Davison et al. (Section 3.3 of this manuscript). The discovery and description of surges in Antarctica is a topic of significant interest per se. Beyond that, detailed descriptions of surges are able to provide valuable information on the dynamic response of glaciers to changes of glacier mass, glacier geometry and climate. In case of the surges presented in the manuscript, the analysis of driving factors for the individual surges is partly affected by the limited quality and completeness of velocity data which vary between individual glaciers and time periods. For one event (Whisky Glacier) the drainage of a subglacial lake has been identified as one of the main triggers.

At large, the presented work on the James Ross Island surges opens up a new topic in the context of Antarctic glacier studies, contributing to the understanding of the multifaceted dynamic behaviour of the glaciers. The manuscript would benefit from more detailed descriptions of surge evolution in space and time and discussion on possible mechanisms for triggering and sustaining the different surges. Estimates on the transfer of ice mass induced by the surges would also be of interest.

The presentation on the potential occurrence of glacier surges in various region of Antarctica conveys rather limited new information. The probability for the occurrence of surge-type glaciers is computed using the Surging Climatic Envelope (SCE) which has been defined by the climate conditions of regions with surging glaciers observed during the last several decades. Based on the SCE and climate reanalysis data and climate projections, occurrence probabilities in between 1940 and 2150 are computed. For selected years probability levels are delineated in maps of the Antarctic Peninsula and Antarctica. Taking into account that the current version of the SCE is based on valley glaciers, it is rather questionable if it is applicable for exploring the potential surge occurrence for outlet glaciers and ice streams of Antarctica and if the surge behaviour will be of relevance for future losses of the Antarctic ice mass. In view of these issues, I recommend shortening the sections dealing with the climatic envelope for surges.

In addition to shortening our discussion of the SCE, noted above, we have expanded our description and discussion of the evolution of the James Ross Island surges in space and time, as suggested by the reviewer. We have included additional time-series of ice speed and surface elevation change at Gourdon Glacier and Kotick Glacier, which provide a more detailed picture of the surge progression. These data are not always sufficient to quantify the transfer of mass by the surges, so we have omitted such quantifications from the manuscript. We argue that omission is acceptable because the central aims of the manuscript are to (1) demonstrate that surging has occurred; (2) characterise those surges; and, primarily (3) discuss the occurrence and possible re-occurrence of surging in Antarctica in the context of climate change.

We note that we do not consider the subglacial lake drainage as a trigger of the Whisky Glacier surge. This is because the lake drains approximately annually and the late-2019 drainage was not particularly large: "given that this lake drained approximately annually during our observational period, and because the late-2019 drainage was smaller than in previous years (Figure 3), it seems unlikely that the lake drainage was the primary cause of the surge". Indeed, we generally avoid the discussion of mechanisms and instead focus on the enthalpy balance model, which inherently assembles multiple individual mechanisms into one framework and which is complemented by our discussion of the SCE.

ID	Comment	Response
01	Information on glacier properties: Main attributes of the three glaciers should be specified, including glacier size, hypsometry, altitude of equilibrium line, estimates of surface mass balance parameters. The size of the floating terminus section and grounding line location before and after the surge should also be quoted (if existing). In this context, possible impacts of oceanic forcing may be addressed.	We have added a new figure (now Supplementary Figure 5), illustrating the elevation and hypsometry of the study glaciers. We discuss this figure along with the observed patterns of elevation change and an estimate of the equilibrium-line altitude (including a discussion of the ongoing thinning of Kotick Glacier – see response ID 11). We note that surface mass balance measurements in this area are sparse and outputs from regional climate models are highly uncertain in this region. In addition, the thickness of these glaciers has not been measured and cannot reliably be inverted because of the aforementioned uncertainties in surface mass balance and the obvious imbalance in glacier flow speed. To our knowledge, there are no measurements of the glacier grounding line locations with respect to position of the glacier termini.
02	Fig. 1 a and b: Please check the orientation of the marker for the North direction.	We have revised the figures to address this mistake.
03	Fig 1 c (Kotick Glacier): Please add a km scale.	Done.
04	Fig. 1, upper right panel: Hardly possible capturing the exact location and setting of the study glaciers within the surface velocity image. I recommend showing this image separately in larger size.	The box in the inset was in the incorrect location – leftover from a previous version of the figure. In the revised version, we have corrected the box size but chose not to show the image in a separate figure.
05	Line 142 – 152: Taking into account the mismatch between the large size of the velocity estimation windows in comparison to the comparatively small size of the surging glaciers and the complex ice motion patterns during surges, the error estimates should be re-considered. The scatter of velocity	We have modified the text in this section to flag your correct point regarding the width of the image patch sizes relative to the width of the glaciers: "We expect these values to underestimate the true error in ice-covered areas, especially where the ice surface profile has changed significantly because we assume a fixed ice surface for the geocoding, and because our image

	data points in points in Fig 2 is also an indication for higher uncertainties.	patch sizes are similar to (0.5 to 1 times) the width of the glaciers". Though we note that the most pronounced scatter of velocity data points in Figure 2 are from those at the most downstream extraction locations prior to 2021, where the image patches will contain a mix of glacier ice and ocean pixels, so greater scatter in those locations is unsurprising. We have added a description of that to the main text on line 155: "though in the terminal zone, glacier margin position changes and sea ice motion can contaminate the velocity retrievals, resulting in greater noise (e.g. Figure 2a)."
06	Section 2.2 Surface elevation change: Please provide an error estimate for the calculated surface elevation change.	Done. We now calculate the error in the surface elevation change as the RMSD between the two REMA strips over the same bedrock areas used during the strip co-registration and include these errors in the relevant figures. The median error from this analysis is 5.5 m.
07	Line 220 to 222: Please specify the locations to which the velocities of 40, 200 and 800 m/yr refer. Fig. 2k shows a large spread of velocities at any time.	Done.
08	Line 231: Changes of the terminus position are shown in Fig. 2m.	Fixed.
09	Section 3.2, Gourdon Glacier: Whereas the data base for the terminus advance in 2005 is rather thin, the data of the 2013 to 2017 event represents a convenient basis for exploring in detail the different phases of the surge and the progress of velocity changes and mass transfer along the terminus. For this task, velocities along the central flowline may be shown on different dates, or time series of velocity at several points in different sections of the glacier, rather than only for one point as shown in Fig. 4k.	In response to this comment and that of another reviewer, we now show speed time-series from several locations along the centrelines of Gourdon Glacier (Figure 4) and Kotick glacier (Figure 5), rather than from a single location as in the previous version of the manuscript. We have expanded the description of the surge progression at these glaciers by drawing on these additional time-series (e.g. lines 269-286, 297-299, 335-348, 349-351).
10	Line 268 – 270: Please explain the processes causing thickening in the upper reaches of the glacier and continuous lowering downstream, and how this is reflected in estimates of the mass transfer.	We now discuss the spatial and temporal patterns of surface elevation change on lines 335-348 of the revised manuscript, drawing on the new Supplementary Figure 5 showing the glacier surface elevation and hypsometry with respect to estimate of the equilibrium-line altitude. Whisky Glacier has not yet begun to thicken, at least in part because flow speeds remain elevated above pre-surge quiescent phase values at the end of our record. Kotick Glacier continues to thin despite returning to quiescent-phase speeds because it has no ice above the ELA. Gourdon Glacier has begun to thicken in some places because of resupply of ice from its high elevation plateau. We note that our aim is not to explain all characteristics of the surge at each glacier.
11	Kotik Glacier, Line 273 – 283 and Figure 5: The data presented in Figures 5k and 5l and the discussion do not provide sufficient information	As noted above, we now include time-series from several locations along the Kotick Glacier centreline (Fig. 5a,k,l), which allows us to

	for clearly describing the development and progress of the surge and for exploring possible driving mechanisms. Time series of surface velocity and elevation change should be shown also for other sections of the glacier rather than only for one point on the lower terminus. Another issue to be explained is the cause for the rather constant gradual decrease of surface elevation between 2015 and 2025 (Fig. 5), in spite of a large drop of velocity in 2017.	characterise these surges in more detail. We have also added a description of the continued lowering in the results and incorporated this observation into our discussion because this is relevant to the wider discussion of climate and surging.

Review #2

The first half of the paper presents an overview of recent surges for three glaciers on James Ross Island based on detailed remote sensing observations and the second half of the paper examines the likelihood of additional surging around Antarctica based on the Surging Climatic Envelope (SCE). The glacier surges are mostly identified using detailed velocity records produced in-house using optical and SAR images, with additional velocities from ITS_LIVE. Surface elevation changes, terminus positions, and geomorphic features are also used to facilitate surge detection in the 21st century and to infer surges that may have occurred in the 20th century. The SCE analysis for the study glaciers compares the SCE of Guillet et al. (2025) to ERA5 reanalysis outputs to determine how long the James Ross Island glaciers have fallen within the climate envelope that is conducive to surging. The paper later builds on this analysis to consider the SCE around all of Antarctica for two Shared Socioeconomic Pathways and the implications regarding surging.

The 21st century observations for the surging glaciers are terrific and it was interesting to see what surges look like for these glaciers. I appreciated the inclusion of the longer-term observations and I think the authors sufficiently conveyed uncertainty regarding interpretation of older, sparser records. I think that framing surges in this region in regards to the SCE is useful as well, but I found parts of SCE discussion to be a bit too hand-wavey and generalized. In particular, the authors state that the SCE is probably not appropriate for the ice sheet but then spend a decent amount of text describing the SCE in relation to the ice sheet. The paper could be strengthened if it was streamlined to remove the broader Antarctic SCE discussion and keep the focus more on the Antarctic Peninsula and islands nearby.

Several recommendations are provided below. I do not consider any of my recommendations to be "major revisions" since I do not recommend any methodological revisions, but the recommended removal of the Antarctic SCE discussion is also not necessarily a minor revision.

As noted above, we agree with your comment that the manuscript would be improved by the removal of the Antarctic SCE discussion and associated figures, so we have removed those in the revised version. As suggested by the reviewer, we think this makes the manuscript more streamlined and robust.

ID	Comment	Response
01	line 19: Replace "Kotick Glacier surged during" to "Kotick Glacier surged from"	Done
02	lines 59-76: References to Terleth et al. (2025; doi: 10.1029/2024GL112514) should be added in this paragraph because that paper describes updates to the enthalpy balance framework to include seasonality in surface melting relevant to this paper.	Thank you for reminding us of this important work. We agree it is highly relevant to this section, so we now cite it as: "The enthalpy balance theory for glacier surging proposes that all surge cycles reflect imbalances between the rates that heat and water (enthalpy) are produced at, and are evacuated from, glacier beds (Benn et al., 2019a, 2022; Terleth et al., 2024)."
03	line 93: You state that you used these data to identify potential surges. Did you put together the same records for all glaciers on James Ross Island? Did you use a subset of the data (e.g., velocity records) to identify the surges and then compile the other observations in support? How did you identify the surges? Did you use an automated	We used the velocity records to manually identify potential surges across the AP. For manually identified surges, we then used elevation and terminus position measurements to characterise the surges. We have modified our wording of this section accordingly: "We used existing (Cook et al., 2005; Gardner et al., 2025) and new measurements of ice surface velocity across the

	threshold-based approach, manual checks, something in between?	Antarctic Peninsula from 1985 to 2025 to identify potential surging behaviour. Candidate surges were confirmed where possible using elevation change and terminus position change measurements. The combination of these measurements allowed us to characterise surges from three glaciers - Kotick Glacier, Gourdon Glacier and Whisky Glacier - on JRI (Figure 1)."
04	line 108: I think the velocity records look terrific but I am wondering what percentage of the data are from your in-house dataset generation and what percentage are from ITS_LIVE? Based on the description of the datasets used in-house, it is not clear why both datasets are needed because they both use the same image platforms for velocity generation. Please include more justification here regarding use of both datasets.	We are really pleased that you think the velocity records look good (though note and agree with your comments regarding standardizing plots 2, 4 & 5). We incorporated data from ITS_LIVE specifically so we could include velocity estimates derived from Landsat 7 image pairs – our in-house feature tracking of Landsat 7 image pairs is still in development. Therefore, the ITS_LIVE data were particularly useful for providing sparse measurements of ice flow early in the record, which helped to constrain, for example, the pre-surge speed of Gourdon Glacier. We have updated the text in Section 2.1 to clarify this. Note also that the ITS_LIVE Sentinel-1 image pair data seem to only be available up to December 2022.
05	line 140: It would be helpful to know what envelopes you used for the magnitude and direction filtering. Were they based on the median +/- some multiple of the MAD? You point to a reference that presumably explains the method more but a little more detail here would be useful.	We have now added this detail: "Additional filtering of the stacked data based on temporal variations in velocity magnitude and flow direction removed remaining outliers, defined as those with a velocity magnitude more than three scaled median absolute deviations from those in a 24-day moving window or a flow direction more than 15 degrees different from the time average (Davison et al., 2025)."
06	lines 142-152: Are the results of the analysis sensitive to the size of the smoothing ROI? 1km seems to span at least half the width of the glacier in some places based on the figures shown for each glacier. How did you decide on the threshold of data coverage of 33% percent to decide if you were going to infill missing values? Am I correct in interpreting that "finite values" means that you obtained velocity estimates for those pixels (i.e., they are non-NaN)? Also, I know that the individual velocity rasters can be quite noisy and so I understand the rationale behind filling holes with a fit to time-averaged data, but I am concerned that the multi-year average rasters could be biased due to temporal averaging during periods when the velocities change dramatically. I'd love to see an example of the variability in speed within the ROI when the ROI is dominated by finite values and one when the ROI has filled values over >50% of the area. If you normalized the ROI speeds by the mean for each ROI snapshot, that would help the	Good questions! Your interpretation of the method is correct and you are correct that the method is effective if the spatial gradient in speed within the ROI remains similar but could introduce biases if the spatial gradient in speed within the ROI changes over time (as might be expected during a surge). Figures R1 and R2 appended below show the (lack of) sensitivity of our ice speed time-series to these methodological choices. The time-series were extracted from a single ROI on Whisky Glacier (~ 2 km upstream of the pre-surge terminus). Figure R1 shows the time-series with and without the gap-filling approach, illustrating that the method does not introduce a bias in the results – the maximum difference between methods is 26 metres per year and the median is 0.6 metres per year. Figure R2 shows the effect of reducing the ROI length to 500 m (from 1000 m), illustrating that the main effect of the larger ROI length is to increase the number of non-NaN time-series points. Note for Gourdon Glacier and Kotick Glacier, we use an ROI of 500 x 500 m and now clarify this in the text. We appreciate this isn't exactly the analysis you requested, but we chose first to perform these simpler tests which we think

	reader get a sense for whether the infilling procedure biases speeds.	demonstrate that these methodological choices do not influence our ability to characterise these surges.
07	line 187: I don't follow this description. Are the glaciers in Guillet et al. (2025) grouped by geographic region and you are only using data from regions that have a high concentration of surging glaciers?	The "region" here refers to a climatic envelope region i.e. the climatic conditions that are associated with a greater or smaller percentage of all surging glaciers. We have clarified this in the revised manuscript on lines 198-200: "The SCE was defined by the climate conditions covering the x % highest density climatic region (in temperature-precipitation space) of all glaciers that surged in between 1990 and 2024 (Guillet et al., 2025); here we consider values of x from 20 to 99."
08	line 202: Why did you smooth the data with a 36-month moving mean?	You're right, this isn't the best way to do this. We have updated the analysis to ensure that our use of RECON and Esperanza data is comparable to that of ERA-5. Therefore, we now calculate a rolling 10-year summer median temperature from both time-series, which has shifted the vertical position of both RECON and Esperanza bars in Figure 7 somewhat, but both observational datasets still act to corroborate the temperature changes shown in ERA-5.
09	Figures 2,4,5: I love these figures but there is a lot going on in them and they are not entirely consistent with each other. My main recommendation is to standardize how the data are shown. Figures 4 and 5 are fairly consistent in regard to the speed and elevation timeseries but Figure 2 is very different. Why are the data in Figure 2 shown at so many points along the centerline but the other figures show data averaged over boxes in select locations? I like the how the data are shown in Figure 2 a bit more but you could have the data points with a larger spacing so that the speed timeseries are easier to interpret. Then I would show the sampling ROIs as boxes like in Figures 4 and 5. For those figures, it would be helpful to show speeds for regularly-spaced points along the centerline instead of at a single location to get a sense for surge propagation.	We chose to show more points on Whisky Glacier than on Gourdon or Kotick glaciers simply because the data quality and availability are much greater at Whisky Glacier. But we agree that this has resulted in a confusing switch in structure between the two sets of plots. In the revised version, we have reduced the number of points shown for Whisky Glacier and have added in ROIs along the centreline of Gourdon and Kotick Glaciers, as you suggested. The updated figures also show the outlines of the ROIs, rather than just the centrepoint.
10	lines 311-312: Please add references to the surges at Sít' Kusá in Alaska as well, which becomes marine-terminating when it surges but is mostly protected from the ocean during quiescence by a subaerial shoal. It is also in Alaska and has ~1.5 year-long surges and has been well-studied in recent years. See Nolan et al. (2021; doi:1017/jog.2021.29) and Liu et al., (2024; doi: 10.1017/ jog.2023.99)	There are many tens of papers that describe the characteristics of the surge phase of individual land-terminating and tidewater glaciers, any of which we could cite here. We think the citation of Guillet et al. (2025) is ideal and sufficient for our statement here, because it demonstrates the differences in duration and shape of the surge phase at land-terminating and tidewater glaciers by observing many glaciers in a single dataset.

11	lines 363-366: You may want to point back to Variegated Glacier here as well because the Eisen references that you mention earlier point to changes in Variegated's recurrence interval over time and the glacier is notably "overdue" for a surge.	We are not aware of any studies linking changes in surge frequency of Variegated Glacier to climate change, though the unusual length of this quiescent phase is really intriguing. The 2003/2004 surge of Variegated Glacier occurred early following the smaller surge in 1995, which followed an anomalously warm two-day period. Similarly, a surge of Tunabreen occurred early and coincided with an extreme rainfall event. We chose not to include these examples because they appear to be single-cycle disruptions caused by unusual weather, whereas the paragraph and subsequent discussion focus on progressive changes in surge cycle duration due to climate changes.
12	Figure 7: I really struggled with some aspects of this figure. I think panel a is interesting and I follow the scatterplot and underlying contours, but the inset was a bit more difficult to interpret. The horizontal position of the inset is independent of precipitation, correct? It is just the vertical positioning that matters? I would move it to a location along the right side of the plot rather than within the plot. Since I recommend that the broader Antarctic SCE discussion is dropped, I would eliminate panels b, d, e. If you keep the Antarctic SCE interpretation, at least remove panel b.	You're correct that the inset contains no information about precipitation, and we can see how including it within the panel was confusing. We have now moved it outside of the panel, to the right. We agree with your suggestion to remove the broader Antarctic SCE discussion, so we have removed panels b, d and e, leaving only a time-series for the Antarctic Peninsula.
13	line 474: Here and elsewhere you say the SCE "contracted" southward. I recommend rephrasing to say it "shifted".	Done.
14	Throughout the discussion and the figures therein, I recommend limiting the focus on the SCE to the glaciers along the peninsula. This trimming will keep the paper more focused and is more appropriate given that the SCE may not be valid for the ice sheet.	Another reviewer also recommended reducing the scope of the SCE discussion for similar reasons and we agree that it may not be valid for the ice sheet and ice streams. In the revised manuscript, we now focus the SCE discussion only on the Peninsula. Specifically, we removed all sections of text referring to areas outside of the Peninsula, which were all in Section 4.4.

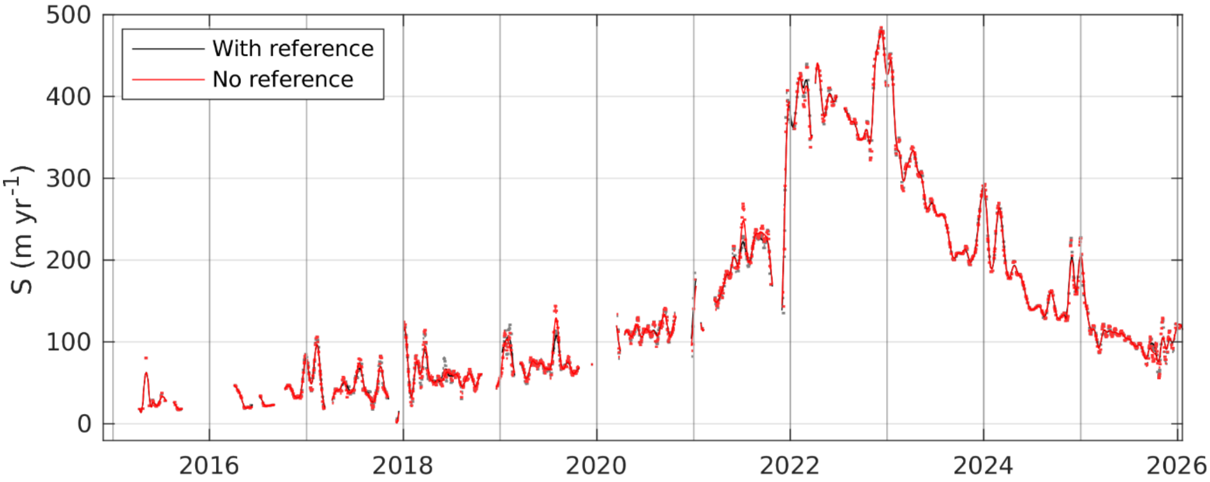

Figure R1. Time-series of Sentinel-1 ice velocity extracted ~2 km upstream of the pre-surge terminus of Whisky Glacier. The grey points and black line show the data extracted using the method described in the main text (using a long-term average velocity map to fill data gaps within the region of interest). The red points and line show the data when no long-term average velocity map is used and gaps are not filled.

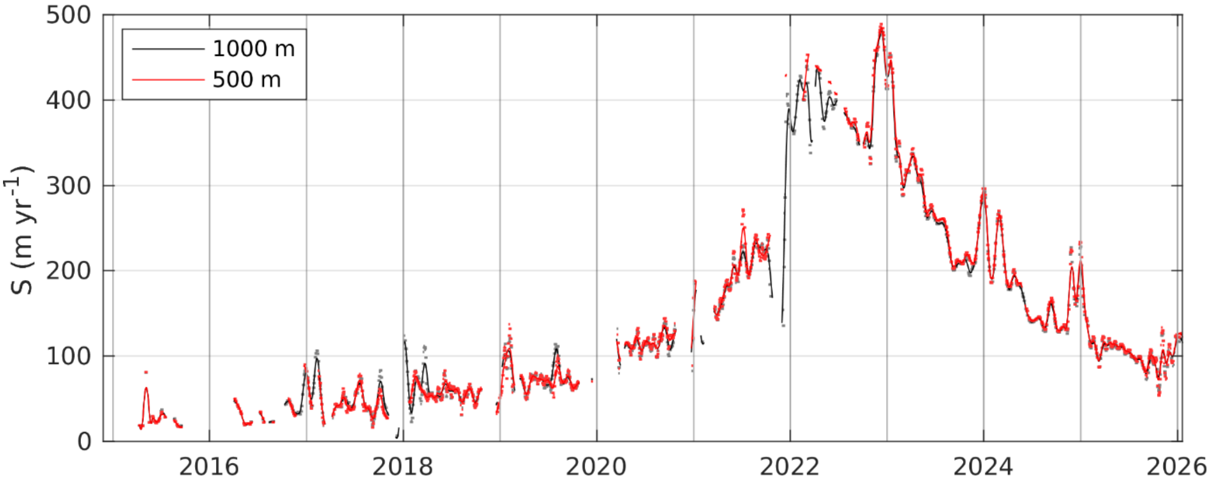

Figure R2. Time-series of Sentinel-1 ice velocity extracted ~2 km upstream of the pre-surge terminus of Whisky Glacier. The grey points and black line show the data extracted from a 1000x1000 m ROI (as in the main text). The red points and line show the data extracted from a 500x500 m ROI.