Responses to Comments (C) Reviewer 1 (R1)

General comment R1: Dear editors and authors,

Thank you for presenting and handling the work about the (in)stability of glaciers in the Patagonian Icefields (PI) (https://doi.org/10.5194/egusphere-2024-1053). I am very excited to see the analysis of Peclet numbers applied for the first time in a region other than the Arctic and the Greenland Ice Sheet. The workflow presented here builds on previously published models with interesting analysis strategies (see my comments below). The results are not easy to comprehend at first sight (for some reasons below), but they reveal patterns that can help decide future plans for monitoring PI glacier changes.

The work is worth publishing in TC to be shared with the glaciology community, especially for those working with PI glaciers or considering using the Peclet number and its model framework on different glacier regions. There are some technical components, however, that I would like to check with the authors and discuss for potential improvement:

Response to general comment R1:

Dear reviewer, we sincerely appreciate your valuable comments on our manuscript and recognition of the importance of applying Péclet number analysis to the Patagonian Ice Fields. In the revised version, we have completely restructured the manuscript to improve its clarity and rigor, focusing on three main findings: the identification of the empirical limit of thinning at $Pe \leq 21$, the relationship between Pe maxima and subglacial topographic features, and the evaluation of the vulnerability of glaciers according to the area under the empirical limit. We have strengthened the analysis using the most recent subglacial topography data set from Fürst et al. (2024) and through a robust statistical analysis to validate the empirical limit, in addition to simplifying the presentation of results to highlight the most significant contributions. We believe that these changes have substantially improved the quality and accessibility of the manuscript, maintaining its original scientific contribution to the understanding of the vulnerability of the Patagonian Ice Fields.

Major comments

R1C1: I might have missed this, but what is the value of l (length of perturbation) as in equation 1?

Response to R1C1: In the revised version of the manuscript, we have clarified that l represents the distance from the glacier terminus to any position along the centerline. We have added this clarification explicitly in the methodology section.

R1C2: The idea of cumulative thinning (equations 11-12) is okay, but wouldn't it be more physically meaningful if we could link or rename this quantity to the cumulative volume loss since the terminus?

Response to R1C2: We agree with your comment. In the revised version of the manuscript, we have substantially modified our methodology to directly relate cumulative thinning to ice volume loss. This is reflected in the new equations 3 and 4, which now explicitly quantify the percentage of volume loss accumulated from the terminus. This is also shown in Figure 2 of the results section.

R1C3: I don't quite understand equations 13-14. The x in CT(x) is the distance from the terminus, but in equation 13, you seem to put a Peclet number in replace of x. In addition, CT(x) itself should be a percentage number by definition, but equation 14 sets it equal to Pe_{limit}, which is not a percentage, to my understanding.

Response to R1C3: Thank you for identifying our mathematical inconsistencies. In the manuscript's revised version, we eliminated equations 13-14 and simplified our analysis of the empirical thinning limit. We now use only equations 3 and 4, which clearly describe the cumulative thinning from the term as a percentage. Additionally, the identification of the empirical limit ($Pe \leq 21$) is based on a robust statistical analysis demonstrating a significant change in the thinning behavior before and after the identified limit.

R1C4: The most necessary improvement in this work is probably the **percentage of ice flow** (equation 15). Unlike the percentage thinning, equation 15 is not physically meaningful to me because ice flow does not add up this way. I was confused multiple times when I read the manuscript; for example, in L552: "glaciers with Pe < 4.85 have 59% of their flow below the empirical limit." (How do you have 59%, not 100%, of the ice flux in a glacier's main trunk? Is the glacier diverging to many branches?) Can you justify the use of this quantity? Alternatively, I can see this quantity might relate to the mapping of high-speed zones of a glacier, and it can physically make sense this way. However, we need a better statement in the manuscript so readers know what we are physically comparing Pe to.

Response to R1C4: We sincerely appreciate this critical observation about the incorrect use of the "ice flow percentage" concept. In the revised version of the manuscript, we have corrected this fundamental conceptual error by replacing "ice flow" with "glacier surface area" to correctly refer to the glacier surface area under the empirical thinning limit. For example, we now indicate that "~76% of the glacier surface area is below the thinning limit", which is a physically consistent measurement. This correction has been applied consistently throughout the manuscript, eliminating equation 15 and any reference to percent ice flow that could confuse. The new metric accurately represents the proportion of the glacier area potentially vulnerable to diffusive thinning.

R1C5: Figure 2: If this is cumulative thinning (volume loss), why does Pe = 2 have a lower value than Pe <= 1?

Response to R1C5: This is due to the variability of the data itself, which does not allow immediate stabilization of the median. This is also observed in Felikson et al. (2017) in the Pe 5-6 transition of Figure 3a. For a more consistent analysis, this new version of the manuscript does address the robustness of the established thinning limit. In this sense, given the model, new data used, and the associated metrics, it is suggested that Pe = 21 is a stable value as a limit.

R1C6: For all figures with Pe as the x-axis, we should probably change the tick labels from 1 and 10 to <=1 and >=10, respectively.

Response to R1C6: The previously reported values have changed slightly due to the implementation of the new methodological framework. We have adopted a window width of Pe = 1. We also did not include the lower and upper-end values that are not critical for this analysis; for example, values that are smaller than Pe = 0 were not included.

R1C7: Since you put p-value in many figures -- Is the "R" correlation coefficient or the coefficient of determination? Is it "R" or "R^2"? What model does the p-value test? It looks like it's testing a linear model, but this should be explicitly specified. And if it's testing a linear model, for trends that are not linear (e.g., Figure 4c and Figure 6b), R(^2) and p-value are not good indicators.

Response to R1C7: We appreciate your observation and apologize for the lack of clarity in the statistical analysis. In the manuscript's revised version, we removed linear correlations and p-values that were inappropriate for nonlinear relationships. In the new version of the manuscript, we have implemented a statistical approach focused on identifying the empirical limit of weight loss ($Pe \leq 21$) based on non-parametric metrics such as the median absolute deviation (MAD) and the interquartile range (IQR).

R1C8: Some information appears multiple times, such as using QGIS and some Python libraries. Since this is not a short article, I wonder if there's a way to present this once and for all and save the length.

Response to R1C8:

Dear reviewer, we appreciate your observation. To avoid redundancy in the description of the tools used, in the revised version of the manuscript, we have consolidated all the information on tools in a single subsection titled **"2.3 Data processing and software"**, where we added the following information to the text:

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 glacier 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).

R1C9: L600-601: The force balance model already assumes that tau_d is the sum of the three resistive forces (cf. L244-245). So what do you mean by "compensate" here?

Response to R1C9: Thank you for observing the inconsistent terminology in our analysis of the balance of forces. In the previous version of the manuscript, when we talked about "compensating" in percentage terms, we were referring to the relative changes in the components of the force balance over time. This means that if the driving stress (τ_d) increases by a certain percentage, the resistive stresses (τ_b, τ_{lat}, τ_{lon}) must be adjusted collectively in a proportion that maintains the equilibrium of the system. However, we recognize that the term "compensate" was not the most appropriate term in this context.

In the revised version of the manuscript, we have entirely removed text related to force balance to maintain focus on our main finding on the empirical thinning limit based on the Péclet number. We believe this decision has allowed us to present a more coherent and direct analysis of the relationship between glacial geometry and the spread of thinning.

R1C10: The authors plan to release the code after the paper is accepted. Do you also plan to release the derived data set, such as the Peclet numbers along the selected flowlines? I appreciate it if you could specify this in the Data Availability section.

Response to R1C10: We have released the code associated with our analysis and the data set used, including the central lines analyzed in this manuscript version. We hope that with this, anybody can produce our results quickly and in a friendly manner, in addition to facilitating the application of the methodology in unexplored regions.

Minor comments

R1C11: There are also a few copyediting suggestions as below:

Response to R1C11: We have carefully checked your recommendations for the text. However, due to the length of the article and the request for systematization by the reviewers, some of the suggestions could not be included since the associated texts were eliminated. We specify whether this situation applies in the text for each of your comments.

R1C12: L129: annual precipitation?

Response to R1C12: Indeed, there are PI areas that exceed 10,000 mm per year. Given the length of the study area description, the paragraph in question was excluded from the subsection.

R1C13: L289: The percentage of total thinning is expressed as the thinning at position x along the flowline as follows

Response to R1C13: In the manuscript's revised version, we have reformulated this section, presenting equations 3 and 4 more clearly and precisely. We now explicitly explain that the cumulative thinning is calculated as a percentage of the total thinning along the centerline, where x represents the distance from the terminus. Additionally, the mathematical notation has been simplified to make the physical interpretation of these equations more accessible.

R1C14: L435: the vulnerability of these

Response to R1C14: Dear reviewer, thank you for bringing this error to our attention. The text in question was removed from the current version of the manuscript due to the request for length reduction.

R1C15: L600: glaciers that are/have been relatively stable

Response to R1C15: Because the force balance was removed from our analysis, the text in question was eliminated in the new version of the manuscript.

R1C16: L610: I don't understand what it means to "have fewer Péclet number than the limit." Less Péclet number? What limit do you mean?

Response to R1C16: Dear reviewer, thank you for pointing out this ambiguity. In the original text, we referred to the fact that glaciers that end in the ocean and lakes presented, on average, in the first 5 kilometers from their front, an average value of Pe lower than the empirical limit we had defined at that time as Pe = 8. However, in the current version of the manuscript, we have completely restructured the results and discussion, eliminating this specific analysis of the terminal region to focus on the new empirical limit identified (Pe ≤ 21) and its relationship with the observed changes in the glaciers, which provides a more precise and more direct presentation of our main findings.

R1C17: L634: The geometry of the fjord is also a topographic condition?

Response to R1C17: In technical terms, the shape of the fjord is an intrinsic topographic condition in a glacial environment, which influences the dynamics through multiple mechanisms (e.g., Frank et al., 2022). In our analysis, the possible impact of the morphological characteristics of the fjord is excluded; instead, we rely exclusively on the subglacial topography.

References:

Frank, T., Åkesson, H., De Fleurian, B., Morlighem, M., & Nisancioglu, K. H. (2022). Geometric controls of tidewater glacier dynamics. *The Cryosphere*, *16*(2), 581-601.

R1C18: L663: The statement here has nothing to do with basal conditions. Maybe "Despite Pio XI Glacier changed its terminus state from marine to land..."?

Response to R1C18: There was indeed confusion in our writing since we were referring to the change in status of the front of the Pio XI glacier. However, in the current version of the manuscript, and as part of the overall restructuring to improve clarity and focus, this sentence has been removed from the conclusions, which now focus on our main findings on the empirical limit of thinning and its implications.

R1C19: L815: Two Felikson et al. (2021) references.

Response to R1C19: We have corrected the duplicate reference.