Authors' response to reviews

April 10, 2026

Reply to the Editor: Thank you for your thoughtful feedback and for the opportunity to revise our manuscript. We appreciate the reviewers' constructive comments, which have helped us strengthen the study. We believe our revisions address the reviewers' concerns and further enhance the manuscript's rigor. Please find our point-by-point response to each comment in the following pages. **The figure, table, section and line references in our replies refer to the track-change document.**

Thank you again for your guidance. We look forward to your feedback on this revised version.

Best regards,

Loris Danjou, Eero Rinne and Erik Schytt Mannerfelt

1 Response to Reviewer 1

1.1 Major comments

Comment 1: The manuscript titled 'Svalbard glacier calving front retrievals during the 1960s and 1970s from archived Landsat and declassified intelligence satellite photographs' presents a valuable reconstruction of calving front changes for all Svalbard tidewater glaciers using historical satellite data (KH-5 ARGON and Landsat) and aerial imagery from the 1930s. This is the first study using Declassified Intelligence Satellite Photographs (DISP) over Svalbard, and the authors conduct a thorough and detailed analysis of the accuracy of remote sensing source materials. Despite the limitations of the DISP data, the results provide valuable insights into the average retreat for the entire archipelago between the 1930s, 1960s and late 1970s. Furthermore, it provides detailed information on the behavior of individual glaciers. The freely available dataset of georeferenced imagery and digitized calving fronts represents a significant contribution that can be highly valuable for future glaciological and climate-related studies.
Based on the review, it is recommended that the manuscript be considered for acceptance with minor revisions. I found it interesting that the retreat in the study period is lower than the values of other authors for slightly later periods. As widely recognized, sea temperature, air temperatures and sea ice cover are among the most significant factors influencing glacier front positions. I suggest underlining it in the paper and discussing the results with the climatic data and reanalysis for the region.

Reply 1: We added the following paragraph and Fig. 9 to Section 4.1 (l. 299–305), which discusses the results with the climate reanalysis ERA5.
"Figure 9 displays the time series of 2-meter temperature (T2m), sea surface temperature (SST) and sea ice concentration (SIC) from the ERA5 reanalysis (Copernicus Climate Change Service 2019), yearly averaged over Svalbard (74–81°N, 7–35°E) between 1940 and 2023. The graph clearly shows an overall increase in T2m and SST, and a decrease in SIC over the whole period. In fact, the average T2m is -6.42°C for 1940–1978 and -4.05°C for 1979–2023, the average SST is 0.42°C for 1940–1978 and 1.23°C for 1979–2023, and the SIC is 41.93% for 1953–1978 (no SIC data is available

prior to 1953) and 25.58% for 1979–2023. Therefore, the evolutions of all three variables support an acceleration of the calving fronts' retreat after 1978 (Luckman et al. 2015; Otero et al. 2017; Błaszczyk et al. 2023) and are consistent with our results."
To clarify methodological differences between the different studies our results are compared to, we kept "However ... widths" (l. 306–311) as a separate paragraph.

Comment 2: In my opinion, all three glaciers, Emmabreen, Schweigaardbreen , and Fonndalsbreen, share similar characteristics; however, the presented results and discussion are insufficient to determine whether they are surging glaciers. I would suggest classifying all of them as advancing glaciers with the possibility of surging.

Reply 2: We appreciate the reviewer's request for further clarity and took steps to improve accordingly. There is indeed ambiguity in the interpretation of glacier advances as being surges or not, unless other data are provided. To this end, we revisited the glacier-specific geodetic mass balance values of Geyman et al. (2022) for each glacier. Allfarvegen, Emmabreen and Stonebreen (the discussed surges/advances in the text) lost mass at 0.26–0.44 ($\pm$ roughly 0.2) m w.e. a^{-1} from 1936–2010, while Fonndalsbreen had a small potential mass gain of 0.07 ± 0.09 m w.e. a^{-1}. Schwiegaardbreen was not covered by the dataset, but we assume similar conditions to Fonndalsbreen due to its proximity ($\sim$50 km on the same ice cap and aspect), and similarity in more recent elevation change rates (Moholdt et al. 2010). Because of the potentially positive mass balance of Fonndalsbreen and Schwiegaardbreen, we refrain from definitively calling them surges, since they may also be entirely mass balance driven. The other glaciers, however, are extremely difficult to explain by other means than surging, since these advances occur during strongly negative mass balance conditions. There could theoretically have been periods of positive mass balance in between, but this is not supported by individual glacier mass balance series, at least since the 1960s (Schuler et al. 2020). It should also be noted that regional inventories such as Harcourt et al. (2026) use substantial frontal advances outside of positive mass balance anomalies as direct indications of surging. Therefore, while we agree in principle on the point of ambiguity, our classification follows established criteria.
We refined the manuscript in revision to 1) include more information on mass balance from Geyman et al. (2022), 2) clarify that the advances Fonndalsbreen and Schwiegaardbreen could either be surges or not, but we refrain from further classification, and 3) better define our surge classification strategy to be fully transparent (l. 329–330 and 344–348).

Comment 3: Additional comments include maintaining a consistent chronology throughout the text and in the legends of figures, as well as avoiding short paragraphs (two to three sentences) and combining them effectively to present your results clearly. Please also consider including the aerial photos from the 1930s in the manuscript title, as they are a primary source in the presented research.

Reply 3: We made sure the chronology remained consistent in the text and figures, and removed or merged short paragraphs. We changed the title for "Svalbard glacier calving front changes from the 1930s to the 1970s from archived satellite and aerial images".

1.2 Minor comments

Comment 4: L2 and the title - please consider changing the word 'retrieve' to 'obtain/digitalize' (or another alternative) as an option.

Reply 4: We changed this sentence to: "We obtained calving front locations of 171 Svalbard tidewater glaciers from archived satellite imagery." (l. 1–2).

Comment 5: L3 – The paper does not discuss geophysical analyses of the calving fronts. Please rewrite this sentence

Reply 5: We removed "geophysical".

Comment 6: L25 – Explain what (+1.5°C) and (+4°C) mean.

Reply 6: These temperatures correspond to mean temperature increase projection by 2100 Rounce et al. (2023). This information have been added to the manuscript (l. 27–28).

Comment 7: L25-27 – The statements are partially inaccurate simplifications and may be interpreted as implying that glacier mass loss acts as a driver of global warming. However, glacier mass loss primarily represents a response to climate warming and may subsequently contribute to local and regional amplification of warming through positive feedbacks, mainly related to changes in surface albedo. Please consider rewriting.

Reply 7: This is indeed an imprecise statement. We rewrote this sentence as: "In the Arctic, glacier mass loss contributes to changing the surface albedo, which amplifies global warming though positive local and regional feedbacks." (l. 29–30).

Comment 8: L29 – Avoid using acronyms (e.g., ECV) when they are used in the text only once or twice. ECV is not necessary in L 31.

Reply 8: We removed this unnecessary abbreviation.

Comment 9: L32 – '...since Svalbard glaciers we study are constrained by terrain in all other directions.' Actually, glaciers also change their area in the upper parts, which is especially visible in long-term analyses. Please consider rewriting or omitting that part.

Reply 9: This sentence was not critical for the understanding of this study's motivations; we removed it.

Comment 10: L33 – I would suggest describing data sources chronologically, starting from aerial photos

Reply 10: This is indeed more logical; the data sources are now presented chronologically (Sect. 1.2 – 1.4).

Comment 11: L52 – What do you mean by 'a retreating trend' here?

Reply 11: This sentence means the marine-terminating glaciers have globally retreated during the study period, with a rate of $23.78\ \mathrm{km}^2\ \mathrm{yr}^{-1}$. We rephrased it in these terms (l. 83).

Comment 12: L69 – Worldview => WorldView
L77 – campaigns been => campaigns have been

Reply 12: These typos have been corrected.

Comment 13: L82 – Geyman et al. (2022) give units in water equivalent ($-0.28\ \mathrm{m}\ \mathrm{yr}^{-1}$ of water equivalent)

Reply 13: The unit was mis-written and has been corrected (l. 40).

Comment 14: L86 – The word “historical” may suggest that the research itself is old, whereas the studies are sometimes modern but use older data. A clearer phrasing might be “..to fill gaps in past observations of Svalbard glaciers...”

Reply 14: We have repharsed this sentence as: "The objective of this study is to bridge the gaps between past observations of Svalbard glaciers, by retrieving glacier calving fronts from DISP (1962–1963) and Landsat (1976–1978) images, and comparing the observations to historical aerial image calving front data (1936–1938)." (l. 127–129).

Comment 15: L88 – Regarding years 1936-38, please mention here the orthophotos of Geyman et al. (2022).

Reply 15: We mentioned here these aerial images (l. 129).

Comment 16: L 104 – Please consider rewriting Section 2.2.1 slightly. Actually, you do not use images covering the period from 1960 to 1972 in your studies (L 109), but only ARGON data from 1962 and 1963. Leave in this section only information relevant to the ARGON mission. It is not clear from the table and text what the final resolution of pixels is: 30 m or 140 m? Is Table 2 necessary? It suggests that you use all this data in your research. Consider reorganizing chapters 2.1–2.3 to describe data in chronological order.

Reply 16: We reorganised sections 2.1, 2.2 and 2.3 to present the data in chronological order.
We corrected section 2.2.1, that only mentions the data used in this study:

"Between 1959 and 1984, the US intelligence agencies operated a series of reconnaissance satellites as part of a program called KeyHole (KH). The photographs were taken on panchromatic film, sent back to Earth in capsules that were retrieved mid-air by the US Air Force, and developed by the Central Intelligence Agency. In total, 1,660,000 satellite images were declassified in three stages in 1995, 2002, and 2011. Today, a copy of these films is conserved by the USGS, which can provide high-quality scans for $30 per frame at the time of writing (U.S. Geological Survey 2018). Once the films are scanned, the images remain freely available on the EarthExplorer website. These images are highly valuable for historical studies and are referred to as DISP in the remote sensing scientific community.
In this study, we focus on eight KH-5 ARGON images, from missions 9034A (May 1962) and 9058A (August 1963). An example is given in Fig. 2. The satellite, whose altitude was between about 300 and 600 km, was equipped with a mapping camera, covering an area of about 540 $\times$ 540 km, with an approximate resolution of 140 m. The images were taken on 127 mm Kodak 3400 film, with a 76.2 mm focal length and an f/2.5 aperture (Burnett, M. G. 1982). The scan resolution is $7\,\mu\mathrm{m}$ (U.S. Geological Survey 2018; Molnár et al. 2021), resulting in a ground-projected pixel size of about 30 m on the digital copy (note the resolution is 140 m but the sampling scale is finner). These images were taken in three different series (two on 16 May 1962, and one on 29 August 1963). Two consecutive images of the same series have an about 60% overlap."

Table 2 was removed.

Comment 17: Figure 1 – Consider rewriting: ‘a mosaic generated from two consecutively acquired images’. Here, and in other figures, please add a dot at the end of each sentence.

Reply 17: We re-wrote the caption exactly as suggested. We added the missing dots in all captions.

Comment 18: L123 – Consider changing ‘deformations are due to parallax’ to ‘deformations arise from terrain-induced parallax...’ to underline that the mountainous terrain of Svalbard influences deformation of images.

Reply 18: We added this precision.

Comment 19: L126-129 – It is not clear how you processed the images ('...a full orthocorrection unnecessary. Instead...'). Was orthocorrection or only a 3rd-degree polynomial transformation applied?

Reply 19: The images were not orthorectified. Instead, they were georeferenced using a 3rd-degree polynomial transformation. We rephrased these sentences as:
"However, as we were only interested in obtaining glacier fronts that adjoin water surfaces, a full orthocorrection was deemed unnecessary and therefore not applied. Instead, we georeferenced the images using a 3rd degree polynomial transformation on QGIS, with 100 to 200 ground control points (GCPs) per image, account for the complex geometry of KH-5 ARGON photographs." (l.169–173).

Comment 20: L148 – If you cite any name, it should be shown on the map or figure. Please show Rijpbreen in the image as an example or remove the name from the text.

Reply 20: The name of Rijpbreen has been added to a map (cf. Fig. 4 and Reply 22).

Comment 21: L154-155 – The whole sentence is not clear. What is the main aim? what is simplified? do you really quantify the glacier motion or just front position changes?

Reply 21: We rephrased this sentence as:
"This study focuses on quantifying glacier frontal changes. Since calving fronts may have a complex geometry and may not move uniformly, we focus on reducing the problem to quantifying their position change along the glacier flow axis." (l. 209–211).

Comment 22: Figure 4. Unfamiliar readers do not know where Hornsund is. Please present it on the Svalbard map, e.g., in Figure 1. Please rewrite the sentence 'Example of split Landsat front in Hornsund' to be more understandable, e.g., 'Example of split of glacier fronts in Hornsund (background: Landsat)'. Add the date of glacier fronts from RGI. Reorder the legend to be chronological. Cite the RGI in the figure caption. Change Mendeleevbreen to Mendelejevbreen.

Reply 22: Reviewer 2 suggested adding a figure presenting the extent of the dataset (Fig. 4). We presented Hornsund on this figure. A reference to this figure has been added to the caption of Fig. 5.
Figure 5 is a corrected figure. The legend has been reorganised in chronological order, and the caption has been re-written as suggested. The glacier outlines from RGI are not precisely dated, but correspond approximately to year 2000.

Comment 23: L182 – Is the 'y' axis correct in Figure 5? (in L265 you mention retreat 7300 m and 7600 m)

Reply 23: The 'y' axis is correct on Fig. 6. The most significant retreats of Negribreen and Nathorstbreen correspond to the lower gray lines on the graph. To improve visibility, the axis is limited to -2000 m.

Comment 24: L184 – What do you mean by 'This length loss'? Is the sum of the linear retreat of all glaciers?

Reply 24: It corresponds to the average glacier linear retreat. We replaced "This length loss" by "This average glacier linear retreat" (l. 253).

Comment 25: L185 and further – Do you analyze the retreat rate? or just faster/slower retreat?

Reply 25: We simply acknowledge that the retreat rate is slower between 1962–1978 compared to 1936–1962. We choose not to further analyse these trends because of the overall high uncertainties. In the discussion, we compare the retreat rate over the whole period to that calculated by Li et al. 2025.

Comment 26: L191 – Maybe start the sentence with 'The area loss between 1936-1978 was...'

Reply 26: We rephrased this sentence as requested (l. 260).

Comment 27: L201 – 'time span ...is high' – you mean short? long?

Reply 27: We meant the time span is long. We corrected this sentence: "Finally, more significant observations are made when the uncertainties on front locations for both years are low, and when the time span between the observations is long." l.271).

Comment 28: L202 – add the studied time span in the first sentence.

Reply 28: We rephrased: "We identify a total of 15 advances and 122 retreats between 1936 and 1978." (l. 273).

Comment 29: L206 – What do you mean by 'both"? Rewrite the sentence to explain the two behaviors in the main part of the sentence, not in the brackets.

Reply 29: The two types of behavior described in this sentence are an advance and a retreat. We rewrote these sentences as:
"One can observe on these maps that a specific front can show both advance and retreat behaviors. For instance, Hilopenbreen retreated between 1938 and 1962–1963 and then advanced between 1962–1963 and 1976–1978, and Freemanbreen advanced between 1936 and 1963 and then retreated between 1963 and 1976. Also, an advance/retreat might not be uniform along the front (e.g. Stonebreen), which we bring up further in the discussion." (l. 276–280).

Comment 30: The entire section 3.2 – please avoid using short paragraphs and try to combine them properly to present your results effectively.

Reply 30: We have combined these paragraphs.

Comment 31: L210-212 – This sentence is not logical. Why was the advance of Recherchebreen detected only between 1936 and 1978? in Figure 7 it looks like advance was between 1936-1962?

Reply 31: Indeed, a visual inspection of the fronts supports an advance of the front between 1936–1962. However, the uncertainties on the 1962 retrievals are much higher compared to those of 1978. Therefore, the distance between the 1936 and 1962 fronts is not significant to conclude to an advance, whereas the distance between the 1936 and 1978 fronts is lower but significant when compared to the displacement uncertainty. Thus, the advance is only detected between 1936 and 1978.
We rephrased this sentence (l. 281–288).

Comment 32: Figure 7. Please reorder the legend chronologically and present the positions of all glaciers on the Svalbard map.

Reply 32: The toponyms have been added to Fig. 4, and the legend is now in chronological order. Figure 8 is the corrected figure.

Comment 33: Section 4.1 – Cite the accuracy of your results. It is worth noting that your findings are slightly lower than those of Li et al. (2025) and Kochtitzky and Copland (2022). This is a very interesting finding, as your research covers an earlier period. Please discuss it in conjunction with the climate in the period of your study and that of the other authors.

Reply 33: As emphasised by Reviewer 2, the R^2 (as was used before to assess accuracy) can be misleading. We have therefore re-calculated trends using a weighted least squares method (rather than the initial ordinary least squares) to take into account the uncertainties' variations from year to year, and calculated trend uncertainties. The corrected global trends (and uncertainties) are:

- Average retreat rate (1936–1978): $-27.3 \pm 3.0\ \mathrm{m\ yr^{-1}}$
- Average area-loss rate (1936–1978): $-17.0 \pm 2.5\ \mathrm{km^2\ yr^{-1}}$

The discussion of these results has been completed by the analysis of the climate reanalysis parameters presented in Reply 1.

Comment 34: L225 – Be precise in the last sentence. Do you describe your or Kochitzky's methods?

Reply 34: This sentence describes Kochitzky's method. It has been clarified (l. 311-314).

Comment 35: Section 4.2 – I miss the consistency in this discussion and conclusions. On what basis were the three glaciers (Emmabreen, Schweigaardbreen and Fonndalsbreen) divided into two separate categories, surging and advancing? In my opinion, you classify Emmabreen as a surging glacier just because it was mentioned as surging later in time (see L241) by Harcourt et al. (2025). But Lovell and Boston (2017) also suggest that Schweigaardbreen would be a surge-type. I agree that the positive mass balance would obscure the interpretation of front position changes for Schweigaardbreen and Fonndalsbreen, but the study period is very long, making it difficult to prove this. It is also worth noting in the manuscript that both glaciers originate from the ice cap Austfonna, which can influence glacier dynamics. Anyway, to document glacier surging, further studies are needed (DEM differentiation, velocity measurements or other indirect evidence). As you do not present any such evidence in all three cases, I would suggest combining sections 4.2.1 and 4.2.2 into one: 'Advances of Emmabreen, Schweigaardbreen and Fonndalsbreen', with the note that a surge of all three glaciers may have occurred during the period under study.

Reply 35: Please see Reply 2 for a detailed answer and correction.

Comment 36: L232 – for consistency with the first sentence in the paragraph, shouldn't be 'cf. Sect. 4.2.1' cited here as well?

Reply 36: Indeed, Sect. 4.2.1. should be also cited, and it is in the corrected manuscript (l. 318–319).

Comment 37: L234, 235 – I suggest using the term 'bay' instead of 'fjord' in this case

Reply 37: The term "bay" sounds indeed more appropriate and replaces "fjord" in the revised manuscript (l. 321–322).

Comment 38: L237-238 – cf. L123

Reply 38: We refered to section 2.2.2 (l. 326).

Comment 39: L246 – You write: ‘ The 1962 DISP images are too inaccurate to narrow down the time frame of the advance.’ So why do you present the 1962/63 position of the glacier in Fig. 7? It suggests that the surge was before 1962/63.

Reply 39: We believe that the corrections suggested in Reply 31 resolve this ambiguity. The front displacement is too small compared to the positioning uncertainty of the 1962–1963 fronts to conclude to a significant advance. However, the front location is consistent with an earlier surge, and is therefore presented on the figure.

Comment 40: L247 – ‘Therefore, we conclude that...’ – the sentence is not logically connected with the previous sentence.

Reply 40: We swapped "The 1962 DISP images are too inaccurate to narrow down the time frame of the advance." and "Therefore, we conclude that our observations are compatible with a possible surge of Schweigaardbreen." to restore the logical consistence of the paragraph (l.337–338).

Comment 41: L248 – ‘They share a vague resemblance..’ – the sentence is not logically connected with the previous sentence. What do you mean by ‘they’? Comparison with the Canadian Arctic glacier is not relevant here, in my opinion.

Reply 41: "They" here refers to Schweigaardbreen and Fonndalsbreen. We removed this sentence.

Comment 42: L282-286 – Clarify ‘subsequent time period’. Do not repeat all numbers from results and discussion, but try to describe the main outcomes - see comments to Section 4.1.

Reply 42: The "subsequent time period" refers to 1985–2023, which is the time frame of the study of Li et al. (2025). We replaced this expression with the precise years (l. 381). We also added the conclusions of the climate reanalysis comparison presented in Reply 1 (l. 385).

Comment 43: L312 – false color => false color composite

Reply 43: We corrected exactly as suggested (l.418).

2 Response to reviewer 2

2.1 General comment

This manuscript presents a historical reconstruction of calving front positions for 171 tidewater glaciers in Svalbard over the period 1936–1978, based on a combination of historical aerial photographs, early Landsat MSS imagery, and declassified KH-5 ARGON satellite photographs. The extension of calving-front observations into the pre-1980 satellite, together with the public release of georeferenced imagery and digitized fronts, gives this study clear long-term value as a data resource for Svalbard glacier research.

However, the scientific contribution of the manuscript is currently not articulated as clearly as it could be. The Introduction largely reads as a chronological listing of previous studies rather than a focused synthesis that motivates why this reconstruction is needed, what specific gap it fills relative to existing datasets, and how it advances current understanding. In addition, although 171 glaciers are mapped, the manuscript lacks a clear overview figure demonstrating the spatial coverage of the dataset, which makes it difficult to fully appreciate the scope and representativeness of the work.

I also have several major concerns related to (i) the treatment and interpretation of georeferencing uncertainties, (ii) the robustness of inferred average retreat and area-loss rates given the sparse temporal sampling, and (iii) the framing of some conclusions, which in places extend beyond what the data can securely support. Addressing these issues would substantially strengthen the scientific rigor, clarity, and long-term usability of the dataset. Overall, I recommend major revision.

Reply 0: We thank the reviewer for this review. The comments are detailed and encouraging, and helped improving the quality of the manuscript.

2.2 Major comments

Comment 1: The study relies heavily on polynomial georeferencing of KH-5 ARGON images using many ground control points (typically 100–200 per image), while explicitly abandoning a physically based orthorectification approach (Sect. 2.2.2). Although the authors justify this choice by focusing on relative calving-front positions near sea level, the implications of this decision require a more critical and transparent discussion. High-order polynomial transformations can provide a good local fit at GCPs but may introduce non-physical distortions between control points, particularly over very large scenes such as ARGON images (>500 km wide). The manuscript also acknowledges that uncertainties in 1962–1963 are especially large, yet several key interpretations (e.g. identification and timing of advances, surge attribution) rely strongly on these data. Further clarification is needed:

- Provide quantitative diagnostics of georeferencing performance (e.g. RMSE at GCPs and, if possible, spatial patterns of residuals).
- Explicitly discuss how non-uniform geometric distortions could bias centerline-based distance estimates, especially for wide or curved glacier fronts.
- Clarify whether any spatial filtering or systematic exclusion (e.g. image edges or areas affected by severe distortions) was applied, beyond individual case decisions such as Emmabreen.

Without this information, it is difficult to assess whether some detected advances or retreats can be confidently distinguished from residual geometric artefacts.

Reply 1: Since our uncertainty assessment relies on direct comparison of front observations, it takes into account georeferencing errors, delineation precision and human mistakes at glacier locations. Therefore, georeferencing errors, as well as their influence on the centerline-based distance estimates and their propagation to the glacier front locations are already accounted for. The GCPs are chosen over land areas that are unaffected by time (e.g. mountain peaks) spread over the whole fraction of the archipelago visible on each image. In addition, all fronts are within the bounds of the GCPs, so there should be no risk of extensive extrapolation. A paragraph has been added to section 2.6 (l. 335–339) to clarify this.
To provide a more detailed analysis, Fig. B2 displays the GCPs' residuals. One can observe there is no clear similar pattern between the images. The standard deviations of front positions we calculated by comparing fronts from the same year are respectively 360 m for 1962 and 182 m for 1963. We doubled these values to get a 95% confidence interval, therefore obtaining positioning uncertainties of 720 m for 1962 and 364 m for 1963. As predicted, these values clearly overestimate the RMSE calculated on the GCPs. This information has been added to Appendix B2.
No systematic filtering was applied to the calving fronts. If unidentified systematic biases exist, they are therefore included in our uncertainty assessment.

Comment 2: The manuscript reports an average retreat rate of 26.3 m yr^{-1} between 1936 and 1978 and discusses variations within this period (Sect. 3.1, Fig. 5a). However, the effective temporal resolution is extremely limited, with only three broad observation windows (1936–1938, 1962–1963, and 1976–1978). Given this sparsity: 1) Linear trends and apparent phases such as "plateaus" or accelerations are poorly constrained. 2)The reported R^2 values may give a misleading impression of robustness. I suggest that the authors:

- Reframe retreat and area-loss rates explicitly as period-averaged net changes, rather than temporally resolved trends.
- Avoid interpretative language implying acceleration or deceleration unless supported by sensitivity tests.
- Emphasize net changes between observation epochs as the primary result, with trends presented only as descriptive summaries.

Reply 2: We thank the reviewer for noting that R^2 values may be misleading. We recalculated the global trends using a weighted least squares (WLS) method and computed their 95% uncertainties:

- Average retreat rate (1936–1978): $-27.3 \pm 3.0 \text{ m yr}^{-1}$
- Average area-loss rate (1936–1978): $-17.0 \pm 2.5 \text{ km}^2 \text{ yr}^{-1}$

These values remain consistent with the previously attained trends.
Figure 6 is a corrected version of Fig. 5 (in the preprint) that displays the general trends (in blue) calculated with the WLS. There is an apparent difference in rates of change between 1936–1962 and 1963–1978, indicating that retreat may have been faster in the early period. Due to the uncertainty of the 1962–1963 measurements, however, a constant linear retreat throughout the entire study period would remain consistent with our observations. We therefore acknowledge differences between 1936–1962 and 1963–1978 are difficult to assess robustly and clarified this (l. 251–253).

Comment 3: Area change is calculated as the product of frontal displacement and average glacier width (Sect. 2.5, 3.1), following Li et al. (2025). While this approach is reasonable given the data limitations, it differs fundamentally from outline-based methods used in other studies cited for comparison (e.g. Kochtitzky and Copland, 2022). Although these differences are acknowledged, the manuscript still draws relatively strong comparative conclusions (Sect. 4.1, 5). I therefore recommend that the authors more clearly separate methodological differences from physical interpretation when comparing results across studies, explicitly discuss how width variability, fjord geometry, and non-parallel retreat may bias area estimates, and consider presenting cumulative frontal length change alongside area change as a more methodologically neutral metric.

Reply 3: We believe the methodological considerations are already separated from the physical interpretations. Note that Li et al. (2025) obtained similar results to those of Kochtitzky and Copland (2022), for the same study area and similar time frames, in spite of the methodological differences. In fact, Li et al. (2025) assessed the possible error sources arising from the comparison of results based on the two methods. They concluded that it remained valid in order to assess a large scale trend rather than to give a precise estimate. This section of our study being limited to this objective, we believe the comparison is valid. We further highlighted this discussion in the revised text (3. 311–314).

Comment 4: The identification of surge events is one of the most interesting aspects of the manuscript. However, the criteria used to distinguish surges from non-surge advances are not always clearly defined. For example, Schweigaardbreen and Fonndalsbreen are treated cautiously as advancing glaciers, whereas Emmabreen is classified as having undergone a previously undocumented surge, largely based on frontal advance magnitude and geometry. Given the long time intervals between observations, this distinction remains ambiguous. I recommend that the authors explicitly state the diagnostic criteria used to classify an event as a surge versus a non-surge advance, clarify the relative roles of frontal advance magnitude, spatial coherence, and ancillary evidence (e.g. crevasse patterns or later velocity observations), and acknowledge that, in some cases, surge classification remains uncertain given the available data. This would improve the conceptual consistency of Sect. 4.2 and help avoid over-interpretation.

Reply 4: We thank the reviewer for noting this imprecision; which was also noted by Reviewer 1. We refer to Response to Reviewer 1, Reply 2 for a detailed answer and changes.

2.3 Minor comments

Comment 5: Consider using the term "delineation" rather than "retrieval" consistently throughout the manuscript.

Reply 5: We thank the reviewer for noting these inconsistencies, We have replaced the occurrences of "retrievals" by "delineations".

Comment 6: The Abstract and Introduction would benefit from clearer separation between background, methods, and the specific contribution of this study. The Introduction (particularly Sect. 1.2) could be substantially condensed; rather than listing previous studies, it should synthesize why DISP and early Landsat data are essential for the present work.

Reply 6: We rebranded the introduction to condense the references rather than listing the studies.

Comment 7: Use "calving-front position" rather than "locations" for consistency with common terminology.

Reply 7: We made the necessary corrections.

Comment 8: An overview map showing all 171 calving fronts is necessary to demonstrate the spatial coverage of the dataset.

Reply 8: Figure 4 shows all the glaciers represented in the dataset. As Reviewer 1 noted that specific Svalbard locations might be unknown from non-specialists, useful toponyms have been added on this map too.

Comment 9: Clarify how many fronts are shown in Fig. 3 and how this relates to the total number of observations.

Reply 9: Figure 3 shows one front, as Fig. 1 and 2 do (figure numbers refer to the preprint here). We added a brief clarifying sentence to all figure captions, stating "The green line represents the digitized glacier front." in this caption specifically.

Comment 10: Several statements describing "faster" or "slower" retreat (e.g. Lines 184–186) are difficult to support given the sparse time sampling and should be revised.

Reply 10: Please refer to Reply 2 for a detailed answer.

Comment 11: Multi-panel figures (e.g. Fig. 7) are information-rich but visually dense; inset maps or simplified color schemes could improve readability.

Reply 11: Reviewer 1 also noted that Fig. 7 (in preprint) should be reorganised. We have rebranded the color schemes and improve readability. Fig. 8 is the corrected figure.

Comment 12: 2.5 notes that centerlines were digitized manually; please clarify whether a single operator performed this task and comment briefly on consistency.

Reply 12: All the centerlines and fronts were digitized by the same person. The centerlines are therefore consistent between the different glaciers. This information has been added in the revised text (l. 192).

Comment 13: The uncertainty framework is thorough, but a concise summary table of typical positional uncertainties for each data source in the main text would aid readers.

Reply 13: We added a table (Table 2) that summarizes the positioning uncertainties calculated from the standard deviations of distributions of distances between fronts dating from the same year (cf. Appendix B1) for each year, and refered to it in Section 2.6.

Comment 14: Please ensure that metadata in the Zenodo archive clearly document coordinate reference systems and georeferencing procedures.

Reply 14: The coordinate reference systems are clearly referenced in the Zenodo repository. The provided images are already georeferenced.

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