Responses to Comments (C) Reviewer 2 (R2)

General comment R2: This paper investigates the influence of glacier geometry on the recent thinning and retreat of ocean/lake terminating outlet glaciers in the Patagonia Icefields. To analyze the geometrical control on glacier changes, the authors compute the Peclet number along the flowline of 45 outlet glaciers. This approach has been proposed by a previous study (Felikson et al., 2017) and applied to glaciers in Greenland and in Svalbard (Felikson et al., 2017; 2020; Zheng, 2022).

Ocean and lake terminating glaciers in Patagonia are rapidly losing mass under the influence of increasingly negative mass balance as well as the ice-ocean/lake interaction. This is a similar situation as in Greenland, although the climate and glaciological settings in these regions are substantially different. Therefore, the application of the recently proposed analysis to Patagonia is interesting and potentially important to better understand the current and future mass loss of the glaciers. As far as I know, this is the first time that Peclet numbers are analyzed for glaciers in Patagonia.

Despite the novelty and potential importance of the study, it is difficult to understand the findings and implications of the study. The way of presentation is one reason, but I suspect fundamental problems in some of the analyses. The manuscript suffers from unclear text and equations, which get in the way of understanding. I list below my major concerns, which are followed by specific/minor comments on the manuscript.

Response to general comment R2: We thank the reviewer for valuable comments and for recognizing the novelty and potential importance of our study on the influence of glacial geometry on the recent thinning and retreat of marine- and lake-terminated glaciers in the Patagonian Ice Fields.

In the revised version of the manuscript, we have made substantial changes to address concerns raised about the clarity and rationale of the analysis. We completely restructured the methodology and results sections to provide a more precise and straightforward presentation, focusing on our main finding: identifying the empirical thinning limit at $Pe \leq 21$, which encompasses more than 95% of ice thinning. In addition, we significantly strengthened the database by using the more recent and robust set of subglacial topography developed by Fürst et al. (2024), which incorporates more than 1.4 million thickness observations.

The analysis has been simplified and strengthened by using single centerlines instead of averages of multiple streamlines, incorporating statistical analyses to validate the robustness of the empirical boundary, and establishing more explicit connections between glacier geometry and observed thinning patterns. Figures have been redrawn to illustrate our main findings directly, and the discussion has been refined to focus on the most significant implications of our results.

These changes have resulted in a more precise and focused manuscript, which we believe effectively communicates our main findings and their implications for understanding the current and future evolution of the Patagonian Ice Fields.

Major comments/concerns

R2C1: Data analysis and presentation. The authors analyze and present the general trends and statistics obtained from 45 outlet glaciers. Except for maps showing some numbers for each glacier (Figures 5 and 6), readers are not able to see data obtained for each individual glacier. Considering the diversity of glaciers in Patagonia as well as large uncertainty in the bed elevation, showing only statistical values is not convincing and insufficient to draw conclusions. I encourage the authors to look into the details of each individual glacier as performed in previous studies (Felikson et al., 2017; 2020; Zheng, 2022). The Peclet number is a value computed from glacier geometry and ice dynamics. To use it as a measure of glacier stability, investigation of the observational data used for the computation (bed and surface elevation, ice speed, elevation change) along the flowline is necessary (e.g. Figure 2 in Felikson et al., 2017).

Response to R2C1: We appreciate your feedback, which has allowed us to reflect the results obtained for each glacier analyzed more appropriately. Due to this, we have substantially modified our analysis and presentation of results. In the revised version, we provide a detailed analysis of individual glaciers. We also changed the data sources used, for example, using the more recent and robust data set of Fürst et al. (2024). We also used the model for Pe developed by Felikson et al., 2017 and expanded in Felikson et al., 2021, which does not use surface velocity data. The latter was done because the new versions of the updated velocity products, mainly during 1999-2000, were more strict in quality control, which generated a significant increase in null pixels in regions where we previously had data. This led us to use an approximation that does not directly include this information since these new data made an appropriate analysis impossible. This dataset represents a significant advance in the accuracy of subglacial topography for the Patagonian Ice Fields, reducing previously noted uncertainties.

Following the methodology of Felikson et al. (2017), we have included the analysis of profiles along the central lines showing Pe, surface elevation, basal elevation, and elevation changes. The above has been incorporated into new appendices, the content of which can be easily reproduced using the codes developed in our research, which are available in Zenodo.

R2C2: Bed elevation data. In comparison with Greenland and Svalbard, observations of glacier bed elevation are sparse in Patagonia, and thus, subglacial geometry has a more significant uncertainty. Therefore, a more careful analysis is required for the bed estimated by inversion (Farinotti et al., 2019). As suggested above, investigation of the glacier cross-section and observations along the flowline of each glacier is necessary. As indicated by the authors (Line 625), please also consider using a more recently compiled bed elevation data set (Furst et al., 2024).

Response to R2C2: We appreciate this critical observation regarding the elevation data of subglacial topography. In the current version of the manuscript, we have addressed this concern using the most recent dataset developed by Fürst et al. (2024), which represents a significant advance in the characterization of the subglacial topography of PI. This dataset incorporates over 1.4 million thickness observations to produce the most advanced basal topography map to date. The quality of these data is substantially superior to previous products because they include local measurements that have not been published. Our entire analysis has been recalculated using this new dataset, which provides a more solid basis for our study of the relationship between glacier geometry and thinning propagation. The above has been incorporated into new appendices, including longitudinal profiles, the content of which can be easily reproduced using the codes developed in our research, which are available on Zenodo.

R2C3: Force budget. I understand that the components of the force budget (Equations 3-5) were computed from surface strain rates obtained from ice speed maps. The authors assume full slip condition referring to a previous study (Line 261, Collao-Barrios et al., 2018), but the previous study suggested 98% sliding specifically for the fastest-flowing glacier tongue of San Rafael Glacier. It is not realistic to assume 100% slip condition for all the glaciers and regions extending upglacier.

Further, the presentation and discussion of the force balance analysis are difficult to understand. The authors argue that "glaciers retreat irreversibly when driving stress cannot be supported by the other components of the force balance" (Line 604, similar statement in Line 470). What do you mean by "cannot be supported"? The driving stress should be supported by other stress components as stated in Line 244-245. Actually, "the basal drag is calculated as the residual" (Line 267), thus I assume imbalance does not happen.

Changes in the force balance components near the glacier fronts between 2000 and 2018 are presented in page 23-24. Some numbers show very large changes, represented by >1000% increase in lateral and longitudinal stresses at Penguin Glacier (Line 481). Without detailed analysis, such a rapid change is difficult to accept and cannot be simply connected to the argument "the increase in stress appears to support the Penguin Glacier's relative stability" (Line 482).

Response to R2C3:

We appreciate your important comments on the force balance analysis in our manuscript. After carefully considering these and the other reviewers' comments, we have decided to remove the force balance analysis from the study for several fundamental reasons.

First, we recognize that the assumption of complete sliding based on the specific observations of the San Rafael Glacier is inappropriate for the entire set of glaciers studied. The study by Collao-Barrios et al. (2018) refers specifically to the tongue of the San Rafael Glacier.

In this sense, our original methodology for the force balance adopted an SSA (Shallow Shelf Approximation) type approach with basal drag without including internal deformation. Through this, we sought to capture the dynamics of temperate outlet glaciers, where basal drag longitudinal and lateral stresses can be significant; however, extrapolating a condition of complete basal drag inland on all glaciers might not be appropriate.

Due to the above, we have excluded the force balance from our analysis and focused the manuscript on our main finding: identifying and characterizing the empirical thinning limit based on the Péclet number. This decision has allowed a more straightforward presentation of our main results. As part of future work, we suggest exploring the relationship between the force balance and the observed Pe values using a more rigorous methodological framework that can adequately capture the complex dynamics of the Patagonian Ice Fields glaciers.

R2C4: Percent ice flow. It is hard to understand the "the percent ice flow" defined by Equation 15. CT is "the cumulative thinning from the glacier front in percent" (Line 295), thus Equation 15 gives a mean of CT between the front and the empirical thinning limit. Why do you call this value the percent ice flow? I am not able to follow the analysis and discussion of this value.

Response to R2C4:

We thank the reviewer for highlighting this important conceptual ambiguity in our original manuscript. In the revised version, we substantially modified the terminology and the analysis to avoid this confusion.

We have eliminated the "percent ice flow" concept and replaced it with a more precise and physically meaningful metric: "percent glacier surface area." This change is not merely terminological but more accurately reflects what we are quantifying: the proportion of glacier surface area below the empirical thinning limit ($Pe \leq 21$).

The equations and analysis have been updated to reflect this conceptual clarification in the revised manuscript.

R2C5: Discussion of the data and results. It is a pity that the conclusion of the paper tells not much more than "90% of the ice thinning is occurring below the locations with $Pe<8$" (Line 657-669). This is because the complex data sets presented in the "3 Results" section are not properly discussed in the "4 Discussion" section. In "4.1 Empirical limit ...", no clear interpretation is given to the relatively large Peclet number found for the upper limit of the thinning. "4.2 Evolution Controlled by Upstream Geometry" discusses the advancing Pio XI Glacier, but difficult to find the point of the argument from previous studies in other regions (Line 549-575). The rest of the section describes previous studies and the discussion is not based on the results obtained in this study. Except for the section "4.3 Limitations of our analysis and future work", the discussion is not well-performed and fails to draw conclusions.

Response to R2C5:

Dear reviewer, we thank you for pointing out these shortcomings in the discussion and conclusions of our original manuscript. In the revised version, we have entirely restructured these sections to provide a deeper and more meaningful interpretation of our results. The discussion section was completely restructured to provide a deeper understanding of our main results and their implications for PI evolution. We have also developed a more robust discussion of the topographic control on thinning propagation. We have ensured that each section of the discussion is firmly anchored in our results, clearly interpreting their meanings and contextualizing them within the existing literature rather than simply describing previous studies. This restructuring has resulted in a more cohesive and meaningful discussion. The conclusions have been expanded to highlight our main findings. In particular, we emphasize how the current geometric configuration of glaciers makes them particularly susceptible to climate change, with significant implications on water resources, ecosystems, sea level rise, and landscape evolution.

Specific/minor comments:

R2C6: Line 21: “Gt year-1 annually” is redundant.

Response to R2C6: We have corrected this expression in the text, removing the word "annually."

R2C7: Line 33: complex “changes in” stresses...?

Response to R2C7: We have modified the text; the sentence now reads, "When a glacier retreats, the ice at the terminus experiences complex changes in stresses, including longitudinal, transverse, and shear stresses."

R2C8: Line 45: “melting caused by water body-glacier contact” >> “melting due to upwelling plume”?

Response to R2C8: We appreciate your comment. We were indeed referring to "melting due to upwelling plume." We have corrected the text.

R2C9: Line 65: The statement “3 mm increase...” is after Zemp et al. (2019).

Response to R2C9: We added the following reference to the text.

Zemp, M., Huss, M., Thibert, E., Eckert, N., McNabb, R., Huber, J., Barandun, M., Machguth, H., Nussbaumer, S.U., Gärtner-Roer, I., et al., 2019. Global glacier mass changes and their contributions to sea-level rise from 1961 to 2016. Nature568, 382–386. https://doi .org /10 .1038 /s41586 -019 -1071 -0.

R2C10: Line 82: “terrain elevation values” >> Do you mean “surface elevation”?

Response to R2C10: The term was corrected by surface elevation.

R2C11: Line 84: What do you mean by “characteristic terrain elevation values”? Definition?

Response to R2C11: The text refers to the dominant elevations in the region; for clarity, we have replaced the text in question with "Surface elevations predominantly range," which more clearly reflects what we wanted to express.

R2C12: Line 95-96: Please consider significant digits of these numbers in %.

Response to R2C12: Changes were made to the text according to the recommendations.

R2C13: Line 101: “quickest” >> fastest flowing?

Response to R2C13: We appreciate your comment and have changed the text as recommended.

R2C14: Line 102: “second largest” >> San Quintin is the largest in the Northern Patagonia Icefield. Do you mean “second fastest”?

Response to R2C14: We have changed the text as recommended.

R2C15: Line 106-121: I wonder if reviewing such details and numbers is necessary for this paper.

Response to R2C15: Indeed, extending the description of the study area too much does not significantly impact our research. We have removed the associated lines from the study area subsection.

R2C16: Line 135-146: Please also consider more recent studies, e.g., Sauter et al., 2020 (Hydrol. Earth Syst. Sci.), Wiedemann et al., 2018 (Front. Earth Sci.) and Salazar et al., 2024 (Climate Dynamics).

Response to R2C16: We appreciate your recommendation. However, given the length of the study area description, the paragraph in question was excluded from the subsection. This was done considering your comments and those of reviewer three.

R2C17: Line 190: “q is the ice flow” >> ice flux?

Response to R2C17: We have corrected the associated text.

R2C18: Line 190: the “surface” slope of?

Response to R2C18: We have corrected the associated text.

R2C19: Line 199: “upper current” >> upglacier?

Response to R2C19: We have corrected the associated text.

R2C20: Line 225-226: “We used the NumPy, Pandas, and SciPy libraries...” >> This or similar statements are repeated many times.

Response to R2C20: We have modified the related text. In order to specify this information only once, we added the following subsection to the text:

“2.3 Data processing and software

We processed all data in Python 3.11, using NumPy for numerical operations, Pandas for managing data, and SciPy for statistical analysis and noise filtering. For spatial analyses, we used QGIS 3.22 to manually delineate the glaciers’ centerline, setting a consistent 50-meter spacing along each centerline for data extraction. We also used a Savitzky-Golay filter along the flow direction to reduce noise in thickness, surface elevation, subglacial elevation, and elevation change profiles, following the methodology of Zheng (2022).”

R2C21: Line 236: “... between 2000 and 2018 in PI glaciers ...” >> Not only here, but “PI glaciers” is not necessary.

Response to R2C21: During the restructuring of the manuscript, we sought to reduce the use of ‘PI glaciers.’

R2C22: Equations 11 and 12: The notations are odd. Do you mean something like this? $dH(x_i)=-dh(x_i)/\{\sum_{j=1}^{N} -dh(x_j)\}$

Response to R2C22:

We thank the reviewer for pointing out the confusing notation in equations 11 and 12. In the revised version of the manuscript, we have simplified and clarified these equations following his suggestion. The new equations in section 2.5 (equations 3 and 4) now clearly express how we calculate the cumulative thinning from the terminus to any position x along the centerline.

R2C23: Equation 14: The Left-hand side is cumulative thinning, whereas the right-hand side is the Peclet number. I am confused.

Response to R2C23: Thank you for pointing out the inconsistency of equation 14. Indeed, the original equation was conceptually confusing, complicating the interpretation of our methods and results. In the revised version of the manuscript, we have completely rewritten the associated methodology section, removed the related equation, and reformulated it completely.

R2C24: Line 317: “slope gradient” >> Do you mean “slope”?

Response to R2C24: This referred to the 'bed slope gradient.' This was specified below: For that, we quantified multiple topographic parameters: bed elevation, bed slope (first derivative of elevation), and bed slope gradient (second derivative). The characterization also included analyzing the distribution of prograde (positive) and retrograde (negative) slopes with respect to the empirical thinning limit.

R2C25: Line 323-324: What do you mean by “slope difference”?

Response to R2C25: The proportion of prograde and retrograde slopes along the centerline, in this case, the first 5 km upstream from the glacier terminus.

R2C26: Line 331: “we classified ...” >> This is already described before (Line 274 and 298).

Response to R2C26: We appreciate your comment; now, we only specify this information once in section "2.5 Empirical thinning limit", as shown below: we group the cumulative percentage thinning values in windows of width Pe = 1.

R2C27: Line 342-343: The first two sentences are not necessary.

Response to R2C27: The mentioned text has been removed due to the new structure of the results section.

R2C28: Figure 2: Please use the same scale for the vertical axes of the two plots. I wonder why accumulated thinning (%) decreases upglacier from Pe=1 to 2 (Figures 2a and b) and Pe=8 to 9 (Figure 2a).

Response to R2C28: This is due to the variability of the data itself, which does not allow for the immediate stabilization of the median. This is also observed in Felikson et al. (2017) in transition 5-6 of Figure 3a. Our new analysis addresses the robustness of the established thinning limit. Given the metrics, Pe = 21 is a stable value as a limit.

R2C29: Line 343: The first sentence in the figure caption is not necessary. Should be in the main text.

Response to R2C29: Due to the new structure of the results section, the mentioned text has been removed.

R2C30: Line 371: What do you mean by “force balance increases”? It’s not a quantity.

Response to R2C30: We thank you for pointing out this conceptual inaccuracy. The phrase "force balance increases" is indeed incorrect since force balance is not a quantity that can increase or decrease but rather a state of equilibrium between different stress components. In the associated text, we referred to the percentage variations of the different stresses analyzed along the flow lines. It is important to note that in the context of restructuring the manuscript, we have eliminated the analysis of force balance as a consequence of what was discussed in R2C3.

R2C31: Line 386: “thinning limit” >> Is this the same as “empirical limit”? Please be consistent.

Response to R2C31: The terms refer to the empirical limit of thinning. Although it continues to be used interchangeably as a writing strategy to avoid excessive repetition of the term, we seek to ensure that this occurs where it does not confuse.

R2C32: Line 394: What do you mean by “geometric limit”?

Response to R2C32: The term referred to the ‘empirical limit of thinning.’ The phrase was rewritten in the context of restructuring the results section.

R2C33: Line 397-405: Please consider the significant digits for the numbers.

Response to R2C33: Dear Reviewer, We have corrected all associated data by including only the appropriate significant digits.

R2C34: Figure 5E: I am confused by the different scales given to the three maps.

Response to R2C34: Dear reviewer, incorporating different scales could have been confusing. In this new version of the manuscript, we did not incorporate additional maps to the study area (Figure 1), as we believe it is unnecessary to present our results properly.

R2C35: Line 418: "Fig. 8" >> Which plot in Figures 8A-I supports this statement?

Response to R2C35: We thank the reviewer for pointing out this lack of precision in the reference to the figures. In the original manuscript, the reference to Fig. 8 did not specify which specific panel supported our claim about Pe values. In that context, panels D-F and G-I allow the differentiation for NPI and SPI to be visualized, respectively. The color bar represents the variability of Pe. However, as part of the overall restructuring of the manuscript, we have significantly simplified the presentation of our results and the associated figures. In particular, we have removed Figure 8. The figures in the revised version of the manuscript have been redesigned to more directly and clearly illustrate our main findings.

R2C36: Line 427: "Fig. 8" >> Isn't it Figure 6? I am confused.

Response to R2C36: Dear reviewer, indeed, the plotted Pe in both cases was the same, i.e., the median for the first five kilometers from the upstream front. However, in Fig. 8, the force balance was additionally plotted. In the context of text restructuring, the figures in the revised version of the manuscript have been removed.

R2C37: Line 529: "According to our results," >> Which results? This is one example that I find difficulty in following the discussion. Please support your argument with data.

Response to R2C37: We thank the reviewer for pointing out this lack of clarity in referencing our results. In this case, we referred to Figure 3 and the associated text described in the results section. We sought to connect our discussion to our findings in the current manuscript version.

R2C38: Line 658: "suggesting the existence of an empirical limit" >> $Pe \leq 8$ was obtained by setting 90% as the threshold of so called empirical thinning limit. Why $Pe \leq 8$ suggests the existence of such a limit?

Response to R2C38: In our previous analysis, we set $Pe = 8$ because more than 90% of the cumulative thinning from the glacier front occurred below this value. In the current version of the manuscript, we have significantly strengthened the identification of the empirical limit ($Pe \leq 21$) through a statistical stability analysis. This analysis shows that the variability of the data after $Pe = 21$ is substantially reduced, with the median absolute deviation and interquartile range decreasing to approximately one-third of their pre-limit values. The confidence intervals also narrow considerably after this threshold, showing that thinning statistically stabilizes for observations of Pe greater than the established limit.

R2C39: Line 673-674: I understand the potential importance of the analysis, but difficult to follow this conclusion. What is "geometric state"? Which data show it is "a key indicator" and "essential"?

Response to R2C39: Dear reviewer, in the revised version of the manuscript, we have significantly strengthened this final section by providing a more precise definition and specific evidence supporting our conclusions. The "geometric state," characterized by the Péclet number, represents the spatial configuration of the glacier resulting from its interaction with topography. Our results demonstrate its importance as a key indicator, where $Pe \leq 21$ values encompass more than 95% of ice thinning, with statistical indicators that strengthen these findings. The practical relevance of this indicator is illustrated with concrete examples from our analysis. For example, we show that, on average, 76% of the surface area of glaciers in the Patagonian Ice Fields is below this empirical limit, with glaciers highly susceptible to thinning propagation and consequent mass loss. This quantification of vulnerability to thinning provides signals of glaciers that might require priority monitoring in the current context of climate change.

R2C40: Line 788-793: What is the difference between 2018a and 2018b?

Response to R2C40: We appreciate you pointing out this inconsistency in the bibliographical references and have corrected the text.

R2C41: Line 812-817: Duplicated?

Response to R2C41: We appreciate you pointing out this inconsistency in the bibliographical references and have corrected the text.