

The state and fate of Glaciar Perito Moreno Patagonia

Corresponding Author: Mr Moritz Koch

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Mr Koch,

Your manuscript titled "The state and fate of Glaciar Perito Moreno, Patagonia" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns.

Two of the reviewers pointed out the weakness of the numerical model. Considering the nonlinearity of the system, keeping the relationship between dh/dt and elevation in the future is not a realistic assumption. Reviewer 2 suggests including ice dynamics and mass balance processes in the model, whereas Reviewer 3 recommends careful analysis of bathymetry and thickness data. Further, Reviewer 1 requests clarity in result descriptions, transparency about data errors and the model's limitations.

In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns. In particular, please ensure that the numerical experiments are updated by including more realistic processes in the model. Please also consider a more careful analysis of the bed elevation data and a discussion of its implications.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Shin Sugiyama, PhD
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Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

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- Life sciences

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review: The state and fate of Glaciar Perito Moreno, Patagonia

Authors: Koch et al.

Summary:

Glaciar Perito Moreno is a prominent glacier of the Southern Patagonia Icefield in both a glaciological and socio-economic perspective. This study therefore provides a valuable update on its current state as well as an interesting prediction on its future fate. The GPR and bathymetry data presented are of high quality and allow for a detailed description of GMPs ice thickness and bed/proglacial topography. Such field data is rarely available for large calving glaciers and will no doubt be of use to similar studies in the future and the wider community. This data also facilitates a greater understanding of the recent behaviour of GPM. The GPR and bathymetry dataset also clearly highlights the potential for pronounced glacial retreat in the future in response to ice calving, as has happened for other outlet glaciers of the SPI when they have retreated into over-deepened basins. The methods used to process the field data, as well as the accompanying remotely-sensed data, are robust and are in line with other similar studies. Overall, there is no doubt this paper offers an important contribution to our understanding of GPM. My main issue with this paper is the execution of the write up. In its current form there are several grammatical and sentence structure errors present that need to be rectified and the manuscript requires a thorough proof-read in its entirety. I have highlighted only a selection of these grammatical issues in my commentary. In terms of content, I feel that the 'Main' section would benefit from a broader description of the mass balance components that influence this glacier and the importance of calving cycles for many of the outlet glaciers of the SPI. How sensitive is this glacier to climatic and/or calving dynamics and what are the processes governing these interactions? As is indicated in my comments, some of the descriptions of the results lack clarity and need to be more concise. There is also a need to be more transparent about the errors present within the data and its interpolations (particularly in relation to the elevation change analysis). Some of this information is available within the appendix and needs to be summarised and inserted into the main text. I find the retreat model used to be interesting and of value as it certainly highlights the potential impact of ice calving in the future for GPM.

However, I feel that its limitations need to be better communicated. Without considering the impacts of future climate-driven change this model can only be viewed as a first order approximation of what might happen in the future.

Given the above, I would recommend that this study is published but only after a significant revision.

Please find my specific comments in the attached document

Reviewer #2 (Remarks to the Author):

This study can be broken into three elements.

1) The authors characterize the current state of Glaciar Perito Moreno, a lake-calving glacier, by analyzing surface DEMs, ITS LIVE velocity products, and glacier outlines. After years of stability, the glacier appears to have entered a phase of retreat. Thinning has led to retreat and flow acceleration in the lower reaches, which is a characteristic signature of the retreat of glaciers that terminate in water.

2) The authors also collected (i) new bathymetric data in recently exposed portions of the proglacial lakes and (ii) helicopter-borne radar measurements of the lower half of the glacier (roughly the ablation area). These data reveal that the glacier is retreating into an overdeepening, suggesting that the glacier should continue to retreat due to ice dynamic feedbacks, much like other water-terminating glaciers.

3) The authors use a simple elevation-dependent thinning rate, based on current observations, to model the evolution of the glacier over the next century.

Elements 1 and 2 appear to include solid observations that are important for understanding the current state of Glaciar Perito Moreno and forecasting how it will change into the future. Element 3 is more tenuous. The model only crudely includes ice-dynamic feedbacks by assuming that dh/dt is a function of elevation and that the dh/dt profile won't change into the future either due to changes in ice dynamics (e.g., calving retreat) or climate (surface mass balance). I think this approach is probably pretty good for short term changes (years to a decade or two), but I don't know how valid it is into the future. I would be more convinced by the model results if the authors could demonstrate that their approach is consistent with more sophisticated models – or better yet, just use a more sophisticated model that includes ice dynamic and climatological processes.

Glaciar Perito Moreno is important because it is a highly observed glacier by tourists, and therefore communicating changes there has implications for global perspectives of climate change. If not for that, I am unsure if this paper rises to the level necessary for Communications Earth & Environment, especially given my concerns about the modeling results.

As an additional aside, I found the abbreviations for the glacier and proglacial lakes a bit cumbersome. I suggest writing out their names to improve readability for readers that are not familiar with the study site.

Reviewer #3 (Remarks to the Author):

The authors present a novel and interesting new dataset that accurately describes the changes experienced by Perito Moreno Glacier in the recent past, as well as its current state, and elaborates on possible future scenarios.

The dataset is extremely valuable due to the significant effort required for its acquisition and because the fate of Perito Moreno Glacier has vast consequences, as it is the main touristic attraction of a significant area of Patagonia.

The manuscript provides an insightful picture of the recent increase in thinning rates and the detachment of the glacier front from a subaquatic moraine. Then, it highlights the significant likelihood of the glacier initiating a retreat phase that would break a long period of stability. This information will most definitely impact the scientific community, the decision-makers of the Santa Cruz province in Argentina, and the many local businesses and inhabitants who rely on the spectacle that Perito Moreno Glacier offers visitors.

General comments

While the manuscript constitutes a significant contribution, its main weakness lies in the retreat model's unrealistic assumption of a constant thinning rate. The description of the recent past draws a picture of a system with highly non-linear behaviour. This picture includes a substantial acceleration of the thinning rate, differential changes in surface speed, and an extremely heterogeneous geometry, which includes subaquatic moraines, variable fjord width and depth, and prograde and reverse bedrock slopes of different degrees. In such a scenario, a constant thinning rate is probably a very poor approximation. Any modelling of future behaviour in this scenario is highly complex, and its research usually starts with rough simplifications of the actual process. These simplifications aim to capture the process's main drivers (first order) and neglect higher-order components. In this context, the retreat model presented by the authors is an honest attempt to create a simplified model. It is illustrative and discloses its assumptions and limitations. However, it fails to capture the main drivers (first order) controlling the glacier retreat. The main shortcoming is the unrealistic assumption of a constant thinning rate. The retreat model presented focuses on flotation, which is indeed one of the processes playing a role during a fast retreat phase. However, it is the thinning rate that ultimately controls when a given area floats and detaches from the bedrock. Glaciers terminated in water bodies are characterized by fast and dramatic retreat phases punctuated by long periods of

stability. The authors mention this fact and provide abundant references.

During those periods of fast change, accumulation rates can be considered constant, and the large mass balance variations are associated with abrupt changes in frontal ablation, which is, in those cases, mainly controlled by calving rates.

Calving is a highly complex process to model on its own, but in a first-order approximation, it will be proportional to the area of the glacier exposed to calving. Long and deep calving fronts will lead to more mass wasting than narrow and shallow fronts.

This lengthy prologue hopes to contextualize my impression that the manuscript will significantly benefit from reducing the relevance given to the retreat model and introducing a discussion of where Perito Moreno's next stable positions could be located.

These stable positions will likely be located in narrow and shallow sections of the valley. For example, the authors could estimate the frontal area that would be exposed to calving once the glacier has retreated to the mouth of its host valley. If that area turns out to be similar or smaller than the area exposed when the glacier was in contact with the frontal moraine. Then, there would be no reason to believe that the glacier will keep retreating, as such geometric change could be enough to return the glacier to a state of equilibrium, and that position might be the next stable position.

A careful analysis of the bathymetry and thickness data could provide insight into several potential stable positions.

Particular comments

25: repeated words "or lakes"

74-78: "Ice flow follows..." This sentence needs to be clarified. I suggest rephrasing and adding new annotations on Figure 1 to make clear where the large confluence is, which is the main flow you refer to, and which are the shallower sections with bedrock ridges.

78: "Ice flow merges..." This also needs to be clarified. Do you mean: "All tributary valleys merge in a single glacier tongue roughly..."?

Fig 1: While ice thickness is a valuable metric. The discussion about glacier stability, retrograde slopes and bedrock topography would be easier to follow if Figure 1 showed bedrock topography with a glacier outline instead of ice thickness. This representation would also be consistent with the bathymetry, which is presented as the absolute altitude of the lake floor (not in terms of water depth which is analogous to ice thickness). Alternatively, you could use contours to display bedrock topography (or ice thickness if you use the colour scale for bedrock topography).

Fig 2: On panel b), add a label on the horizontal axis. Also, the "hypsometric distribution" part of the plot needs to be explained better. Hypsometry usually refers to a area v/s elevation relationship. In this case, it is elevation v/s distance to the front. Which I would understand as an elevation profile more than an hysometric distribution. What exactly are you showing here?

It would be instructive to annotate on panel a) where along the flowline falls the 1500 m mark that is used as the starting point of panel b).

Regarding the velocities, there seems to be a very significant spread of data points, varying by several hundreds of m/y between continuous points. Please explain how the thick colour lines were calculated from those points. Abrupt changes on the lines look like the artifact of an outlier robust moving average. Or the effect of extreme outliers on a standard moving average.

Move the sentence "Background: SRTM hillshade." to the description of panel a).

218 It might help the discussion to explore the differences in velocity change between areas grounded above or below the lake level. In the case of areas grounded below the lake level, the basal water pressure will be kept relatively constant and similar to the water column between lake level and ground level. Therefore, thinning the glacier would lead to a corresponding reduction of overburden pressure and, then, to a reduction of effective pressure, leading to increasing basal sliding. If other contributing factors of surface speed are relatively small, perhaps this pattern can be observed in the data, and could be useful to better understand the observed speed variations.

226 It might be pertinent to mention that when the moraine is in contact with the ice, it also reduces the area of the glacier front exposed to the water which lacks a buttressing stress. Therefore, it reduces the frequency of calving events, and the typical mass released on each event.

238-241: Repeated and poorly phrased sentence. Please rephrase.

252: What do you mean by "thick ice cover" as an error source? Clarify.

272-277: Unclear and confusing use of "surface lowering rate" and "elevation change rate". What is the difference?

277-278: "This assumption could..." clarify what you mean by extreme summer. For example, "extremely hot summer".

278-280: "However, the opposite is also feasible...". Do you mean "since flow acceleration can lead to increasing downwasting"?? instead of "no flow acceleration"?

288: "are expected to increase further than expected". It seems an oxymoron. Rephrase.

310: "hmorligihem-1" What that means? Explain or remove.

315: "the base station was located close to the starting and landing area.". Please indicate how far was that landing area from the flight lines.

347: The ice thickness reconstruction requires slope. It is unclear why the authors chose to use SRTM if they observed differential thinning, and, therefore, slopes have changed since 2000. Why not use one of the more current DEMs the authors already have? Please provide an explanation of why that choice was made and how it can impact the results.

409: ¿XX m?

490: "developed by 55." Either add the reference after "algorithm" or put "developed by Gardner et al.(55)"

525: "J.-L.B.-B." is a confusing way of putting the acronym. I would suggest to use "J.B." or "J.L.B.B."

623: In figure S6, please describe what the black error bars mean. Max/min?

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Decision Letter:

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Dear Mr Koch,

Your revised manuscript titled "The state and fate of Glaciar Perito Moreno, Patagonia" has now been seen by the original three reviewers, and we include their comments at the end of this message. You will see that the reviewers appreciate the effort you put in the revisions, but there are still some important concerns raised by reviewers 2 and 3. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

Specifically, for publication in Communications Earth & Environment to be appropriate, a revised manuscript must present a robust and comprehensive model that includes dynamic effects to provide a quantitative assessment of the glacier's future stability.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

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Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:

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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

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Alireza Bahadori, PhD
Associate Editor
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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Second Review: The state and fate of Glaciär Perito Moreno, Patagonia

Authors: Koch et al.

Summary: I feel that the authors have addressed mine and the other reviewer's comments well and conceptually and structurally this is a much-improved manuscript. However, I still feel there are some issues with the execution of the write up. I have included specific comments below which outline some of these issues. If these mainly sentence structure and grammatical issues are resolved, I would be happy to recommend this manuscript for publication.

Specific comments:

Line 10-12: Does bistatic microwave data need to be mentioned twice?

Line 17: Should you not mention the model.

Line 32: Insert 'The Patagonia region'

Line 34: Insert 'either into'

Line 33-37: These sentences basically say the same thing.

Line 59-62: Do you mean at GPM specifically?

Line 59: Introduce the description of climate sensitivity.

Line 68: Citation needed

Line 68-71: Who found this? Citations needed.

Line 74: This does not flow particularly well.

Line 85: Change to 'of a glacier using models, knowledge of glacier geometry is crucial.'

Line 90: ice thickness (remove field)

Line 94: are being to? Revise.

Line 94: This bit is a little vague and is almost an aim which are outlined in the next paragraph

Line 115: This is a result and should be put in the results. At this point you should outline the aim and then suggest what you hope to find.

Line 136: change to 'In Figure 1b'

Line 143: change to 'Figure 1c'

Figure 2: Put units in brackets. Elevation above sea level (m).

Line 209: change to 'retreat'

Line 221: change strongest to largest

Line 266: Change to 'flowline (Figure 2b)'

Line 266: Not sure what the significance of the 10 year gap is. If anything, you would expect this gap to observe any change more prominently. Delete this section of the sentence.

Line 280-282: Maybe quantify this loss by giving an average value in brackets at the end of the sentence.

Line 285: Revise this sentence, make it clear what you want to communicate.

Line 290: This needs to be revised.

Line 290: Re-word. This makes it sound like the velocity data represents profiles instead of single points. Change Line 291-292 to make this clearer.

Line 299: V1 point. This sentence needs to be revised.

Line 313: This sentence needs an ending. When will it prevail until?

Line 318: This sentence needs to be re-written. The main aim of the numerical model is to forecast.....

Line 320: Remove 'of'

Line 313-324: This section does not flow particularly well and needs to be written more clearly.

Line 326: Figure 3a – use this format throughout.

Line 332: Maybe just name this Stage 1

Line 341: Could you link a past example here. Has a similar process occurred and been documented (include citation) for another outlet of the SPI in the past.

Line 346: Just Stage 2

Line 386-388: Re-word sentence. The elevation datasets presented here reveal....

Line 390-391: This does not read correctly. A substantial portion of the glacier terminus will become buoyant.

Line 395-399: This does not read well. Re-write and split into two sentences.

Line 403: It is not clear how Figure 4 demonstrates how buoyancy driven calving will lead to detachment from the peninsula?

Line 408: From Figure 4 it looks like ice velocity along the central flow line of the main ice trunk increases and is not relatively unchanged. It may be worth adding a third panel to Figure 4 which maps velocity change between the two periods.

Line 422-423: It is not clear what you mean by devoid of buttressing stress.

Line 432-433: These sentences do not flow well. Remove the full stop after 'stack' and write in once continues sentence.

Line 440-442: This section needs to be written more clearly.

Line 469: Which modelling approach. Be specific here.

Line 474-474: This needs to be written more clearly.

Line 491: given that 2022/2023 was an extremely hot summer in Argentina.
 Line 497-500: This sentence is clunky and should be revised.
 Line 503-505: This sentence does not really add anything and should be deleted.
 Line 533: During three survey flights,
 Line 534: was surveyed
 Line 535: 60 km per hour?
 Line 540-542: Improve grammar
 Line 545: Change to > applying the process detailed below.
 Line 550-51: This sentence does not make sense.
 Line 556-558: This sentence does not make sense.
 Line 572-573: This sentence does not lead anywhere, it is not clear what you want to communicate here.
 Line 591-593: Not clear what you mean by capturing the maximum extent which formed the subaqueous moraine in the Canal de los Témpanos.
 Line 602: In 2011 and 2024 several scenes were available.
 Line 604: We selected, write in past tense throughout.
 Line 618-621: Revise sentence, difficult to follow
 Line 670: 10 m

Reviewer #2 (Remarks to the Author):

My primary concern with the previous version of this paper was with the model of glacier retreat. In the model, the authors use observations of the rate of surface elevation change (dh/dt) between 2019–2023 to construct an elevation-dependent dh/dt curve. This curve is then held fixed and used to model the evolution of the glacier surface forward in time; terminus retreat occurs when the terminus reaches flotation. The problem with this approach is that it doesn't account for the effects of ice dynamics on glacier surface lowering. In response to my comments, the authors tempered some of their discussion to be more qualitative about the model results. However, the problem with making the discussion more qualitative is that much of what is now written could be suggested just simply based on our understanding of glacier dynamics and glacier response to climate. The value of using a model is that it can provide more specific predictions and/or teach us something about processes.

What I'm really looking for is some analysis or discussion that provides some more confidence in the model results, which can then be used make quantitative statements about the glacier's future. There are several papers that use a similar modeling approach. I think some of these papers should be cited and used to contextualize the approach used in this paper. The authors may also need to consider some modifications to their model to make it consistent with theory and results from previous studies. Here are just a couple of studies that I am aware of (there are likely more):

1. Huss et al. (2008, 2010) use a similar, but slightly different, parameterization. In the 2010 paper they compare their dh/dt -parameterization to a flow model and show that there is pretty good agreement – at least for a land-terminating glacier. This gives some confidence in their approach that is lacking in this paper.
2. Trüssel et al. (2013, 2015) discuss how the mass wastage of a lake-terminating glacier in Alaska is mostly dominated by surface mass balance. In the 2015 paper they also use an approach similar to that adopted in this paper, except they include a dynamic correction to the elevation-dependent dh/dt curve.

If the authors insist on sticking with their modeling approach, then they should at a minimum provide more justification for their assumptions and put their work in the context of these previous studies. For this reason I think the paper still requires significant revisions before it is ready for publication.

References:

- Huss, M., Farinotti, D., Bauder, A. and Funk, M. (2008), Modelling runoff from highly glacierized alpine drainage basins in a changing climate. *Hydrol. Process.*, 22: 3888-3902. <https://doi.org/10.1002/hyp.7055>.
- Huss, M., Juvet, G., Farinotti, D., and Bauder, A.: Future high-mountain hydrology: a new parameterization of glacier retreat, *Hydrol. Earth Syst. Sci.*, 14, 815–829, <https://doi.org/10.5194/hess-14-815-2010>, 2010.
- Trüssel BL, Motyka RJ, Truffer M, Larsen CF. Rapid thinning of lake-calving Yakutat Glacier and the collapse of the Yakutat Icefield, southeast Alaska, USA. *Journal of Glaciology*. 2013;59(213):149-161. doi:10.3189/2013J0G12J081.
- Trüssel BL, Truffer M, Hock R, Motyka RJ, Huss M, Zhang J. Runaway thinning of the low-elevation Yakutat Glacier, Alaska, and its sensitivity to climate change. *Journal of Glaciology*. 2015;61(225):65-75. doi:10.3189/2015JoG14J125.

Reviewer #3 (Remarks to the Author):

The authors present a novel and interesting new dataset that accurately describes the current state and changes experienced by Perito Moreno Glacier in the recent past and elaborates on possible future scenarios.

The dataset is extremely valuable due to the significant effort required for its acquisition and because the fate of Perito Moreno Glacier has vast consequences, as it is the main touristic attraction of a substantial portion of Patagonia. The manuscript provides an insightful picture of some key recent developments, such as a dramatic increase in thinning rates and the detachment of the glacier front from a subaquatic moraine. Then, it highlights the significant likelihood of the glacier initiating a retreat phase that would break a long period of stability. This information will most definitely impact the scientific community, the decision-makers of the Santa Cruz province in Argentina, and the many local businesses and inhabitants who rely on the spectacle that Perito Moreno Glacier offers visitors.

General comments

The authors have done an excellent job addressing the concerns and comments of the reviewers. I find the manuscript clearer, building stronger conclusions based on the facts and ground truth their research has uncovered. It is an excellent piece of research as it is. However, the manuscript would benefit from a more quantitative analysis of the different stability points (stages) proposed for the Perito Moreno Glacier retreat. For example, studying the changes in the cross-sectional area (or frontal area) of the glacier front as it sits at different positions along the valley. Or assessing the thickness changes needed to start flotation at each position. Also, with some caution and acknowledging the now clearly presented uncertainties. The authors could use the model output to give the reader a rough estimate of the timing of stage 1. Although, I agree with the authors that the available information is not sufficient to estimate the duration of stage 1 or the timing of the retreat beyond this position.

It might also be interesting to add to the discussion or to "The Fate of Glaciär Perito Moreno" any observation, if any, that could hint at, or at least suggest that the proposed points of stability have been occupied by the glacier in previous glacier cycles. Perhaps the overdeepening upstream of stage 1 could be one of such observations.

Particular comments:

31,35,36 add space between G_t and a^{-1}

46 Consider removing the word "seasonal"

77 Consider changing "are being" for "are used"

97 For consistency, use a dot as decimal separator for the longitude (instead of a comma)

Figure 1: The figure says that the solid black line corresponds to the 2024 glacier outline, while the caption says it is the 2023 outline. Please correct the wrong one.

The last sentence of the caption (regarding the grey base map). Refers to panels a and b. Move it to the section describing panel a. In its current position, it seems to refer to panel c.

It would be helpful to highlight in this figure the location of the ELA, which is mentioned several times, saying it is located at 1200 m a.s.l., but it is not shown on any of the maps.

111 Consider changing "ice masses" to "ice mass"

112 Consider rephrasing. Maybe something like, "In this area, we measured the maximum ice thickness, close to 700 m."

126 Consider referring to the "north-south oriented valley" as "the valley extending to the south". Otherwise, it can be confused with the other small north-south oriented valley extending to the north.

141 Consider rephrasing to avoid the use of the apostrophe on "glacier's terminus"

155 Typo in retreat

188 142 is a large number, and it is confusing given that in the figure there are only 10 lines. There are details about the stacking of elevation models that you address in the discussion, but they should go here.

Figure 2: Add a space between "Figure 2" and "a)". As mentioned in my earlier review, the grey barplot is a surface elevation profile, not a hypsometric distribution (Figure S4 shows a hypsometric distribution), this mistake was corrected in the figure axis label but not in the figure caption. Consider rephrasing "solid lines a weighted moving average" to "solid lines are weighted moving averages".

It would be helpful to know if the lines shown in panel b correspond to data acquired always in February (as SRTM) or if some seasonal variability could be mixed into the inter-annual lowering rate. This emphasizes the need to explain in this section all the details about elevation model dates and stacking procedures.

Figure 3: I assume there is an error in the description of panel a) and where it says "stages 2 to 4 are shown" should say "stages 1 to 3 are shown"

308 I don't understand what the authors mean by "as these regions experience a strong decline once they are situated above". Please clarify.

323 With the current phrasing, the authors should remove the word "ratio". Or rephrase to refer to the large AAR.

326 This clarification of DEM dates and the following one about the stacking procedure should be on the Methods (or in both sections).

365 marked "a" extremely...?

375 Consider rephrasing to avoid the use of an apostrophe on "glacier's response"

383 Consider rephrasing. Perhaps something like "are associated with a subaqueous moraine" or "coincide with a subaqueous moraine".

378 This is the first time you refer to a timeframe in the manuscript. It should be mentioned before or removed from here.

395 Add a comma after flights.

397 Do the authors mean ~60 km/h (per hour)?

398 was collected?

400 Maybe replace the last "systems" with "receivers"

402 More relevant than the distance to the helipad is the distance to the glacier, which is unclear. I would suggest adding a distance from the GNSS base to the glacier front or pointing the reader to Figure S7 to see where the landing area is located relative to the glacier.

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Version 2:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Mr Koch,

Your revised manuscript titled "The state and fate of Glaciar Perito Moreno, Patagonia" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment, provided you include an argument that acknowledges the limitations associated with the approach.

If you can address this request, we therefore invite you to revise your paper to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Shin Sugiyama, PhD
Editorial Board Member
Communications Earth & Environment
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Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

The authors have (mostly) addressed my concerns. The paper presents some nice new data and synthesis of existing data to assess the state of Glaciar Perito Moreno. These results are solid, but I don't know if by themselves they would be sufficient for a publication in Communications Earth and Environment. The paper then uses a model to assess the future evolution of the glacier. Although the authors have made some attempt to qualify and contextualize the model results, I still feel a bit uncomfortable with them. This is not to say that I think the results are wrong, per se; rather, I just don't have a good sense of the uncertainty in the model results.

This is all to say that I think that the paper would be suitable for publication in a specialty journal. What I don't know, given my concerns with the model, is whether this paper reaches the level desired by Nature Communications.

Reviewer #3 (Remarks to the Author):

The authors present a novel and interesting new dataset that accurately describes the current state and changes

experienced by Perito Moreno Glacier in the recent past and elaborates on possible future scenarios. The dataset is extremely valuable due to the significant effort required for its acquisition and because the fate of Perito Moreno Glacier has vast consequences, as it is the main touristic attraction of a substantial portion of Patagonia. The manuscript provides an insightful picture of some key recent developments, such as a dramatic increase in thinning rates and the detachment of the glacier front from a subaquatic moraine. Then, it highlights the significant likelihood of the glacier initiating a retreat phase that would break a long period of stability and present possible future equilibrium positions. This information will most definitely impact the scientific community, the decision-makers of the Santa Cruz province in Argentina, and the many local businesses and inhabitants who rely on the spectacle that Perito Moreno Glacier offers visitors.

General comments

The authors have once more done an excellent job addressing the concerns and comments of the reviewers. The manuscript is clearer, building stronger conclusions based on the facts and ground truth their research has uncovered. It is an excellent piece of research as it is. However, I have multiple minor specific comments about the form in which the information is presented.

The only more general comment concerns the extremely superficial description of the ice thickness reconstruction method. While the authors provide pertinent and valuable references, many particularities are not clarified, like the source data and parameters used to run the model. The authors have added a sentence highlighting the large uncertainties on the surface mass balance of Perito Moreno Glacier, but nevertheless, they use a thickness reconstruction model that requires surface mass balance data, not even mentioning what data they used and how the aforementioned uncertainties could impact the results.

I recommend the publication of the manuscript once these issues are addressed.

Particular comments:

Line 11: Showing the mean value for one period and the maximum value for the other doesn't really say anything about the changes in the rate. The maximum values in the first period could be 6 m/y with a mean of 0.34, and this still holds true. This looks handcrafted to sound more dramatic, but it is not rigorous. Please provide mean values for both periods (and if you want maximum values or another metric within parentheses).

Line 22: You use the form "Ice Field" indistinctively, like in this line, or Icefield, like in most other mentions. For consistency, stick to one of the two forms. I strongly recommend using "Icefield."

Line 33: In "aforementioned mass loss of" Consider changing "of" by "with"

Line 82: The authors should introduce the reader to the local geography before discussing specific place names such as "Canal de los Témpanos." This can be achieved by referring the reader to figure 1 here or earlier (maybe on line 36)

Line 107: Please double-check if it is correct to say that the bed becomes shallow. I understand that what becomes shallow is the glacier or the lake. Perhaps rephrase to something like "The contour lines in the central part of the terminus show the position of the bedrock ridge extending from Peninsula Magallanes, dividing the ice flow to the north and east."

Figure 1b: The red "lake level" contour line in the tip of the glacier near Península de Magallanes seems to suggest that about a quarter of the northern margin on that side would be sitting on above-lake-level bedrock. However, even recent pictures show a calving front in that area, which suggests that the ice is grounded below the lake level. It would be worth checking the accuracy of that line.

Figure 1: The gray background would be much clearer to the reader unfamiliar with the area if water areas were highlighted in a different colour. You can use the SRTM water mask for that purpose.

Line 119: Consider changing "expands" to "extends."

Line 125: Consider changing "valley" to "branch" for clarity

Line 138: "This phenomenon can be attributed to the circulation and stratification of water in the vicinity of freshwater calving glaciers." it is unclear to what phenomenon you are referring to. The sill? How would the sill "be attributed to the circulation and stratification"? Or do you mean "This phenomenon impacts/control/influences the circulation and stratification..."?

Figure 2b: Just a minor suggestion. It would be informative to see where V1-V4 are in this panel, and it would be easier to read if the distance scale in the x-axis used kilometers instead of meters.

Line 173: Mission (singular)

Line 181: For "each" year

Line 187: I assume this refers to differences over stable terrain. Please say it in the text if that is the case.

Line 188: It took me a while to understand what "The respective uncertainty of the 2019 to 2023 differencing" means. It was clear only after looking at Table S3. I suggest rephrasing to something like "In the case of the change between 2019 and

2023 using TanDEM-X DEMs, the uncertainty was $\pm 0.35 \text{ m a}^{-1}$ "

Lines 193-195: Try to clarify what each number refers to. Saying that towards the terminus, the lowering is up to 5.5 m/a seems to contradict that at the deeper troughs at the terminus, it goes above 6 m/a. I assume that "towards the terminus" does not include the terminus. Or that one refers to point data and the other to a spatial average. Try to reword it less confusingly.

Lines 231-233: This affirmation requires supporting evidence to be stated with such confidence. The reduction of effective pressure associated with the thinning might be one factor. However, reduced buttressing stress associated with the separation from the frontal subaquatic moraine or just by the front moving into deeper water could be another factor equal to, or more important. In addition, the reduction of effective pressure would be instantaneous throughout the thinning area. In contrast, the reduction in buttressing would create a dynamic adjustment propagating upstream from the front, just as you describe.

I suggest changing your confident wording that attributes the enhanced flow to this single factor and mention at least debuttressing in addition to thinning induced effective pressure reduction.

Line 263: Add a comma between O'Higgins and Viedma

Line 294: "submerged" is confusing here. Perhaps what you mean is "grounded".

Line 301: I suggest changing "in 2022" to "since 2022"

Line 303: Instead of "mass," it would be more appropriate to say "thickness" or "overburden pressure."

Figure 4: "Absolute surface velocity difference between 2022 and 2017 in m" does not make sense. If it is a difference in surface velocity, it should be in m a^{-1} (meters per year). If it is in meters, you probably mean "surface elevation difference." Please correct.

Line 307: I assume 100 m "per year." Please include the correct unit. The same is true for the "~50-70 m" on the same line.

Line 315: Maybe it is worth adding that a larger frontal area would also enhance calving in addition to the mentioned subaqueous melting

Line 368: I struggle to understand the sentence "Ice thickness as a source of error..." Please find a clear way to explain that or expand the explanation with the required details/context.

Line 370: If the authors used a model to fill the gaps in the GPR data, I think "interpolated" is not the right word. Interpolation usually refers to simple gap-filling techniques such as linear and bilinear interpolations.

Line 377: Earlier, you provided the depth measured in that area by the seismic profile. It would be very interesting to report the borehole's depth (and derived bedrock elevation) and compare it with the bedrock elevation measured by your GPR survey.

Line 394: "Contrary" reads awkwardly here. Consider rewording or just removing that word.

Line 410: Here, the authors argue that the uncertainty on the surface mass balance of Perito Moreno Glacier hinders the use of a more physically realistic model. This is a statement I could agree with. However, the ice thickness reconstruction method used by the authors relies on surface mass balance data. The authors need to explain how they reconcile that the SMB data is good enough for ice thickness reconstruction but not for dynamical modelling. Alternatively, just remove that argument and say that a complex model was out of the scope of this work.

Line 411: ¿on? the plateau

Line 458: Fig. S7 (currently missing the "S")

Line 488: I feel this section needs to go deeper as it relates to the primary dataset used in the study. Saying: "we applied an established state-of-the-art ice thickness reconstruction method" and giving the references is acceptable, but even so, I would expect a few sentences explaining how that method works in a nutshell. What is not acceptable is omitting the source data used for the model, such as surface mass balance data, glacier outlines, surface velocity, and model parameters.

Lines 547-549: Remove one of the two "whenever possible"

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Reviewer #1 (Remarks to the Author):

Review: The state and fate of Glaciar Perito Moreno, Patagonia

Authors: Koch et al.

Summary:

Glaciar Perito Moreno is a prominent glacier of the Southern Patagonia Icefield in both a glaciological and socio-economic perspective. This study therefore provides a valuable update on its current state as well as an interesting prediction on its future fate. The GPR and bathymetry data presented are of high quality and allow for a detailed description of GPMs ice thickness and bed/proglacial topography. Such field data is rarely available for large calving glaciers and will no doubt be of use to similar studies in the future and the wider community. This data also facilitates a greater understanding of the recent behaviour of GPM. The GPR and bathymetry dataset also clearly highlights the potential for pronounced glacial retreat in the future in response to ice calving, as has happened for other outlet glaciers of the SPI when they have retreated into over-deepened basins. The methods used to process the field data, as well as the accompanying remotely-sensed data, are robust and are in line with other similar studies. Overall, there is no doubt this paper offers an important contribution to our understanding of GPM.

We thank the reviewer for his constructive comments and support to improve our manuscript. We very much appreciate the time and effort invested by the reviewer in this task.

My main issue with this paper is the execution of the write up. In its current form there are several grammatical and sentence structure errors present that need to be rectified and the manuscript requires a thorough proof-read in its entirety. I have highlighted only a selection of these grammatical issues in my commentary.

We have checked the entire manuscript for grammatical and misleading sentence structures.

In terms of content, I feel that the 'Main' section would benefit from a broader description of the mass balance components that influence this glacier and the importance of calving cycles for many of the outlet glaciers of the SPI.

We added information about mass balance components to the main section and mentioned previous seasonal calving cycles of Glaciar Perito Moreno to the introduction, too.

How sensitive is this glacier to climatic and/or calving dynamics and what are the processes governing these interactions?

Added information regarding the climate sensitivity of Perito Moreno and Patagonian glaciers and the calving dynamics in the introduction.

As is indicated in my comments, some of the descriptions of the results lack clarity and need to be more concise. There is also a need to be more transparent about the errors present within the data and its interpolations (particularly in relation to the elevation change analysis).

Some of this information is available within the appendix and needs to be summarised and inserted into the main text. I find the retreat model used to be interesting and of value as it certainly highlights the potential impact of ice calving in the future for GPM. However, I feel that its limitations need to be better communicated. Without considering the impacts of future climate-driven change this model can only be viewed as a first order approximation of what might happen in the future.

Given the above, I would recommend that this study is published but only after a significant revision.

We thank the reviewer for this positive statement. We give now more information on uncertainties and better constrain the limitations of our modelling approach. In order to account for those, we also make the timeline less explicit and now refer to potential retreat stages according to the bed topography to be reached. Some of the figures have been adapted and changed respectively and the text rewritten accordingly.

Please find my specific comments in the attached document

Reply to Reviewer 1 specific comments:

L. 3: This is a bit of a broad statement. Can you give a little more detail about how GPM has defied global warming.

Added: While adjacent glacier basins retreated, the frontal position and ice surface elevation of GPM appeared unaffected.

L. 9: Surface elevation lowering?

Now reads [...] ice surface elevation lowering [...].

L. 11-12: Using what technique?

Our time-series consists of TanDEM-X scenes and SRTM. To make it clear we have used space borne microwave data the sentence now reads:

Using bistatic microwave radar satellite data, we observe an acceleration of glacier surface elevation lowering in the lower regions from 0.34 m a^{-1} (2000-2019) to up to 5.5 m a^{-1} (2019-2024), with corresponding glacier acceleration on the tongue and unprecedented frontal retreat.

L. 14: Susceptibility to what? Be more specific here.

Re-phrased the sentence to: The high susceptibility of GPM to surface lowering follows a similar pattern to that of other lacustrine calving glaciers in Patagonia in recent decades, while our new database enables subsequent in-depth modelling.

L.25: Grammar

Deleted "or lakes".

L. 25: Give a time period here. Mass loss over what period?

Added the referred time period of 2000 to 2019.

L. 27: Can you give an up-to-date example here?

Added: To illustrate, Glaciar Upsala (see fig. 1a) contributed to the aforementioned mass loss of $-1.4 \pm 0.4 \text{ Gta}^{-1}$, predominantly during a retreat of approximately 3km from 2008 to 2010. Glaciar O'Higgins (see fig. 1a) exhibited a loss of $-1.7 \pm 0.4 \text{ Gta}^{-1}$ during 2010 to 2019. In contrast to the

situation at Upsala, the ice front has been in retreat since 2017, following a period of approximately 15 years during which its position remained relatively stable.

L. 37-41: Revise this sentence. Maybe split into two sentences.

Revised and split into two sentences: It is noteworthy that air temperature records measured in close proximity to the glacier terminus since the mid-1990s until 2020 indicate a decadal warming trend of $0.2^{\circ} \pm 0.1^{\circ}\text{C}$. In that period, particularly strong warming trends ($0.31^{\circ} \pm 0.1^{\circ}\text{C}$ in DJF and $0.27^{\circ} \pm 0.2^{\circ}\text{C}$ in SON) were observed during the summer months. This has resulted in an increase in surface melt.

L. 37: Surface air temperature?

Yes. Added "Surface" (see above).

L. 38: Available for the ...

Replaced prior wording into [...] measured in close proximity to the glacier terminus [...]. (see above).

L. 42: For example, topographic pinning points

Changed to: For example, topographic pinning points [...].

L. 44-45: This sentence needs to be phrased better.

Rephrased sentence to: It has been observed that glacier fronts remain at pinning points for extended periods following significant shifts in climatic conditions.

L. 48: When did this occur?

Added years of seismic and bore hole measurements (1996 & 2010).

L. 49: This needs to be introduced further, including relevant citations.

Added context to the statement as well as additional, relevant and up to date citations:

To forecast the evolution of a glacier with numerical models, information about a glacier's geometry is a key information. In this context, the term geometry is used to describe both the surface and the underlying topography. Although the surface can be observed directly, for instance, via satellite remote sensing, deriving the bed topography is a more challenging process. Generally stated, the bed topography is derived from reconstructing the ice thickness field and subtracting it from the surface elevation¹. The deployment of a helicopter-borne ground-penetrating radar (GPR) represents the most viable methodology for the accurate assessment of extensive areas of strongly crevassed glaciers situated within steep mountainous terrain. Consequently, GPR measurements are being employed extensively in research projects around the globe to determine the topography of the bedrock for the purpose of modelling¹⁻⁷

L. 51: Revise grammar.

Revised grammar (see above).

L. 52: Repetition here.

Removed repetition (see above).

L. 54: A new ice thickness survey at GPM using a

Added your suggestion to the text: [...] we conducted a new ice thickness survey at GPM ice thickness using a [...].

L. 57: Re-phrase this section of the sentence.

Rephrased sentence: These measurements were then incorporated into an established ice thickness reconstruction approach to derive the bedrock topography of the entire glacier domain.

L. 64-66: Re-phrase this sentence.

Rephrased to: We further developed a conceptual model to project the potential future glacier evolution based on buoyancy-driven retreat. Based on recently observed ice elevation change rates, our model highlights the potential for near-term large-scale glacier retreat due to the topographic setting beneath the glacier.

L. 68: Remove “The”.

Removed.

L. 74-76: This sentence requires more detail remove the use of “they”.

We rewrote this section since Figure 1b has been changed to displaying the bedrock topography instead of the ice thickness. The ice thickness map has been moved to Figure 3.

L. 74: GPR data collected in this study.

See above.

L. 75-79: This section requires more detail and specifics. Maybe label the different flow components on the map. This will help you describe the distribution better.

See above.

L. 76: at the glacier terminus.

See above.

L. 80: Of the

See above.

L. 85-87: Needs to refer to a figure here. Also, maybe another figure is needed that focuses on the ice thickness of the terminus as this is an important feature.

Yes, we agree on this point. We believe the bedrock figure shows more bedrock features.

L. 86: GPM.

Changed to GPM.

L. 96–98: Explain this further.

Added a more detailed explanation:

The moraine reaches heights of 100 to 120 m above lake bottom, which governs the temperature stratification of the water in proximity of the glacier’s terminus. This phenomenon can be attributed to the circulation and stratification of water in the vicinity of freshwater calving glaciers⁸. The processes are governed by density disparities between glacial meltwater and lake water, which are themselves contingent upon temperature, pressure, and sediment concentration^{8,9}. The cold and turbid water in front of PMG is confined to the depth of the bathymetric sill, while only the upper water layers are

subjected to heating by solar radiation and mixing by catabatic winds. This results in a reduction in subaqueous melting at depth¹⁰.

L. 98–100: This sentence needs to be re-phrased.

Re-phrased to: After the year 2020, a pronounced recession was observed on the north-western shore of the Canal de los Témpanos. Here, at its northern end, the terminal receded by 800 metres in four years.

L. 98: Is this calving into the CT?

Yes, it is. Added information to the text (see above).

L. 104: acquired.

Added “acquired”.

L. 109-115: There needs to be some mention of surface elevation accuracy of the SRTM and TanDEM-X comparisons and the TanDEM-X and TanDEM-X comparisons. Integrate information from Table S1 here.

Added information about the DEM differencing uncertainties and refer to table S1 to S3 in the appendix for detailed information.

L. 118: Austral.

Added ‘austral’ before summer.

L. 121: Ice elevation.

Added ‘ice elevation’.

L. 129: Surface elevation lowers...

Re-phrased to: ‘[...] surface elevation lowers [...]’.

L. 140: Figure 2b needs an x axis title. Y axis needs to be changed to Ice surface elevation change. Figure 2c – What are the small dots of colour on this graph? Consider removing.

Added axis title to Figure 2b. The small dots are all individual measurement extracted for the given coordinate and the observational period. Added: “Each dot represents an individual measurement, solid lines a weighted moving average.”

L. 149-150: Is this within the vertical error rates of this DEM comparison?

This depends. The reviewer rightfully notes that small changes in the surface elevation have to be discussed carefully. Added a statement that this could be true for the highest accumulation areas where fresh snow could still be present in February (acquisition of SRTM during February). Rewrote partially to:

It is acknowledged that SRTM is susceptible to biases in high-mountain areas due to signal penetration in dry snow. Consequently, the elevation difference is within the vertical error towards the higher altitude¹¹. The ablation zone of the glacier is not affected by the penetration bias due to the acquisition taking place during February, when the ablation zone of the glacier is snow free. In the subsequent eight-year period up to 2019, a gradual reduction in the ice surface is observable.

L. 151: What do you mean by this?

Re-phrased to be clearer: In the subsequent eight-year period up to 2019, a gradual reduction in the ice surface is observable. However, there is no discernible acceleration in the rate of surface lowering.

L. 156-158: This description needs to be more specific.

Re-wrote and added information: This trend intensified until 2024. In addition to the considerable rates of surface lowering observed in the ablation zone of the glacier, significant elevation loss is also evident in the accumulation zone. Despite the considerable reduction in ice elevation observed in 2024, with a decrease from 1000 m a.s.l., the surface lowering is not as pronounced. At higher elevations along the glacier flowline, a slight positive rate of change is observable, however our confidence in elevation changes in the accumulation area has rather low confidence due to the signal penetration bias of TanDEM-X and SRTM.

L. 165: Is this the average?

Yes, added '[...] on average [...]’.

L. 172: In response to pronounced ice calving?

The velocity increases to a lowered effective pressure at the glacier bed. This allows for faster basal sliding of the glacier, increased ice flow. Due to the fast flow, the ice stretches leading to the formation of large crevasses. This in combination to near or buoyant conditions facilitates high calving rates.

L. 178: Can this be described as robust if it assumes constant retreat and thinning rates?

It may have been misleading to put “robust” here. Re-phrased.

L. 181-182: This sentence needs to be rephrased.

Re-moved the sentence due to restructuring of our discussion.

L. 182: to capture.

Re-moved the sentence due to restructuring of our discussion.

L. 183: What do you mean by this?

This referred to the simplicity of the model, but was admittedly poorly worded. The sentence has been removed due to changes to the whole section.

L 188: Chance x axis to Distance from 2023 terminus (m).

Referring to the 2023 surface in both figures.

L. 189: GPM.

Re-moved the sentence due to restructuring of our discussion.

L. 200-203: The connection between these two sentences needs to be made more explicit here.

Re-moved the sentence due to restructuring of our discussion.

L. 211-212: Integrate the time periods into this sentence.

Integrated the time periods.

L. 212: What do you mean by this?

Re-phrased to “Should glacier thinning continue at this rate [...]”

L. 212-214: Need to make reference to the bed topography as to why buoyancy may be achieved for the ice section suggested.

A figure was incorporated in order to demonstrate the areas of fast ice flow and thinning, which are associated with the regions exhibiting the most significant surface lowering rates.

L. 215: Should this not be ice calving rate and the glacier bed?

We were referring to the surface height of the glacier being close to the flotation height. Re-phrased to: “Here, we can observe a fragile relationship between the thickness of the ice and the depth of the lake. “

L. 222: Periods of enhanced retreat.

Added.

L. 238-239: Not clear what you mean here.

The DEMs acquired within a 60-day window are then stacked, and the mean elevation is calculated from this raster stack. This approach serves to mitigate the impact of residual interferometry, co-registration, and penetration artefacts that may be present in individual scenes. Re-phrased to: In order to achieve a reduction in the vertical bias of the SAR elevation change measurement at GPM, we aggregate DEMs of the same season using stacks of median elevation. This serves to mitigate the impact of residual interferometry, co-registration, and penetration artefacts that may be present in individual scenes.

L. 240-241: Repetition.

Deleted.

L. 241-242: Re-phrase sentence.

Re-phrased to: Furthermore, GPR data is subject to inherent uncertainties.

L. 244-247: Re-phrase sentence.

Re-phrased to: Since the campaign was conducted at the end of the austral summer, the presence of a snow layer above the solid ice of the glacier, which could have influenced the speed of wave propagation, was not a factor. The air layer between the antenna and the glacier surface was taken into consideration.

L. 254: I think the cumulative error rate needs to be better communicated within the main text. Similar goes for the ice surface elevation change analysis.

The cumulative error in the ice thickness reconstruction in the ablation zone of the glacier can be largely attributed to the error in the GPR measurements, as the observations constrain the reconstruction, especially in the ablation zone of the glacier where we measured in dense grids. The error of the GPR measurements is discussed, but we have added information to better explain the potential source of error in the thickness field.

Uncertainties from the height change analysis have been added to the main section.

L. 257-258: Why? What other evidence do you have? Quantification of the ice thickness error should then give you an indication as to whether the magnitude of the overdeepening is greater than any error.

Prior to the GPM measurements, two measurements were carried out. The seismic profile at the "Buscaini" site was conducted in 1996, and a borehole drilling was undertaken in 2010. A high degree of concordance was observed between these measurements and the present study's findings. The magnitude of the over deepening is far beyond any error inherent to GPR measurements. This conclusion remains robust even when considering an error of 10% in our GPR measurements, thereby substantiating our ice thickness reconstruction, which is strictly constrained by the dense measurement grid.

L. 261-262: Most importantly, it ignores the climate signal so can only give a first-order approximation of the future retreat scenarios. Previously you have mentioned the glaciers response to El Nino events which indicates sensitivity to climate fluctuations. I think the model is useful in indicating the role of ice calving but can not be over egged in terms of its forecasting accuracy.

We agree. We have highlighted the models shortcoming in regards to its forecasting ability since it does not account for the SMB or climatic influences in general. Since we have changed the section regarding the model as well as the figure and discussion, we hope to have addressed your remarks.

L. 273-276: Re-phrase this sentence.

This section has been removed.

L.278-280: Not sure what you mean here.

The "no" was a typo. The sentence was removed during the restructuring process.

L. 282: Surface air temp?

Yes. Added.

L- 285-287: Yes, this needs to be made more prominent and discussed at the start of the discussion.

The text was modified to provide a clearer and more concise description of the model's limitations with respect to climate glacier interactions. In response to the comment made by Reviewer #1, the paper was restructured to ensure that the model results were a) of a qualitative nature and b) less prominent than the other findings. We therefore intend to maintain the structure of the discussion as it currently follows the content of the result section.

L. 287: Are we talking about the GPM or the SPI as a region.

Added "region".

L. 297-298: This contradicts itself.

Agree, this was phrased poorly. It now reads: "Our bathymetric study shows maximum advances of the glacier in the Canal de los Témpanos in the form of a subaqueous moraine which has added stability to the glacier front in the past. However, recent observations have revealed a retreat of more than 800 m in this area"

L. 308: Research articles are typically written in 3rd person. This needs to be consistent throughout.

We have found several recent publications in Communications Earth & Environment which are written in first person and decided to keep it as is.

L. 309-311: Re-phrase this sentence.

Re-phrased the sentence.

L. 310: Not sure I know what this is?

This was a typo. Deleted it.

L. 315: helicopter.

Added 'helicopter'.

L. 323-324: Not sure what is meant here but this may be due to my lack of experience with this processing.

This processing step facilitates a static correction in time direction by the given input. This step corrects the corresponding antenna offset. The offset refers to the physical separation between the transmitter and the receiver antenna. By shifting the traces to time zero, we correct the data to the exact moment the radar pulse is transmitted from the antenna.

L. 332-333: Do similar studies also use these parameters?

Yes. The major processing steps were established in prior studies using the same or similar system¹²⁻¹⁵. Some minor adjustments to the processing chain have been done such as energy decay or gain functions to get "better" reflections of the bedrock at deeper layers.

L. 335-337: Revise grammar.

Revised.

L. 342: remove.

Removed.

L. 343: Why is it state of the art?

The reconstruction of applied ice thickness has been developed and applied to a multitude of regions across the globe. This reconstruction has been compared to other approaches, thereby proving its concept^{6,7,16}. The methodology employed incorporates all available observations for entire regions, with the GPR measurements being utilised to inform the basin-wide thickness reconstruction. Given its development for this specific purpose, it is considered the most suitable approach for our research, thus classifying it as state-of-the-art.

L. 346: What is the overall accuracy?

In an application for the entire Patagonian ice fields the ice volume accuracy for the NPI and SPI was estimated to be at ~10% and ~25% respectively⁶. A comparison was made of the model runs with and without the GPR data. It was found that the overall volume estimate for Glaciar Perito Moreno was satisfactory. However, the ice thickness distribution in the ablation zone was found to be either too shallow (in the central parts) or too deep (towards the glacier margins) without the newly obtained data.

L. 373: Just need to use GPM.

Deleted "Glaciar Perito Moreno".

L. 374: Re-phrase sentence.

Re-phrased.

L. 382: Not sure I understand what you mean by this?

This was worded poorly. Rephrased to: [...] and converted to elevation values on a reference surface DEM. As a reference surface, we use the void-filled Shuttle Radar Topography Mission (SRTM) [...]"

L. 382: As a

Added.

L. 388-391: Re-phrase this sentence.

Re-phrased.

L. 402: So these years had less SAR data available?

Yes. In said names less acquisitions during the end of the ablation period were present. See appendix table S1.

L. 403: Do you mean DEMs collected in the same year?

We refer to DEMs that were prior or past February 16th to approximately match the acquisition date of the SRTM DEM in 2000.

L. 404:-406: Re-phrase sentence.

Re-phrased.

L. 409: Value is needed.

We added values for the vertical spread to the text.

L. 445: Revise grammar.

Revised.

L.460-462: Re-phrase sentence.

Re-phrased.

L. 568: the DEMs used.

Replaced.

L. 607: Refer to a figure or dataset.

Added a citation to which we are referring here to.

L. 631: Re-phrase sentence.

Following a thorough review, the decision has been taken to remove the section on the sensitivity study. This is due to the fact that the model outcomes are no longer described quantitatively.

L. 634: Do not understand what you mean here. Please revise.

This section has been removed due to the restructuring of the Appendix. The original sentence was referring to the models capability of only calving glacier areas due to buoyant conditions or ice surface lowering below the bedrock height.

L. 638-640: Re-phrase this sentence.

See above.

L. 640: Convert to future tense.

See above.

Reviewer #2 (Remarks to the Author):

This study can be broken into three elements.

1) The authors characterize the current state of Glaciar Perito Moreno, a lake-calving glacier, by analyzing surface DEMs, ITS_LIVE velocity products, and glacier outlines. After years of stability, the glacier appears to have entered a phase of retreat. Thinning has led to retreat and flow acceleration in the lower reaches, which is a characteristic signature of the retreat of glaciers that terminate in water.

2) The authors also collected (i) new bathymetric data in recently exposed portions of the proglacial lakes and (ii) helicopter-borne radar measurements of the lower half of the glacier (roughly the ablation area). These data reveal that the glacier is retreating into an overdeepening, suggesting that the glacier should continue to retreat due to ice dynamic feedbacks, much like other water-terminating glaciers.

3) The authors use a simple elevation-dependent thinning rate, based on current observations, to model the evolution of the glacier over the next century.

Elements 1 and 2 appear to include solid observations that are important for understanding the current state of Glaciar Perito Moreno and forecasting how it will change into the future. Element 3 is more tenuous. The model only crudely includes ice-dynamic feedbacks by assuming that dh/dt is a function of elevation and that the dh/dt profile won't change into the future either due to changes in ice dynamics (e.g., calving retreat) or climate (surface mass balance). I think this approach is probably pretty good for short term changes (years to a decade or two), but I don't know how valid it is into the future. I would be more convinced by the model results if the authors could demonstrate that their approach is consistent with more sophisticated models – or better yet, just use a more sophisticated model that includes ice dynamic and climatological processes.

We thank the referee for his critical review of our manuscript. We agree that our observational part is certainly the strongest contribution and acknowledge the remarks made concerning the mid to long term validity of the model. We are aware that these are not fully-fledged projections and that the timing of retreat is dependent on several assumptions. Hence, we have modified the presentation and discussion of the model results to a more qualitative analysis, as opposed to a

quantitative approach. The new presentation highlights potential retreat and stability phases, thereby placing greater emphasis on the findings from our survey than on those from the model. The section addressing the potential limitations of our approach has been augmented to incorporate your concerns. A full ice-dynamical modelling approach is unfortunately out of scope of this paper, but certainly target of a subsequent project.

We have changed Figure 3 due to the more qualitative description of our modelling results and adapted the entire section.

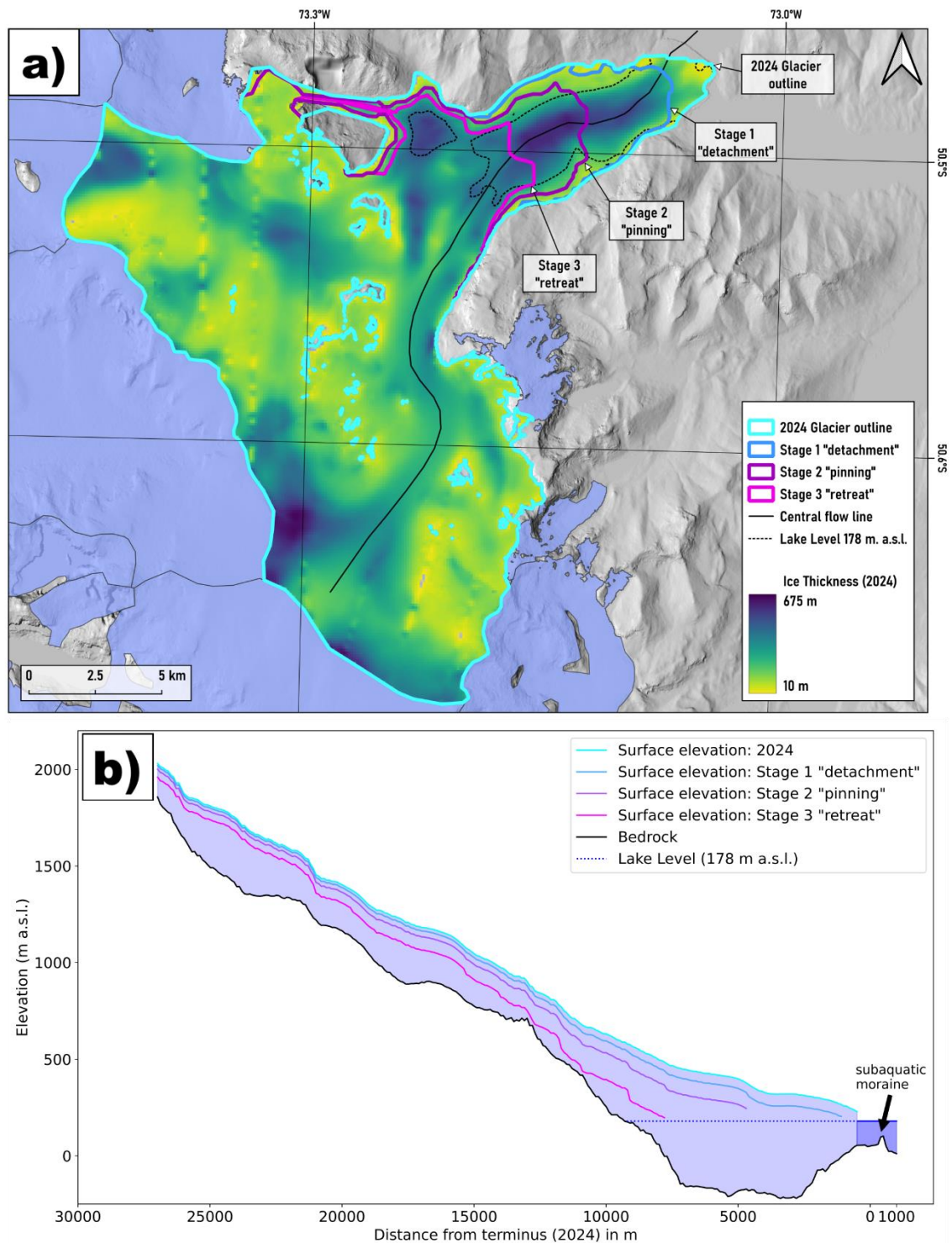


Figure 3 a) The reconstructed ice thickness (2023) of the Glaciar Perito Moreno based on GPR measurements carried out in 2022. Current outline (2023), as well as the outlines throughout stages 2 to 4 are shown. The lake level is indicated by the dotted black line, the solid black line indicates the cross section shown in panel b). b) Cross section of the glacier displaying the current surface elevation as well as surface elevations and frontal positions of stage 1 to 3.

Glaciar Perito Moreno is important because it is a highly observed glacier by tourists, and therefore

communicating changes there has implications for global perspectives of climate change. If not for that, I am unsure if this paper rises to the level necessary for Communications Earth & Environment, especially given my concerns about the modeling results. As an additional aside, I found the abbreviations for the glacier and proglacial lakes a bit cumbersome. I suggest writing out their names to improve readability for readers that are not familiar with the study site.

The glaciers and proglacial lakes name are now written out.

Reviewer #3 (Remarks to the Author):

The authors present a novel and interesting new dataset that accurately describes the changes experienced by Perito Moreno Glacier in the recent past, as well as its current state, and elaborates on possible future scenarios.

The dataset is extremely valuable due to the significant effort required for its acquisition and because the fate of Perito Moreno Glacier has vast consequences, as it is the main touristic attraction of a significant area of Patagonia.

The manuscript provides an insightful picture of the recent increase in thinning rates and the detachment of the glacier front from a subaquatic moraine. Then, it highlights the significant likelihood of the glacier initiating a retreat phase that would break a long period of stability. This information will most definitely impact the scientific community, the decision-makers of the Santa Cruz province in Argentina, and the many local businesses and inhabitants who rely on the spectacle that Perito Moreno Glacier offers visitors.

General comments

While the manuscript constitutes a significant contribution, its main weakness lies in the retreat model's unrealistic assumption of a constant thinning rate. The description of the recent past draws a picture of a system with highly non-linear behaviour. This picture includes a substantial acceleration of the thinning rate, differential changes in surface speed, and an extremely heterogeneous geometry, which includes subaquatic moraines, variable fjord width and depth, and prograde and reverse bedrock slopes of different degrees. In such a scenario, a constant thinning rate is probably a very poor approximation. Any modelling of future behaviour in this scenario is highly complex, and its research usually starts with rough simplifications of the actual process. These simplifications aim to capture the process's main drivers (first order) and neglect higher-order components. In this context, the retreat model presented by the authors is an honest attempt to create a simplified model. It is illustrative and discloses its assumptions and limitations. However, it fails to capture the main drivers (first order) controlling the glacier retreat. The main shortcoming is the unrealistic assumption of a constant thinning rate. The retreat model presented focuses on flotation, which is indeed one of the processes playing a role during a fast retreat phase. However, it is the thinning rate that ultimately controls when a given area floats and detaches from the bedrock.

Glaciers terminated in water bodies are characterized by fast and dramatic retreat phases punctuated by long periods of stability. The authors mention this fact and provide abundant references.

During those periods of fast change, accumulation rates can be considered constant, and the large mass balance variations are associated with abrupt changes in frontal ablation, which is, in those cases, mainly controlled by calving rates.

Calving is a highly complex process to model on its own, but in a first-order approximation, it will be proportional to the area of the glacier exposed to calving. Long and deep calving fronts will lead to more mass wasting than narrow and shallow fronts.

This lengthy prologue hopes to contextualize my impression that the manuscript will significantly benefit from reducing the relevance given to the retreat model and introducing a discussion of where Perito Moreno's next stable positions could be located.

These stable positions will likely be located in narrow and shallow sections of the valley. For example, the authors could estimate the frontal area that would be exposed to calving once the glacier has retreated to the mouth of its host valley. If that area turns out to be similar or smaller than the area exposed when the glacier was in contact with the frontal moraine. Then, there would be no reason to believe that the glacier will keep retreating, as such geometric change could be enough to return the glacier to a state of equilibrium, and that position might be the next stable position.

A careful analysis of the bathymetry and thickness data could provide insight into several potential stable positions.

We thank the referee for his critical review of our manuscript and the constructive comments. They helped us rearranging and re-focussing our manuscript. We agree with most of his points. We hope that with the changes made, we could also address most of the reviewers concerns.

Particular comments

25: repeated words "or lakes"

Deleted "or lakes".

74-78: "Ice flow follows..." This sentence needs to be clarified. I suggest rephrasing and adding new annotations on Figure 1 to make clear where the large confluence is, which is the main flow you refer to, and which are the shallower sections with bedrock ridges.

This section has been rewritten entirely and now reads: "In the north-south oriented valley, observations indicate that the ice is flowing through two prominent channels. These subglacial channels are separated by a 300-metre-high bedrock ridge. The formation of this ridge is attributed to the inflow of extensive ice masses from the accumulation zone, which subsequently flow into the host valley."

78: "Ice flow merges..." This also needs to be clarified. Do you mean: "All tributary valleys merge in a single glacier tongue roughly..."?

This was what we were originally referring to. It now reads as "Here, the glacier surface expands as a result of all the ice masses of the glacier flowing together."

Fig 1: While ice thickness is a valuable metric. The discussion about glacier stability, retrograde slopes and bedrock topography would be easier to follow if Figure 1 showed bedrock topography with a glacier outline instead of ice thickness. This representation would also be consistent with the bathymetry, which is presented as the absolute altitude of the lake floor (not in terms of water depth which is analogous to ice thickness). Alternatively, you could use contours to display bedrock topography (or ice thickness if you use the colour scale for bedrock topography).

Yes, we agree. We have redone figure 1b to now show the bedrock topography instead of the ice thickness map. We have added contour lines to give a better impression of the slopes. Additionally, we have merged the bedrock map from the ice thickness reconstruction with the bathymetry map in the Canal de los Témpanos. We have moved the ice thickness map to figure 3 as an additional panel, since fig. 3 now shows potential retreat phases rather than years according to the model.

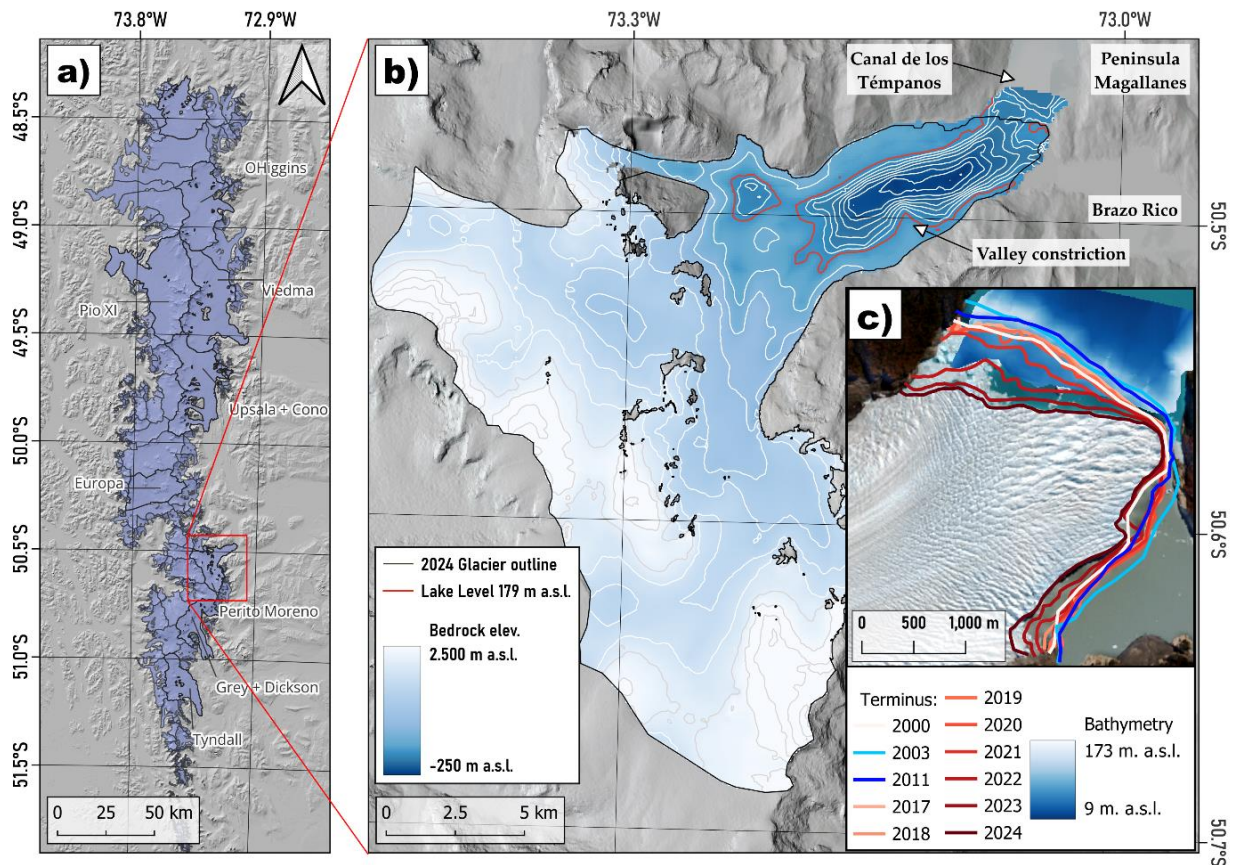


Fig 2: On panel b), add a label on the horizontal axis. Also, the "hipsometric distribution" part of the plot needs to be explained better. Hypsometry usually refers to a área v/s elevation relationship. In this case, it is elevation v/s distance to the front. Which I would understand as an elevation profile more than an hysometric distribution. What exactly are you showing here?

You are correct. We changed the axis title to "Elevation vs. distance" to be more concise.

It would be instructive to annotate on panel a) where along the flowline falls the 1500 m mark that is used as the starting point of panel b).

Added information to the figure description. The 1500m mark is right at the V1 extraction point.

Regarding the velocities, there seems to be a very significant spread of data points, varying by several hundreds of m/y between continous points. Please explain how the thick colour lines were calculated from those points. Abrupt changes on the lines look like the artifact of an outlier robust moving average. Or the effect of extreme outliers on a standard moving average.

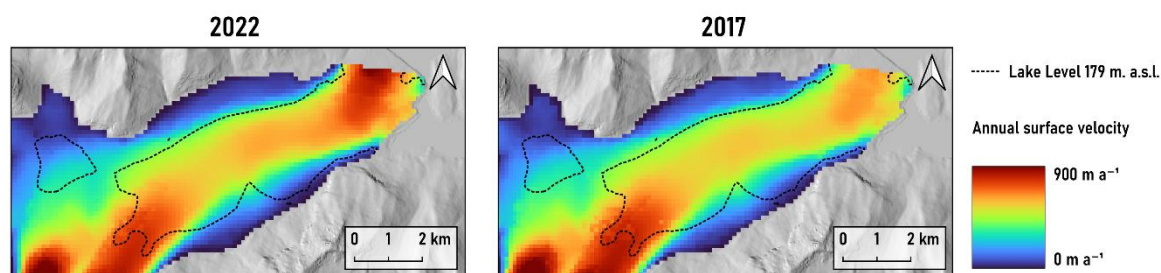
Move the sentence "Background: SRTM hillshade." to the description of panel a).

The assumption is indeed correct: the line is a weighted moving average with a window size of 75. The spikes in V4 are caused by unusual high or low outliers. This is mainly due to the steep mountain topography close to the extraction point, which drastically reduces the amount of available measurement points in this area. Hence the observable spikes in the line plot.

Moved "Background: SRTM hillshade" to the panel description a).

218 It might help the discussion to explore the differences in velocity change between areas grounded above or below the lake level. In the case of areas grounded below the lake level, the basal water pressure will be kept relatively constant and similar to the water column between lake level and ground level. Therefore, thinning the glacier would lead to a corresponding reduction of overburden pressure and, then, to a reduction of effective pressure, leading to increasing basal sliding. If other contributing factors of surface speed are relatively small, perhaps this pattern can be observed in the data, and could be useful to better understand the observed speed variations.

Your valuable suggestion has been added to the results and discussion complemented by a figure displaying the surface velocities and the lake level, indicating the areas which are grounded below lake level.



226 It might be pertinent to mention that when the moraine is in contact with the ice, it also reduces the area of the glacier front exposed to the water which lacks a buttressing stress. Therefore, it reduces the frequency of calving events, and the typical mass released on each event.

Added that to our discussion. "The presence of a moraine in contact with the glacier front also reduces the area exposed to water, which is devoid of buttressing stress. Consequently, this phenomenon leads to a reduction in both the frequency of calving events and the magnitude of the mass discharged during each event."

238-241: Repeated and poorly phrased sentence. Please rephrase.

Removed repetition and re-phrased: We then stack these DEMs and calculate the median elevation of each raster stack. This serves to mitigate the effects of residual interferometry, co-registration and penetration artefacts that may be present in individual scenes.

252: What do you mean by "thick ice cover" as an error source? Clarify.

This was poorly worded. Ice thickness as a source of error refers to the effect of the error in the value of ice thickness measured by GPR and the error in ice thickness due to the error in horizontal positioning. Added the explanation to the text.

272-277: Unclear and confusing use of "surface lowering rate" and "elevation change rate". What is the difference?

It is indeed confusing as both terms refer to the same process. Reworded. We've also restructured this paragraph because we've changed the focus of the retreat model from its ability to predict glacier evolution quantitatively to its ability to describe retreat stages qualitatively. We have therefore also removed the years from the sensitivity study.

277-278: "This assumption could..." clarify what you mean by extreme summer. For example, "extremely hot summer".

Changed to "extremely hot summer".

278-280: "However, the opposite is also feasible...". Do you mean "since flow acceleration can lead to increasing downwasting"?? instead of "no flow acceleration"?

Yes! Removed "no".

288: "are expected to increase further than expected". It seems an oxymoron. Rephrase.

Re-phrased.

310: "hmoirgihem-1" What that means? Explain or remove.

That was a typo. Removed.

315: "the base station was located close to the starting and landing area.". Please indicate how far was that landing area from the flight lines.

The GNSS base was mounted approximately 10 meters to the helicopter starting / landing area. Added the information to the text.

347: The ice thickness reconstruction requires slope. It is unclear why the authors chose to use SRTM if they observed differential thinning, and, therefore, slopes have changed since 2000. Why not use one of the more current DEMs the authors already have? Please provide an explanation of why that choice was made and how it can impact the results.

Yes, you are right, slope is required for the approach used here. You are also right that more up to date DEM products exist. However, SRTM was used due to data consistency with all the other inputs for the reconstruction like velocity fields and outlines. In addition, elevation changes only started to considerably impact in the last few years after 20 years of quasi-stability. How does that impact the results: Rightfully pointed out a change in slope would impact the values of the ice thickness reconstruction. However, the glacier surface experiencing the strongest changes in its slope due to the enhanced ice flow are also the ones surveyed in dense grids. Due to the fact that the ice thickness observations strongly constrain the ice thickness reconstruction, we do not expect a noticeable difference in the results for the ice thickness values near the terminus.

409: ¿XX m?

We added values of 0.2 m (2013 DEM stack) to 11.4 m (2024 DEM stack), respectively.

490: "developed by 55." Either add the reference after "algorithm" or put "developed by Gardner et al.(55)"

Added Gardner et al.

525: "J.-L.B.-B." is a confusing way of putting the acronym. I would suggest to use "J.B." or "J.L.B.B."

Changed to J.L.B.B.

623: In figure S6, please describe what the black error bars mean. Max/min?

Yes, the whiskers indicate the maximum and minimum offset per group. Added that information to the text.

Literature:

1. Zekollari, H., Huss, M., Farinotti, D. & Lhermitte, S. Ice-Dynamical Glacier Evolution Modeling—A Review. *Reviews of Geophysics* **60**, e2021RG000754 (2022).
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3. Juvet, G., Huss, M., Funk, M. & Blatter, H. Modelling the retreat of Grosser Aletschgletscher, Switzerland, in a changing climate. *J. Glaciol.* **57**, 1033–1045 (2011).
4. Gantayat, P., Kulkarni, A. V., Srinivasan, J. & Schmeits, M. J. Numerical modelling of past retreat and future evolution of Chhota Shigri glacier in Western Indian Himalaya. *Ann. Glaciol.* **58**, 136–144 (2017).
5. Zekollari, H., Fürst, J. J. & Huybrechts, P. Modelling the evolution of Vadret da Morteratsch, Switzerland, since the Little Ice Age and into the future. *J. Glaciol.* **60**, 1155–1168 (2014).
6. Fürst, J. J. *et al.* The foundations of the Patagonian icefields. *Commun Earth Environ* **5**, 142 (2024).
7. Fürst, J. J. *et al.* The Ice-Free Topography of Svalbard. *Geophysical Research Letters* **45**, (2018).
8. Sugiyama, S. *et al.* Thermal structure of proglacial lakes in Patagonia. *JGR Earth Surface* **121**, 2270–2286 (2016).

9. Truffer, M. & Motyka, R. J. Where glaciers meet water: Subaqueous melt and its relevance to glaciers in various settings. *Reviews of Geophysics* **54**, 220–239 (2016).
10. Sugiyama, S. *et al.* Subglacial discharge controls seasonal variations in the thermal structure of a glacial lake in Patagonia. *Nat Commun* **12**, 6301 (2021).
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13. Blindow, N., Salat, C., Gundelach, V., Buschmann, U. & Kahnt, W. Performance and calibration of the helicopter GPR system BGR-P30. in *2011 6th International Workshop on Advanced Ground Penetrating Radar (IWAGPR)* 1–5 (IEEE, Aachen, Germany, 2011).
doi:10.1109/IWAGPR.2011.5963896.
14. Rückamp, M. & Blindow, N. King George Island ice cap geometry updated with airborne GPR measurements. *Earth Syst. Sci. Data* **4**, 23–30 (2012).
15. Lippl, S. *et al.* Uncertainty Assessment of Ice Discharge Using GPR-Derived Ice Thickness from Gourdon Glacier, Antarctic Peninsula. *Geosciences* **10**, 12 (2019).
16. Fürst, J. J. *et al.* Application of a two-step approach for mapping ice thickness to various glacier types on Svalbard. *The Cryosphere* **11**, 2003–2032 (2017).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Second Review: The state and fate of Glaciar Perito Moreno, Patagonia

Authors: Koch et al.

Summary: I feel that the authors have addressed mine and the other reviewer's comments well and conceptually and structurally this is a much-improved manuscript. However, I still feel there are some issues with the execution of the write up. I have included specific comments below which outline some of these issues. If these mainly sentence structure and grammatical issues are resolved, I would be happy to recommend this manuscript for publication.

First and foremost, we would like to thank you once again for your time in reviewing our manuscript and your positive feedback. We are grateful for the constructive and detailed recommendations you have provided so far, which has certainly improved the content and readability of the manuscript. We have addressed all your individual comments throughout the manuscript. Please refer to the below section for further details:

Specific comments:

Line 10-12: Does bistatic microwave data need to be mentioned twice?

No, not really. Deleted the second mentioning.

Line 17: Should you not mention the model.

It now reads:

Utilizing a simple numerical model, we extend current retreat and thinning rates into the future, which shows the potential of buoyancy-driven retreat of several kilometers once the glacier decouples from an observed bedrock ridge.

Line 32: Insert 'The Patagonia region'

Inserted.

Line 34: Insert 'either into'

Inserted.

Line 33-37: These sentences basically say the same thing.

Merged the sentences, so there's less repetition.

Line 59-62: Do you mean at GPM specifically?

Yes. Since the entire paragraph is referring to GPM we did not refer to the GPM specifically here.

Line 59: Introduce the description of climate sensitivity.

Added.

Line 68: Citation needed

It is cited in (now) line 61.

Line 68-71: Who found this? Citations needed.

The citation was in L 61. This finding is from: Minowa, M., Skvarca, P. & Fujita, K. Climate and Surface Mass Balance at Glaciar Perito Moreno, Southern Patagonia. *Journal of Climate* 36, 625–641 (2023).

Line 74: This does not flow particularly well.

Rephrased.

Line 85: Change to 'of a glacier using models, knowledge of glacier geometry is crucial.'

Changed.

Line 90: ice thickness (remove field)

Removed field.

Line 94: are being to? Revise.

Added "conducted".

Line 94: This bit is a little vague and is almost an aim which are outlined in the next paragraph

Rephrased to: "As a result, GPR measurements are being conducted to determine bedrock topography. Having information about the ice thickness distribution at a given time can then subsequently serve as an input for modelling approaches which reconstruct the ice thickness distribution of entire glacier catchments."

Line 115: This is a result and should be put in the results. At this point you should outline the aim and then suggest what you hope to find.

Thank you for pointing that out. Removed the second sentence. It now reads: "A conceptual model was further developed in order to project the potential future evolution of the glacier based on buoyancy-driven retreat. Utilising our recently acquired data on the bedrock topography and the time series of the glacier surface and ice velocity evolution, we aim to provide insight into the past stability of the glacier, the current ice dynamics, and the potential future of this iconic Patagonian glacier."

Line 136: change to 'In Figure 1b'

This sentence does indeed refer to panel 1c. The addition of the phrase "in the optical satellite imagery" was made with the intention of enhancing the clarity of the link. For these reasons, the reference has been left unchanged.

Line 143: change to 'Figure 1c'

This reference is to be understood in relation to Figure 3a, rather than Figure 1c, since the ice thickness map has been relocated to Figure 3 (it was Figure 1 previously, which now displays the bedrock elevation). Consequently, the reference has been left unchanged.

Figure 2: Put units in brackets. Elevation above sea level (m).

Revised.

Line 209: change to 'retreat'

Changed.

Line 221: change strongest to largest

Changed.

Line 266: Change to 'flowline (Figure 2b)'

Added.

Line 266: Not sure what the significance of the 10 year gap is. If anything, you would expect this gap to observe any change more prominently. Delete this section of the sentence.

Deleted the first half of the sentence.

Line 280-282: Maybe quantify this loss by giving an average value in brackets at the end of the sentence.

Now giving elevation bin averaged values below and above the beforehand mentioned ELA.

Line 285: Revise this sentence, make it clear what you want to communicate.

Revised. Now reads: "In 2024, the significant lowering is only present at lower elevations (~ below 1500 m a.s.l.). In higher elevations, surface lowering is barely detectable."

Line 290: This needs to be revised.

Revised.

Line 290: Re-word. This makes it sound like the velocity data represents profiles instead of single points. Change Line 291-292 to make this clearer.

Surface velocities were extracted at four points V1 to V4 along the flow line as depicted in figure 2a.

Line 299: V1 point. This sentence needs to be revised.

Revised.

Line 313: This sentence needs an ending. When will it prevail until?

Added “[...] throughout all iterations of the model run”.

Line 318: This sentence needs to be re-written. The main aim of the numerical model is to forecast.....

Revised.

Line 320: Remove ‘of’

Removed.

Line 313-324: This section does not flow particularly well and needs to be written more clearly.

Agreed. Changed to: “Whilst the model does not incorporate a physically based calving solution, it does capture buoyancy-driven calving by comparing the buoyant condition of each ice column after each iteration of surface lowering¹, since it is widely acknowledged that buoyant forces at lake-terminating glaciers play a pivotal role in glacier retreat in the region^{2,3}. The results presented herein demonstrate the potential for future glacier retreat, as based on newly obtained bedrock information.”

Line 326: Figure 3a – use this format throughout.

Changed throughout the rest of the manuscript.

Line 332: Maybe just name this Stage 1

Changed.

Line 341: Could you link a past example here. Has a similar process occurred and been documented (include citation) for another outlet of the SPI in the past.

Yes of course. The following papers are now referred to and given as examples for similar behaviour of outlet glaciers in the region:

Minowa, M., Schaefer, M. & Skvarca, P. Effects of topography on dynamics and mass loss of lake-terminating glaciers in southern Patagonia. *J. Glaciol.* 1–18 (2023) doi:10.1017/jog.2023.42.

Sakakibara, D., Sugiyama, S., Sawagaki, T., Marinsek, S. & Skvarca, P. Rapid retreat, acceleration and thinning of Glaciar Upsala, Southern Patagonia Icefield, initiated in 2008. *Ann. Glaciol.* **54**, 131–138 (2013).

Bown, F. *et al.* Recent ice dynamics and mass balance of Jorge Montt Glacier, Southern Patagonia Icefield. *J. Glaciol.* **65**, 732–744 (2019).

Naruse, R., Skvarca, P. & Takeuchi, Y. Thinning and retreat of Glaciar Upsala, and an estimate of annual ablation changes in southern Patagonia. *Ann. Glaciol.* **24**, 38–42 (1997).

Line 346: Just Stage 2

Changed.

Line 386-388: Re-word sentence. The elevation datasets presented here reveal....

Revised.

Line 390-391: This does not read correctly. A substantial portion of the glacier terminus will become buoyant.

Thank you, changed.

Line 395-399: This does not read well. Re-write and split into two sentences.

Agreed. Split it up and restructured.

Line 403: It is not clear how Figure 4 demonstrates how buoyancy driven calving will lead to detachment from the peninsula?

It does not demonstrate it per se, but implies our assumption. Rephrased to be clearer here.

Line 408: From Figure 4 it looks like ice velocity along the central flow line of the main ice trunk increases and is not relatively unchanged. It may be worth adding a third panel to Figure 4 which maps velocity change between the two periods.

Good point. Added a panel displaying the absolute surface velocity change between 2022 and 2017:

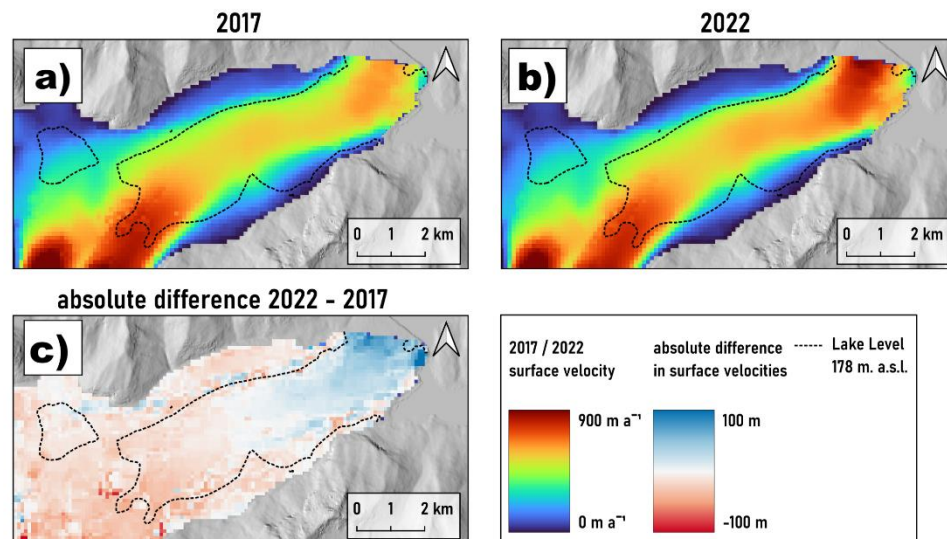


Figure 4 a & b) Annual surface velocity mosaics from 2017 and 2022. **c)** Absolute surface velocity difference between 2022 and 2017 in m. Data source: ITS_Live. Background: SRTM.

Line 422-423: It is not clear what you mean by devoid of buttressing stress.

That was a typo. Rephrased to “The presence of a moraine in contact with the glacier front also exerts buttressing stresses on the glacier front, thereby enhancing its stability.”

Line 432-433: These sentences do not flow well. Remove the full stop after ‘stack’ and write in once continues sentence.

Rephrased.

Line 440-442: This section needs to be written more clearly.

Rephrased.

Line 469: Which modelling approach. Be specific here.

Revised.

Line 474-474: This needs to be written more clearly.

Added “In this context, the term 'membrane stress gradients' is understood to refer to longitudinal stresses and transverse horizontal shear stresses.”

Line 491: given that 2022/2023 was an extremely hot summer in Argentina.

Revised.

Line 497-500: This sentence is clunky and should be revised.

Changed to "However, it has been observed that the positive effect of higher snowfall in winter is rapidly diminished by extreme surface melt rates in the subsequent summer season."

Line 503-505: This sentence does not really add anything and should be deleted.

Deleted.

Line 533: During three survey flights,

Added comma.

Line 534: was surveyed

Revised.

Line 535: 60 km per hour?

Yes. Added.

Line 540-542: Improve grammar

Revised grammar.

Line 545: Change to > applying the process detailed below.

Changed.

Line 550-51: This sentence does not make sense.

Changed to: "To remove coherent noise in the data, a spatial average (of 201 traces) is calculated and subtracted from each trace. This step aims to remove "background noise" present in the data."

Line 556-558: This sentence does not make sense.

Rephrased to "In order to account for the phenomenon of diffraction, it is necessary to determine the air and ice layer. This is achieved by manually delineating the surface reflections and subsequently creating a two-dimensional model consisting of an air and ice layer for each subset."

Line 572-573: This sentence does not lead anywhere, it is not clear what you want to communicate here.

Deleted.

Line 591-593: Not clear what you mean by capturing the maximum extent which formed the subaqueous moraine in the Canal de los Témpanos.

For the outlines of the figure 1c, we used late austral summer scenes to visualise the retreat of the terminus in the recent past. However, as the moraine was most likely formed during the last advance phases of the glacier, we used late austral winter scenes to capture this maximum extent of the glacier. Rephrased in the manuscript for clarity.

Line 602: In 2011 and 2024 several scenes were available.

Changed.

Line 604: We selected, write in past tense throughout.

Changed.

Line 618-621: Revise sentence, difficult to follow

Split up and added information. "To further increase the quality of our DEMs we have removed erroneous processing artefacts. These includes outlier cells from layover/shadow estimation, which is an effect common before and behind steep terrain (in azimuth), the unwrapping of the interferometric phase, which describes the process of restoring the correct multiple of 2π to each point of the interferometric phase. Finally, we masked out land area, with unrealistically high vertical deviations (± 100 m) from the SRTM reference surface."

Line 670: 10 m

Revised.

Reviewer #2 (Remarks to the Author):

My primary concern with the previous version of this paper was with the model of glacier retreat. In the model, the authors use observations of the rate of surface elevation change (dh/dt) between 2019–2023 to construct an elevation-dependent dh/dt curve. This curve is then held fixed and used to model the evolution of the glacier surface forward in time; terminus retreat occurs when the terminus reaches flotation. The problem with this approach is that it doesn't account for the effects of ice dynamics on glacier surface lowering. In response to my comments, the authors tempered some of their discussion to be more qualitative about the model results. However, the problem with making the discussion more qualitative is that much of what is now written could be suggested just simply based on our understanding of glacier dynamics and glacier response to climate. The value of using a model is that it can provide more specific predictions and/or teach us something about processes.

What I'm really looking for is some analysis or discussion that provides some more confidence in the model results, which can then be used make quantitative statements about the glacier's future. There are several papers that use a similar modeling approach. I think some of these papers should be cited and used to contextualize the approach used in this paper. The authors may also need to consider some modifications to their model to make it consistent with theory and results from previous studies. Here are just a couple of studies that I am aware of (there are likely more):

1. Huss et al. (2008, 2010) use a similar, but slightly different, parameterization. In the 2010 paper they compare their dh/dt -parameterization to a flow model and show that there is pretty good agreement – at least for a land-terminating glacier. This gives some confidence in their approach that is lacking in this paper.

2. Trüssel et al. (2013, 2015) discuss how the mass wastage of a lake-terminating glacier in Alaska is mostly dominated by surface mass balance. In the 2015 paper they also use an approach similar to that adopted in this paper, except they include a dynamic correction to the elevation-dependent dh/dt curve.

If the authors insist on sticking with their modelling approach, then they should at a minimum provide more justification for their assumptions and put their work in the context of these previous studies. For this reason I think the paper still requires significant revisions before it is ready for publication.

References:

Huss, M., Farinotti, D., Bauder, A. and Funk, M. (2008), Modelling runoff from highly glacierized alpine drainage basins in a changing climate. *Hydrol. Process.*, 22: 3888-3902.
<https://doi.org/10.1002/hyp.7055>.

Huss, M., Juvet, G., Farinotti, D., and Bauder, A.: Future high-mountain hydrology: a new parameterization of glacier retreat, *Hydrol. Earth Syst. Sci.*, 14, 815–829,
<https://doi.org/10.5194/hess-14-815-2010>, 2010.

Trüssel BL, Motyka RJ, Truffer M, Larsen CF. Rapid thinning of lake-calving Yakutat Glacier and the collapse of the Yakutat Icefield, southeast Alaska, USA. *Journal of Glaciology*. 2013;59(213):149-161. doi:10.3189/2013JOG12J081.

Trüssel BL, Truffer M, Hock R, Motyka RJ, Huss M, Zhang J. Runaway thinning of the low-elevation Yakutat Glacier, Alaska, and its sensitivity to climate change. *Journal of Glaciology*. 2015;61(225):65-75. doi:10.3189/2015JoG14J125.

First and foremost, we would like to sincerely thank you for taking the time and effort to review our manuscript a second time and for providing such valuable and constructive feedback. We also appreciate the literature you shared, which not only contextualizes but also aligns with our own prior considerations regarding the critique of our modelling approach. We would like to emphasize that we fully acknowledge and agree with the points you raised concerning alternative modelling strategies and the rationale behind your initial suggestions. During the design phase of our study, we carefully deliberated on various modelling setups and approaches. In retrospect, we recognize that we did not communicate this decision-making process clearly enough to you or to the reader. In the revised manuscript, we have adopted the structure you suggested, incorporating the references you provided, while also offering a more transparent and detailed explanation of why we ultimately opted for a simpler modelling approach.

In summary, the central challenge in our study region is the lack of reliable precipitation data at higher elevations, particularly across the accumulation zone of the Southern Patagonian Icefield, which is necessary for accurately modelling the surface mass balance (SMB) of individual glaciers. This data gap has led to a consistent overestimation of accumulation in SMB models that rely solely on climate data^{4,5}. We have now added a paragraph in the discussion section that explicitly addresses the limitations of our current approach. This is followed by a contextualization of more advanced modelling techniques—such as those you suggested—and an explanation of why, given the prevailing data limitations, accurate SMB projections in this region remain an exceptionally difficult task.

“The assumption of linear surface lowering rates does not allow direct quantitative statements and should therefore only be understood as a first approximation of possible retreat phases that the glacier could go through. A limitation of this approach is that it does not consider mass movements or alterations in surface mass balance other than elevation-dependent surface lowering. Consequently, the ability to ascertain whether the glacier might be in equilibrium at any given point, for instance at the aforementioned valley constriction, is precluded. It is acknowledged that there exist more sophisticated modelling approaches that take into account mass movement, changes in surface mass balance over time, elevation feedbacks and incorporate updated glacier geometries and thickness fields. These approaches allow for a correction of the elevation-dependent surface lowering field to be incorporated^{6–9}. The efficacy of such approaches has been demonstrated for lake-terminating glaciers in Alaska, as they facilitate the modelling of ice dynamical feedbacks in the future⁹. However, such models rely on climate variables, particularly temperature and precipitation, as inputs to calculate the surface mass balance of the glacier¹⁰. In Patagonia, precipitation estimates vary considerably⁴. The precipitation and precipitation rate at higher elevations therefore remain a largely unknown component for one of the wettest regions on Earth⁴. The effect of the general overestimation of accumulation rates has been thoroughly discussed⁵. There is a large discrepancy when comparing modelled surface mass balances using climate data^{11–14} compared to mass balances from geodetic approaches, which tend to be significantly more negative^{15–20}. It has been hypothesised that this discrepancy can be attributed to an overestimation of precipitation in Patagonia^{4,21}, a phenomenon that persists even in regional climate models⁵. A study modelling the SMB of the Southern Patagonian

Icefield until 2050 corrected their modelling results with frontal ablation estimates and still found a marginally positive projection under the Representative Concentration Pathway (RCP)2.6 pathway and only a slightly negative one under RCP8.5⁵.”

Additionally, to reply to your and reviewer #3’s remarks alike, we have added more information and quantitative analysis to the discussion. Figure 4 now also shows the absolute change in surface velocities between 2017 and 2022. In addition to that, we have calculated the frontal area at four cross sections of the glacier and discuss the implications of the increase of ice being in direct contact with the water. Furthermore, we have added more information on reported or similar processes, which justify our assumptions.

“The glacier surface elevation datasets presented here reveal that the glacier is losing mass at an accelerated rate across its entire ablation zone. Should glacier thinning continue at this rate, a substantial portion of the glacier terminus will become buoyant, particularly within the first 5 km upstream from its current day front. Here, we can observe a fragile relationship between the thickness of the ice above lake level and the depth of the lake. In the lower ablation zone in proximity to the glaciers terminus, a 16-fold increase in surface lowering rates were observed between 2017 and 2024 in comparison to the 2000 to 2017 period. In good correspondence with that, we have identified patterns of surface velocity increases during that period. As shown in figure 4a and 4b, these fast-flowing areas coincide well with areas submerged below or at lake level. It is hypothesised that the increase in velocity is attributable to a decrease in effective basal pressure and reduced lateral stresses. Similar periods of fast retreat have been reported to be associated with increased calving activity and speed-up near the terminus of Patagonian outlet glaciers^{2,22–24}. For example, the glaciers Viedma, Upsala and O’Higgins have also undergone stages of enhanced retreat after detaching from previous pinning points^{2,25,26}. At Glaciar Perito Moreno the observed pattern of surface velocity increases in 2022 is particularly pronounced in areas where the glacier is grounded below lake level. In these regions, the reduction in effective pressure resulting from a decrease in glacier mass leads to an acceleration of ice flow. This relationship is evident Figure 4c, where the absolute change in surface velocities between 2022 and 2017 is visualized. The areas with the most significant increases in ice speed clearly coincide with those located below lake level, while the regions above lake level experience comparatively lower ice flow. Notably, a decrease in surface velocities is also observed upstream of the valley constriction. The observation of an increase in annual velocities of more than 100 m at the glacier’s terminus, while a reduction of ~50-70 m is visible at the inflow to the lower ablation zone and above lake level, indicates that the glacier is not only losing mass at its terminus but also that less mass is being transported from the accumulation zone. This supports the hypothesis that the glacier will continue to thin, experience increased calving and subsequently retreat toward the retrograde slope, where a large-scale glacier retreat is expected, once the glacier is no longer stabilized by the subglacial bedrock ridge. Furthermore, as the glacier retreats further upstream, a greater portion of its ice will come into direct contact with lake water. This will subsequently lead to more subaqueous melt at the glacier front. We calculated the change in area below lake level the current (2024) glacier front to four cross sections (see Appendix Fig. S8b and Table S4). Close to stage 1 “retreat”, cross section 1, the area has already increased by one third compared to 2024. Cross section 2 is roughly half way between stage 1 “retreat” and 2 “pinning”, where the glacier trough has reached

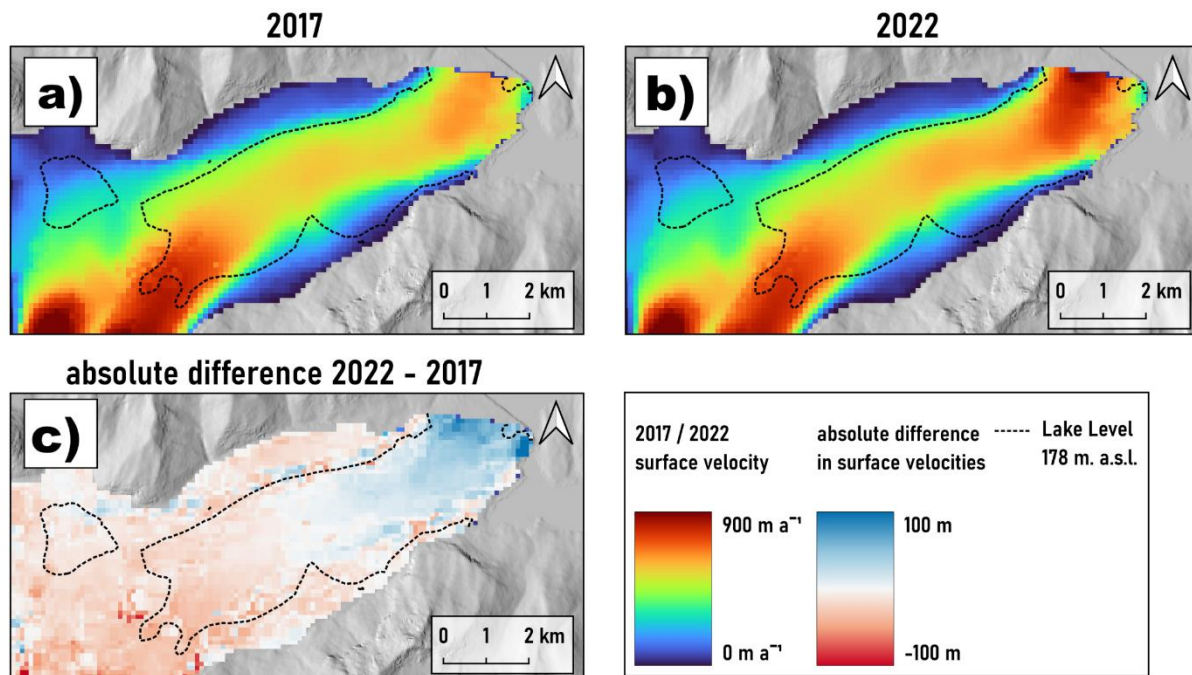


Figure 4 a & b) Annual surface velocity mosaics from 2017 and 2022. **c)** Absolute surface velocity difference between 2022 and 2017 in m. Data source: ITS_Live. Background: SRTM.

its maximum depth. The subaqueous frontal area at this stage has already grown by 90.76%. At cross section 3, located at the valley constriction, where we expect a phase of stability, the area decreases by approximately 35% compared to cross section 2. Water temperature measurements in austral summer 2013 in both lakes revealed near surface temperatures of 7 to 6°C and 5.9 to 5.4°C at depth in Canal de los Témpanos and Brazo Rico, respectively²⁷. Since the glacier front is no longer directly at a subaqueous moraine, also deeper lake layers will experience warming due to wind driven mixing^{27,28}. Unlike to marine terminating glaciers, the cold and turbid water discharged by lake terminating glaciers stays at the lake bottom²⁷. Warmer surface layers increase melt rates at the glacier front, which destabilizes the glacier front further and subsequently triggers buoyancy-driven calving²⁹."

Reviewer #3 (Remarks to the Author):

The authors present a novel and interesting new dataset that accurately describes the current state and changes experienced by Perito Moreno Glacier in the recent past and elaborates on possible future scenarios.

The dataset is extremely valuable due to the significant effort required for its acquisition and because the fate of Perito Moreno Glacier has vast consequences, as it is the main touristic attraction of a substantial portion of Patagonia.

The manuscript provides an insightful picture of some key recent developments, such as a dramatic increase in thinning rates and the detachment of the glacier front from a subaquatic moraine. Then, it highlights the significant likelihood of the glacier initiating a retreat phase that would break a long period of stability. This information will most definitely impact the scientific community, the decision-makers of the Santa Cruz province in Argentina, and the many local businesses and inhabitants who rely on the spectacle that Perito Moreno Glacier offers visitors.

General comments

The authors have done an excellent job addressing the concerns and comments of the reviewers. I find the manuscript clearer, building stronger conclusions based on the facts and ground truth their research has uncovered.

It is an excellent piece of research as it is. However, the manuscript would benefit from a more quantitative analysis of the different stability points (stages) proposed for the Perito Moreno Glacier retreat. For example, studying the changes in the cross-sectional area (or frontal area) of the glacier front as it sits at different positions along the valley. Or assessing the thickness changes needed to start flotation at each position. Also, with some caution and acknowledging the now clearly presented uncertainties. The authors could use the model output to give the reader a rough estimate of the timing of stage 1. Although, I agree with the authors that the available information is not sufficient to estimate the duration of stage 1 or the timing of the retreat beyond this position.

Thank you very much for your positive response to the revised version of our manuscript. We greatly appreciate the time and effort you have dedicated to reviewing our work, as well as your valuable contributions that have helped enhance the overall quality of the manuscript. In response to your comments, we have added a figure to the Supplementary Material showing the height-above-bouancy and cross-profile plots, which are now referenced in the main text. Specifically, we now include additional considerations related to historical lake water temperature and stratification, and we discuss the potential for a substantial increase in subaqueous melt rates.

It might also be interesting to add to the discussion or to "The Fate of Glaciär Perito Moreno" any observation, if any, that could hint at, or at least suggest that the proposed points of stability have been occupied by the glacier in previous glacier cycles. Perhaps the overdeepening upstream of stage 1 could be one of such observations.

Thank you for raising that point. In terms of seasonal cycles, past fluctuations between summer and winter rarely exceeded 50–60 meters. We have found no evidence that the glacier retreated as far as it has over the past century, but this is certainly an important area for future research.

Regarding your general comments:

Added the following plot to the appendix. Referring to height above buoyancy and different cross sections at our suggested “stages” in the main (discussion) section.

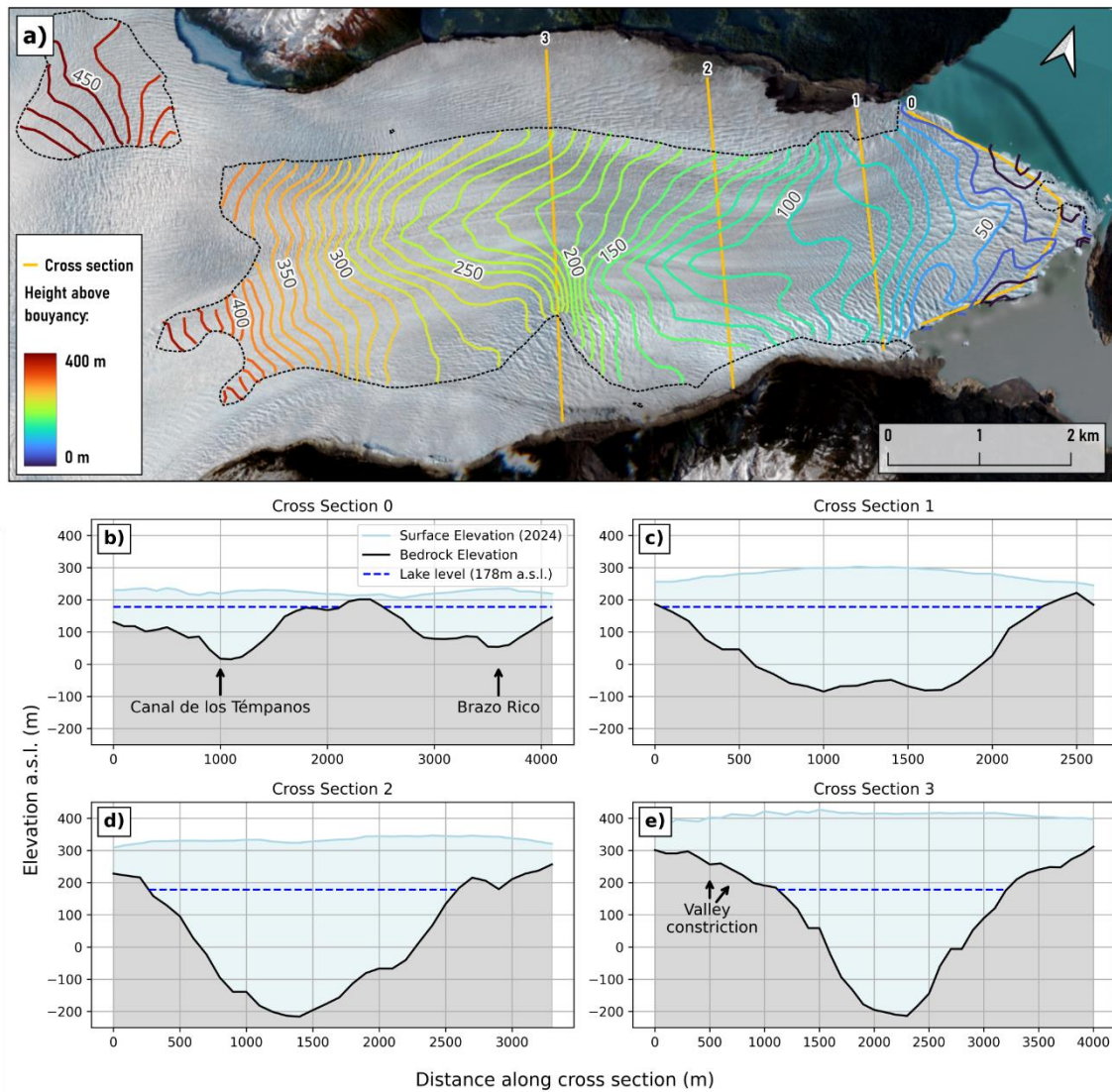


Figure S8. a) Height above buoyancy in 10 m contour lines. b) – e) cross sections at different distance to the terminus. Surface elevation indicated in light blue, lake level in dashed dark blue, bedrock in solid black.

Particular comments:

31,35,36 add space between Gt and a^{-1}

Revised.

46 Consider removing the word "seasonal"

Removed.

77 Consider changing "are being" for "are used"

Changed.

97 For consistency, use a dot as decimal separator for the longitude (instead of a comma)

Revised.

Figure 1: The figure says that the solid black line corresponds to the 2024 glacier outline, while the caption says it is the 2023 outline. Please correct the wrong one.

The last sentence of the caption (regarding the grey base map). Refers to panels a and b. Move it to the section describing panel a. In its current position, it seems to refer to panel c.

It would be helpful to highlight in this figure the location of the ELA, which is mentioned several times, saying it is located at 1200 m a.s.l., but it is not shown on any of the maps.

Thank you for your suggestions. It was indeed the 2024 outline. -> Changed. Added an ELA contour line to figure 1b. Moved the section describing the grey base map behind description of panel b. See below:

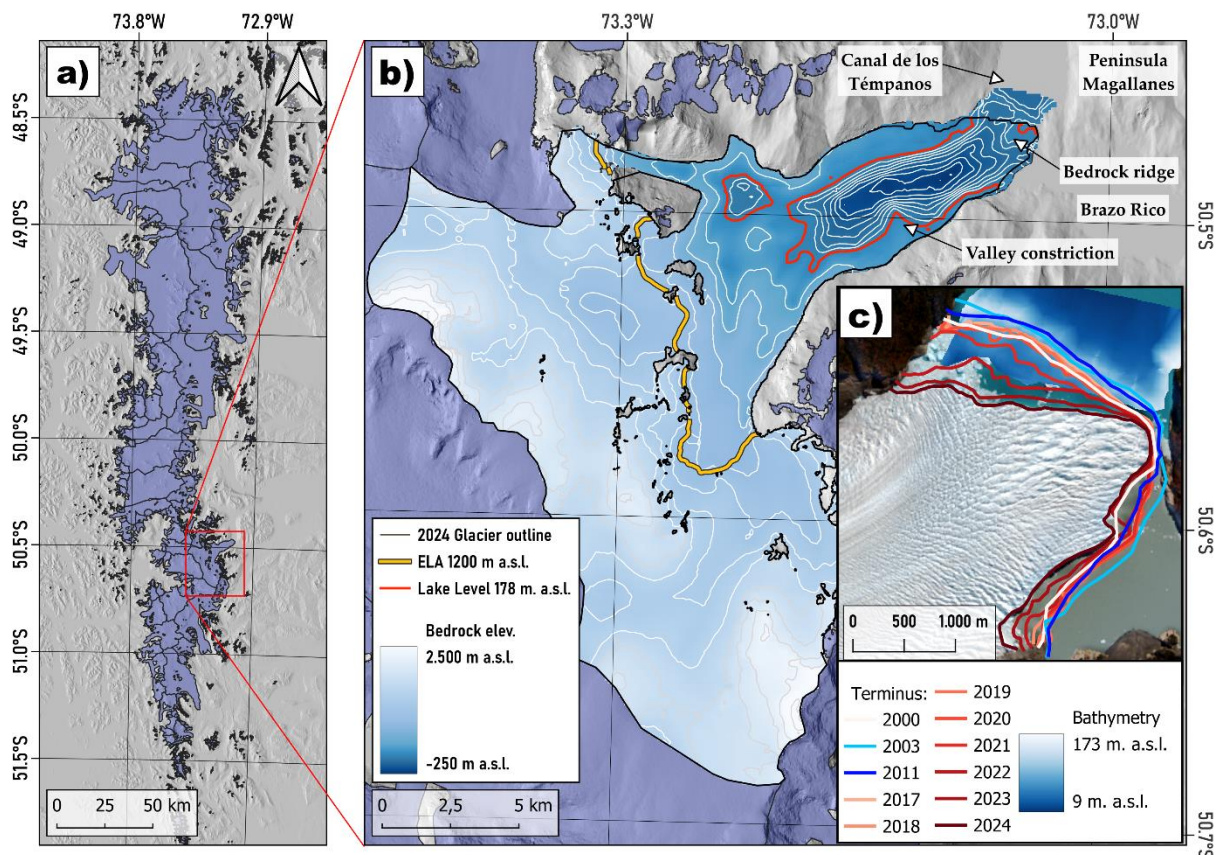


Figure 1 a) Overview of the Southern Patagonian Ice Field (in light blue). Solid black lines are the outlines of the glaciers according to the Randolph Glacier Inventory (v7.0). The red square indicates the location of Glacier Perito Moreno. b) The bedrock topography of Glaciar Perito Moreno and Canal de los Témpanos. Contour lines below the lake level (solid red) have a 50 m spacing, above 200 m. The black and orange line indicates the ELA at 1200 m a.s.l. The solid black line indicates the glacier outline of 2024. Grey base map is a hill shaded SRTM digital elevation model. c) Sentinel-2 optical image of the terminal position in May 2023. The white to red lines indicate the terminal position in April of each year delineated from Sentinel-2 and Landsat-7 imagery with the exception of 2003 and 2013 where November scenes were used.

111 Consider changing "ice masses" to "ice mass"

Changed.

112 Consider rephrasing. Maybe something like, "In this area, we measured the maximum ice thickness, close to 700 m."

Rephrased.

126 Consider referring to the "north-south oriented valley" as "the valley extending to the south". Otherwise, it can be confused with the other small north-south oriented valley extending to the north.

Agreed, revised.

141 Consider rephrasing to avoid the use of the apostrophe on "glacier's terminus"

Rephrased.

155 Typo in retreat.

Revised.

188 142 is a large number, and it is confusing given that in the figure there are only 10 lines. There are details about the stacking of elevation models that you address in the discussion, but they should go here.

Agreed, removed "142". It is sufficient information that TanDEM-X data was used.

Figure 2: Add a space between "Figure 2" and "a)". As mentioned in my earlier review, the grey barplot is a surface elevation profile, not a hypsometric distribution (Figure S4 shows a hypsometric distribution), this mistake was corrected in the figure axis label but not in the figure caption. Consider rephrasing "solid lines a weighted moving average" to "solid lines are weighted moving averages". It would be helpful to know if the lines shown in panel b correspond to data acquired always in February (as SRTM) or if some seasonal variability could be mixed into the inter-annual lowering rate.

Revised figure caption.

This emphasizes the need to explain in this section all the details about elevation model dates and stacking procedures.

Agreed. Added "The date in figure 2b refers to the median date of all stacked DEM scenes used for that year. We selected only scenes at the end of austral summer, which is also close to the acquisition time of SRTM." To the text and "[...] date refers to the median acquisition time." To the figure description.

Figure 3: I assume there is an error in the description of panel a) and where it says "stages 2 to 4 are shown" should say "stages 1 to 3 are shown"

Thank you for pointing that out. The figure description was wrongfully referring to a prior version.

308 I don't understand what the authors mean by "as these regions experience a strong decline once they are situated above". Please clarify.

Also pointed out by Reviewer #1; that section has been rewritten.

323 With the current phrasing, the authors should remove the word "ratio". Or rephrase to refer to the large AAR.

Rephrased.

326 This clarification of DEM dates and the following one about the stacking procedure should be on the Methods (or in both sections).

We refer to it in the method section, too:

L475 – 479:

In 2011 and 2024 several scenes were available covering the entire glacier basin. Whenever possible, we selected SAR acquisitions of the southern hemisphere ablation period to minimize elevation offsets due to time-varying depths of signal penetration into the glacier volume and differences in snow and ice accumulation.

And L608 – 615:

The selected, contemporaneous DEMs were stacked and the median elevation and acquisition date was calculated for each cell. The following section provides a comprehensive overview of the available digital elevation model (DEM) products. In addition, it details the creation of multi-temporal elevation mosaics, as well as the derivation of median acquisition dates. This information can be found in Table S1 (see Appendix). The mean vertical elevation deviation of all aggregated SAR acquisitions, i.e. the average difference between the highest and lowest elevation value of each raster cell and each group of stacked DEMs (Table S2), respectively, range from 0.2 m (2013 DEM stack) to 11.4 m (2024 DEM stack).

365 marked "a" extremely...?

Revised.

375 Consider rephrasing to avoid the use of an apostrophe on "glacier's response"

Sentence was removed.

383 Consider rephrasing. Perhaps something like "are associated with a subaqueous moraine" or "coincide with a subaqueous moraine".

Rephrased.

378 This is the first time you refer to a timeframe in the manuscript. It should be mentioned before or removed from here.

Removed.

395 Add a comma after flights.

Added.

397 Do the authors mean ~60 km/h (per hour)?

Yes! Added km/h.

398 was collected?

Changed to "was".

400 Maybe replace the last "systems" with "receivers"

Changed.

402 More relevant than the distance to the helipad is the distance to the glacier, which is unclear. I would suggest adding a distance from the GNSS base to the glacier front or pointing the reader to Figure S7 to see where the landing area is located relative to the glacier.

Referring to fig. 7 now.

Literature:

1. Bondzio, J. H. *et al.* The mechanisms behind Jakobshavn Isbræ's acceleration and mass loss: A 3-D thermomechanical model study. *Geophysical Research Letters* **44**, 6252–6260 (2017).
2. Minowa, M., Schaefer, M. & Skvarca, P. Effects of topography on dynamics and mass loss of lake-terminating glaciers in southern Patagonia. *J. Glaciol.* 1–18 (2023) doi:10.1017/jog.2023.42.
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4. Sauter, T. Revisiting extreme precipitation amounts over southern South America and implications for the Patagonian Icefields. *Hydrol. Earth Syst. Sci.* **24**, 2003–2016 (2020).
5. Bravo, C., Bozkurt, D., Ross, A. N. & Quincey, D. J. Projected increases in surface melt and ice loss for the Northern and Southern Patagonian Icefields. *Sci Rep* **11**, 16847 (2021).
6. Juvet, G., Huss, M., Funk, M. & Blatter, H. Modelling the retreat of Grosser Aletschgletscher, Switzerland, in a changing climate. *J. Glaciol.* **57**, 1033–1045 (2011).
7. Huss, M., Juvet, G., Farinotti, D. & Bauder, A. Future high-mountain hydrology: a new parameterization of glacier retreat. *Hydrol. Earth Syst. Sci.* (2010) doi:10.5194/hess-14-815-2010.
8. Trüssel, B. L., Motyka, R. J., Truffer, M. & Larsen, C. F. Rapid thinning of lake-calving Yakutat Glacier and the collapse of the Yakutat Icefield, southeast Alaska, USA. *J. Glaciol.* **59**, 149–161 (2013).
9. Trüssel, B. L. *et al.* Runaway thinning of the low-elevation Yakutat Glacier, Alaska, and its sensitivity to climate change. *J. Glaciol.* **61**, 65–75 (2015).

10. Hock, R. A distributed temperature-index ice- and snowmelt model including potential direct solar radiation. *J. Glaciol.* **45**, 101–111 (1999).
11. Schaefer, M., Machguth, H., Falvey, M. & Casassa, G. Modeling past and future surface mass balance of the Northern Patagonia Icefield. *JGR Earth Surface* **118**, 571–588 (2013).
12. Schaefer, M., Machguth, H., Falvey, M., Casassa, G. & Rignot, E. Quantifying mass balance processes on the Southern Patagonia Icefield. *The Cryosphere* **9**, 25–35 (2015).
13. Lenaerts, J. T. M. *et al.* Extreme Precipitation and Climate Gradients in Patagonia Revealed by High-Resolution Regional Atmospheric Climate Modeling. *Journal of Climate* **27**, 4607–4621 (2014).
14. Mernild, S. H., Liston, G. E., Hiemstra, C. & Wilson, R. The Andes Cordillera. Part III : glacier surface mass balance and contribution to sea level rise (1979–2014). *Intl Journal of Climatology* **37**, 3154–3174 (2017).
15. Dussaillant, I., Berthier, E. & Brun, F. Geodetic Mass Balance of the Northern Patagonian Icefield from 2000 to 2012 Using Two Independent Methods. *Front. Earth Sci.* **6**, 8 (2018).
16. Braun, M. H. *et al.* Constraining glacier elevation and mass changes in South America. *Nature Clim Change* **9**, 130–136 (2019).
17. Malz, P. *et al.* Elevation and Mass Changes of the Southern Patagonia Icefield Derived from TanDEM-X and SRTM Data. *Remote Sensing* **10**, 188 (2018).
18. Abdel Jaber, W., Rott, H., Floricioiu, D., Wuite, J. & Miranda, N. Heterogeneous spatial and temporal pattern of surface elevation change and mass balance of the Patagonian ice fields between 2000 and 2016. *The Cryosphere* **13**, 2511–2535 (2019).
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26. Bown, F. *et al.* Recent ice dynamics and mass balance of Jorge Montt Glacier, Southern Patagonia Icefield. *J. Glaciol.* **65**, 732–744 (2019).
27. Sugiyama, S. *et al.* Thermal structure of proglacial lakes in Patagonia. *JGR Earth Surface* **121**, 2270–2286 (2016).
28. Sugiyama, S. *et al.* Subglacial discharge controls seasonal variations in the thermal structure of a glacial lake in Patagonia. *Nat Commun* **12**, 6301 (2021).
29. Sugiyama, S., Minowa, M. & Schaefer, M. Underwater Ice Terrace Observed at the Front of Glaciar Grey, a Freshwater Calving Glacier in Patagonia. *Geophysical Research Letters* **46**, 2602–2609 (2019).

Final revision round 3

Reply to reviewer #3

Reviewer #3 (Remarks to the Author):

The authors present a novel and interesting new dataset that accurately describes the current state and changes experienced by Perito Moreno Glacier in the recent past and elaborates on possible future scenarios.

The dataset is extremely valuable due to the significant effort required for its acquisition and because the fate of Perito Moreno Glacier has vast consequences, as it is the main touristic attraction of a substantial portion of Patagonia.

The manuscript provides an insightful picture of some key recent developments, such as a dramatic increase in thinning rates and the detachment of the glacier front from a subaquatic moraine. Then, it highlights the significant likelihood of the glacier initiating a retreat phase that would break a long period of stability and present possible future equilibrium positions. This information will most definitely impact the scientific community, the decision-makers of the Santa Cruz province in Argentina, and the many local businesses and inhabitants who rely on the spectacle that Perito Moreno Glacier offers visitors.

General comments

The authors have once more done an excellent job addressing the concerns and comments of the reviewers. The manuscript is clearer, building stronger conclusions based on the facts and ground truth their research has uncovered.

It is an excellent piece of research as it is. However, I have multiple minor specific comments about the form in which the information is presented.

The only more general comment concerns the extremely superficial description of the ice thickness reconstruction method. While the authors provide pertinent and valuable references, many particularities are not clarified, like the source data and parameters used to run the model. The authors have added a sentence highlighting the large uncertainties on the surface mass balance of Perito Moreno Glacier, but nevertheless, they use a thickness reconstruction model that requires surface mass balance data, not even mentioning what data they used and how the aforementioned uncertainties could impact the results.

I recommend the publication of the manuscript once these issues are addressed.

Thank you very much for your comments and very helpful suggestions—this being the third time now. You are correct that the input data for the thickness reconstruction was not sufficiently described. We have now added detailed information on the input data, including relevant references, to the *Methods* section. Additionally, we have addressed the potential biases that the quality (or lack thereof) of these inputs may introduce to the reconstructed ice thickness field in the discussion and Appendix.

In short: While our reconstruction method relies on SMB input and is thus sensitive to its uncertainties, previous sensitivity tests (e.g., on Svalbard) have shown that the method remains robust when supported by good observational coverage. In our study, the dense GPR survey provides strong constraints, allowing for reliable reconstruction. However, we acknowledge that poorly constrained SMB fields can still introduce significant uncertainty in forward modelling applications.

Particular comments:

Line 11: Showing the mean value for one period and the maximum value for the other doesn't really say anything about the changes in the rate. The maximum values in the first period could be 6 m/y with a mean of 0.34, and this still holds true. This looks handcrafted to sound more dramatic, but it is not rigorous. Please provide mean values for both periods (and if you want maximum values or another metric within parentheses).

You are correct, that this sounds handcrafted. However, these are the mean values for the same area (close to the terminus) for the two periods. Reformulated the sentence to:

"We observe an acceleration in the mean glacier surface elevation lowering rates, with average values in frontal regions increasing from 0.34 m a⁻¹ during 2000–2019 to 5.5 m a⁻¹ during 2019–2024."

Line 22: You use the form "Ice Field" indistinctively, like in this line, or Icefield, like in most other mentions. For consistency, stick to one of the two forms. I strongly recommend using "Icefield."

Thank you for pointing that out. Changed to "icefield" through out the Manuscript.

Line 33: In "aforementioned mass loss of" Consider changing "of" by "with"

Changed "of" to "with".

Line 82: The authors should introduce the reader to the local geography before discussing specific place names such as "Canal de los Témpanos." This can be achieved by referring the reader to figure 1 here or earlier (maybe on line 36)

Good point. Referring to Fig 1b now in L. 86.

Line 107: Please double-check if it is correct to say that the bed becomes shallow. I understand that what becomes shallow is the glacier or the lake. Perhaps rephrase to something like "The contour lines in the central part of the terminus show the position of the bedrock ridge extending from Peninsula Magallanes, dividing the ice flow to the north and east."

Replaced with your suggestion.

Figure 1b: The red "lake level" contour line in the tip of the glacier near Península de Magallanes seems to suggest that about a quarter of the northern margin on that side would be sitting on above-lake-level bedrock. However, even recent pictures show a calving front in that area, which suggests that the ice is grounded below the lake level. It would be worth checking the accuracy of that line.

You are right about that. This comes from the ice thickness reconstruction, which produces too little ice thickness in this particular region and relatively small ice thickness values measured here. We largely attribute this to relatively small ice thickness values measured during the campaign (we

attribute this to the orientation, size and height of the crevasses which distort the radar signal).
Added a paragraph discussing the inaccuracy in that particular region of the glacier.

Figure 1: The gray background would be much clearer to the reader unfamiliar with the area if water areas were highlighted in a different colour. You can use the SRTM water mask for that purpose.

Thank you for the suggestion. Added the SRTM water body mask to all figures throughout the manuscript.

Line 119: Consider changing "expands" to "extends."

Changed.

Line 125: Consider changing "valley" to "branch" for clarity

Changed.

Line 138: "This phenomenon can be attributed to the circulation and stratification of water in the vicinity of freshwater calving glaciers." it is unclear to what phenomenon you are referring to. The sill? How would the sill "be attributed to the circulation and stratification"? Or do you mean "This phenomenon impacts/control/influences the circulation and stratification..."?

The latter, changed to "controls..."

Figure 2b: Just a minor suggestion. It would be informative to see where V1-V4 are in this panel, and it would be easier to read if the distance scale in the x-axis used kilometers instead of meters.

Added V1 to V4 positions to the x axis (incl. dashed lines) to the plot.

Line 173: Mission (singular)

Revised.

Line 181: For "each" year

Revised.

Line 187: I assume this refers to differences over stable terrain. Please say it in the text if that is the case.

That is correct. Added "on stable terrain".

Line 188: It took me a while to understand what "The respective uncertainty of the 2019 to 2023 differencing" means. It was clear only after looking at Table S3. I suggest rephrasing to something like "In the case of the change between 2019 and 2023 using TanDEM-X DEMs, the uncertainty was $\pm 0.35 \text{ m a}^{-1}$ "

Thank you for the advice. Changed to “In the case of the change between 2019 and 2023 using TanDEM-X DEMs, the uncertainty was..”

Lines 193-195: Try to clarify what each number refers to. Saying that towards the terminus, the lowering is up to 5.5 m/a seems to contradict that at the deeper troughs at the terminus, it goes above 6 m/a. I assume that "towards the terminus" does not include the terminus. Or that one refers to point data and the other to a spatial average. Try to reword it less confusingly.

Thank you. Reworded to:

Towards the terminus, the glacier surface lowers at an average rate of up to 5.5 m a⁻¹. However, this surface lowering is not uniform. The highest rates—exceeding 6 m a⁻¹—occur along the deepest glacial troughs near the terminus. In contrast, areas where the ice rests on shallower promontories show lower rates of about 3 m a⁻¹.

Lines 231-233: This affirmation requires supporting evidence to be stated with such confidence. The reduction of effective pressure associated with the thinning might be one factor. However, reduced buttressing stress associated with the separation from the frontal subaquatic moraine or just by the front moving into deeper water could be another factor equal to, or more important. In addition, the reduction of effective pressure would be instantaneous throughout the thinning area. In contrast, the reduction in buttressing would create a dynamic adjustment propagating upstream from the front, just as you describe.

I suggest changing your confident wording that attributes the enhanced flow to this single factor and mention at least debuttressing in addition to thinning induced effective pressure reduction.

Thank you very much for the clarification. We have revised that sentence and avoid overstating the causality now as well as included your feedback! It now reads:

The enhanced glacier flow may result from a reduction in effective pressure at the bed due to thinning; however, other factors, such as decreased buttressing from separation from the subaqueous frontal moraine or the advance into deeper water, could play an equal or even more significant role. While reduced effective pressure would act immediately across the thinning area, changes in buttressing are likely to trigger a dynamic response that propagates upstream from the terminus, consistent with observed flow acceleration patterns.

Line 263: Add a comma between O'Higgins and Viedma

Added.

Line 294: "submerged" is confusing here. Perhaps what you mean is "grounded".

Changed to “grounded”.

Line 301: I suggest changing "in 2022" to "since 2022"

Changed.

Line 303: Instead of "mass," it would be more appropriate to say "thickness" or "overburden pressure."

Changed to "thickness".

Figure 4: "Absolute surface velocity difference between 2022 and 2017 in m" does not make sense. If it is a difference in surface velocity, it should be in m a^{-1} (meters per year). If it is in meters, you probably mean "surface elevation difference." Please correct.

It now reads correctly: 4a&b) "annual surface velocity..." and c) Surface velocity difference (since it is 2002-2017).

Line 307: I assume 100 m "per year." Please include the correct unit. The same is true for the "~50-70 m" on the same line.

Revised.

Line 315: Maybe it is worth adding that a larger frontal area would also enhance calving in addition to the mentioned subaqueous melting

Good point. Added.

Line 368: I struggle to understand the sentence "Ice thickness as a source of error..." Please find a clear way to explain that or expand the explanation with the required details/context.

Rephrased to: Ice thickness is a source of error due to inaccuracies in the GPR-measured thickness itself, as well as errors introduced by uncertainties in horizontal positioning.

Line 370: If the authors used a model to fill the gaps in the GPR data, I think "interpolated" is not the right word. Interpolation usually refers to simple gap-filling techniques such as linear and bilinear interpolations.

You are right! It should read "reconstructed". Revised.

Line 377: Earlier, you provided the depth measured in that area by the seismic profile. It would be very interesting to report the borehole's depth (and derived bedrock elevation) and compare it with the bedrock elevation measured by your GPR survey.

We added the following sentence to the paragraph: The bedrock elevation determined at a borehole ~4 km from today's terminus in 2011 was 380 m below lake level. Our nearest measurements indicate elevations of approximately 350 to 360 m below lake level, providing additional confidence in the overall accuracy of our results.

Line 394: "Contrary" reads awkwardly here. Consider rewording or just removing that word.

Removed "contrary".

Line 410: Here, the authors argue that the uncertainty on the surface mass balance of Perito Moreno Glacier hinders the use of a more physically realistic model. This is a statement I could agree with. However, the ice thickness reconstruction method used by the authors relies on surface mass balance data. The authors need to explain how they reconcile that the SMB data is good enough for ice thickness reconstruction but not for dynamical modelling. Alternatively, just remove that argument and say that a complex model was out of the scope of this work.

Thank you for your comment. You are correct on both points. We do utilize an SMB (Surface Mass Balance) field in the initial step of the ice thickness reconstruction, and consequently, must account for the uncertainties that SMB introduces into the method.

This issue has been addressed in a previous study conducted on Svalbard¹, where the sensitivity of the reconstruction approach was evaluated using different SMB products for the region (1975–2015 MAR-SMB vs. 2003–2013 WRF-SMB). The respective mean SMB values were $-0.08 \text{ m i.e. yr}^{-1}$ for MAR and $-0.3 \text{ m i.e. yr}^{-1}$ for WRF. When applying the method with all available thickness measurements for the region, the reconstructed ice thickness fields were found to be highly consistent, differing in total ice volume by only 2%. The most significant deviations in ice thickness distribution occurred in areas where no observations were available. The test further showed that towards the ice margins—where observational constraints are sparse—and due to the generally higher mean temperatures in the WRF-SMB, ice thickness tended to be lower compared to reconstructions using MAR-SMB. However, in regions with observational coverage, the reconstructed mean ice thickness did not necessarily differ significantly between the two SMB inputs. This indicates that the reconstruction method is robust to SMB uncertainty, provided that a sufficiently dense network of ice thickness observations exists. In our current study, we benefit from a high-resolution GPR survey grid across the entire ablation zone. This data provides the necessary observational constraints to ensure the accuracy of the reconstruction. Nevertheless, it is important to note that uncertainties in SMB remain a limiting factor when using the results for forward modelling of glacier evolution.

Line 411: ¿on? the plateau

Changed to “on”.

Line 458: Fig. S7 (currently missing the "S")

Added.

Line 488: I feel this section needs to go deeper as it relates to the primary dataset used in the study. Saying: "we applied an established state-of-the-art ice thickness reconstruction method" and giving the references is acceptable, but even so, I would expect a few sentences explaining how that method works in a nutshell. What is not acceptable is omitting the source data used for the model, such as surface mass balance data, glacier outlines, surface velocity, and model parameters.

Thank you for the feedback. We have added all relevant information about the input of all data, as well as a short description of the method applied.

Lines 547-549: Remove one of the two "whenever possible"

Removed.

Literature:

1. Fürst, J. J. *et al.* Application of a two-step approach for mapping ice thickness to various glacier types on Svalbard. *The Cryosphere* **11**, 2003–2032 (2017).